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PEACE AND JUSTICE

By A. E. PARR

DIRECTOR, THE AMERICAN MUSEUM OF NATURAL HISTORY

IN OUR private dreams of a better world for our children and our children's children, and in the plans we debate in public discussions, we instinctively start from the premise that a just peace must be the immediate goal of all our efforts and the primary purpose of our victory. We seem to take it for granted that in seeking justice we shall find peace. It apparently never occurs to us that such a search might merely lead into new conflicts, although history teaches us that a demand for justice has always been the battle-cry of wars and rebellions. And anthropology has clearly shown that there is no universally accepted definition of righteousness from which the principles of universal justice might be derived.

Our concepts of right and wrong and therefore also of justice are products of our own traditions, valid and applicable only within the cultural pattern of which they form a part. In our Anglo-Saxon inclination to make justice an end in itself we fail to recognize the existence of these limitations. And in the frustration of our desire for universal principles in a field in which no generalizations are yet possible, we allow our own security to be undermined by our uncertainties, while we actively provoke new threats to our safety through needless conflicts over abstractions which have only been developed to serve as tools for the attainment of the higher ideal of health and happiness for all, within the particular design and conventions of our own society.

Today we are joined with our allies in dedication to a cause which extends its purposes beyond the boundaries of any particular society or civilization to which each of us belongs by his birth and by the traditions in which he has been reared. We have set as our aim the welfare of all humanity, regardless of creed, color, cultural traditions, or geography, because we have learned that only in a world in which such an ideal prevails can we also find complete and secure happiness for ourselves. The success of our efforts may depend upon our ability to recognize that the broadening of our purpose and of our ideals does not immediately, and by itself, remove the limitations of the methods which are valid and useful only against the background of our own traditions and ways of life. It is of supreme importance for us in this crisis of world history not to lose sight of the distinction between ends and means in order that our purpose shall not be defeated by a dogmatic insistence upon the methods and ideas to which we ourselves have become accustomed as members of a fairly homogeneous culture.

Setting as our aim the hallowed goal of education through the ages—*mens sana in corpore sano*—and extending it to embrace all humanity, let us immediately recognize that our only real and final purpose is purely biological, psychological, and spiritual, and that physical, economic, political, and legal measures and principles are only among the practical means of attaining our purpose. Let us further bear in mind that our justicial concepts and procedures are the tools least likely to prove of universal usefulness because they are most likely to be specifically identified with our own traditions alone.

Hunger and poverty feel alike and yield to the same treatment regardless of geography. The state is universally accepted as the

basic unit of international politics, and underneath the ideological differences of motives, purposes, and methods of government the organic structure of the states shows a high degree of similarity in all nations. Education deals with the same general fields of learning everywhere, and the basic processes by which knowledge is acquired are the same for all who seek it. A religious craving is common to all humanity and therefore capable of being understood and respected by all, even though the faiths in which it finds expression differ among the peoples. But the terrible war we are now fighting is renewed evidence that humanity has not yet reached a common ground in its judgments of right and wrong, and until it does we shall have no solid foundation on which to base a world-wide system of justice as a primary instrument for the improvement of human welfare.

If, in these circumstances, we try to make justice as we see it in our own tradition the immediate aim of our efforts, we shall find ourselves defeated by our own principles. In passing judgments upon the acts of man within our own society we have long recognized the moral significance of the motives behind the deed. If we attempt to deal in the same manner with acts committed in traditions entirely alien to our own way of thinking, we shall also be forced by our own convictions to give consideration to motivations entirely beyond our capacity to comprehend. And we shall become bewildered in our own judgments. We also accept the innocence of the insane, and evidence will accumulate to make us wonder about our right to condemn. We shall be lost in a wilderness of circumstances which our own experience does not enable us to understand and evaluate, and we may end by losing our cause because we attempt to reach our goal through an avenue of approach in which our habitual ways of reasoning do not apply.

But if we remember that justice is only one among many means to a higher end, its failure to serve in the particular state of the world today will not mean the failure of our aspirations, and we shall still be able to turn with equanimity to other methods of achieving the improvement of the human lot which is our actual goal. If we find it necessary, as we almost certainly shall, to make increasing allowances for debatable motivations, and to abide by the principle that even temporary insanity is not accountable before the bars of justice, we will also remember that neither do we let the criminally insane roam free to wreak their destruction upon our society. Nor will we forget that the purpose of the measures we must take is not suppression, but cure, in order that the principles and methods of justice which are bound to fail in application today may ultimately prevail throughout the world, as the most civilized means of attaining the greatest common good among all people.

The world is facing a titanic problem in bodily and mental health. It must be treated by methods immediately and universally applicable to the basic causes and realities of the situation. These methods are those practiced in medicine, psychology, psychiatry, public health, sociology, anthropology, engineering, economics, and statecraft. The methods of justice still face a long process of regional evolution within culture areas in which a kinship of ideas already exists, before these areas can ultimately be absorbed in a world-wide fusion of principles and aspirations common to all mankind. Any attempt to force this evolution by a premature imposition of our own particular concepts of abstract justice is bound to hinder rather than advance the progress of civilization of which we dream.

It is inherent in our psychology, and in the entire situation we are facing, that a peace formulated and proclaimed in the name of justice must begin as a hard peace, inspired by the wrath of our righteous indignation, only to end in the softness of doubts and uncertainties concerning the definition and application of the principles on which it was based. In the ultimate outcome it will therefore neither give satisfaction to the victims of aggression nor will it provide a cure for the spirit of the aggressors. History will remain free to repeat itself again until we finally learn to keep our main purpose unobscured by spiritual commitments on secondary questions as to the practical methods which, at any stage, may best serve the development of a better world.

THE ART AND SCIENCE OF CROP FEEDING

By CHARLES J. BRAND and H. R. SMALLEY

THE NATIONAL FERTILIZER ASSOCIATION

THE total output of American farms in 1944 was 33 per cent above the 1935–39 average and 45 per cent above the 1910–14 average. This record production means that there has been plenty of food for the men in the armed services, for our Allies, and for a civilian consumption 7 per cent larger than in the prewar years.

There are actually fewer workers on farms than there were five years ago, 17 per cent fewer than the 1910–14 average. While millions of farm workers have gone into industry and to war, those remaining on the farms have worked longer hours, and wives and daughters have driven tractors and have done a thousand and one other jobs. Older men have come out of retirement to help; many office workers and teen-age youngsters have helped in many localities, as have war prisoners, soldiers, and laborers from Mexico and the West Indies. Moreover, the industries that provide farmers with machinery and equipment, fertilizer, liming materials, insecticides, feed, and seed have made an absolutely essential contribution.

In establishing this new all-time record of production during the war, farmers have demonstrated the enormous value of the scientific research that has been done by our State agricultural experiment stations, by the U. S. Department of Agriculture, and by private agencies in many fields. It is research that made possible the vertical expansion of agriculture, more produce per acre. The alternative is horizontal expansion, which means more acres, more manpower, and more horse or machine power.

Every branch of agricultural science has made a significant contribution to the expanded production program. The improvement of the soils; the use of fertilizers, lime, and manure; the growing of legumes and cover crops; and erosion control could not be fully effective in raising crop yields without the work of the plant breeder, the pathologist, and the entomologist. The plant breeder has given us high-yielding, often disease-resistant, varieties that can make use of increased soil fertility. Hybrid corn is an outstanding example of the breeder's work, but there are literally hundreds of others. And along with the improvement in soils and varieties, there must in many cases be developed a spraying or dusting program to control insects and plant diseases.

On the animal-production side, science has made equally effective contributions. Among the improvements in this field are breeding for higher production of milk and eggs; for more efficient meat production; and better methods of feeding animals for all purposes.

USE OF COMMERCIAL PLANT FOOD A FACTOR IN DETERMINING YIELD

Acreage yields of corn, wheat, and cotton were relatively constant for 50 or more years prior to 1937. Almost uniformly during that period increased production was the result of increase in the number of acres harvested. In contrast, since 1937 the factor of yield per acre has become ascendant, especially as to corn and cotton. Wheat also has shown an uptrend in yields per acre. These crops have consumed nearly 45 per cent of total commercial plant food used in recent years.

In the 17-year period from 1916 to 1932, we harvested on the average 102,000,000 acres of corn, producing an annual average of 2,646,000,000 bushels, with an average yield of 25.94 bushels per acre. In the 7-year period from 1937 to 1943, inclusive, we harvested 90,700,000 acres on the average, with an average production of 2,731,000,000 bushels, and an average yield per acre of 30.10 bushels for the period. In other words, with acreage harvested reduced by approximately 12,000,000 acres between the two periods, we nevertheless harvested an average of 85,000,000 bushels more on the smaller acreage.

The following figures show what has happened with respect to the fertilization of corn

acreage, comparing 1929 with two recent years:

ACREAGE OF CORN FERTILIZED

In 1929	*In 1938*	*In 1942*
14,786,000	18,725,000	21,543,000

Not only did we have a direct increase of approximately 6,750,000 acres of corn fertilized, but we had a distinct increase from 1929 to 1942 of from 17.5 per cent to almost 21.0 per cent in the plant-food content of the fertilizers used.

Wheat ranks third, with corn and cotton first and second in acreage fertilized. Winter wheat is customarily responsible for about two-thirds of our total wheat production, and 12 of the Midwestern States ordinarily produce about 60 per cent of the winter-wheat crop. The 5-year average annual use of fertilizer in these States for 1930–1934 was 618,636 tons. Consumption in them in 1943 was 1,773,657 tons. The 5-year average annual yield of winter wheat in this group of States from 1930 to 1934 was 14.6 bushels per acre; for 1938 to 1942, inclusive, it was 16.4 bushels; in 1943 the average yield in the Midwest was 15.6; in 1944 it was 18.8.

An increase of more than 1,155,000 in tons of fertilizer used was not without considerable influence in maintaining and increasing wheat yields. One ton of average fertilizer produces an average increase in yield of 85 bushels of wheat.

A brief comparison of wheat yields of two groups of nations is given below. Those in the first list use fertilizers with reasonable adequacy, and those in the second do not. Rainfall and other factors, of course, account for some of the difference.

AVERAGE YIELDS OF WHEAT FOR 1930–1934

Country	*Bushels per acre*
Sweden	33.3
United Kingdom	33.3
Germany	32.1
Austria	23.6
France	23.0
Spain	14.1
Argentina	13.8
Canada	13.6
United States	13.5
Australia	12.2

Fertilization of Cotton. Only for cotton do we have relatively detailed and accurate estimates. They have been made yearly since 1922 by the U. S. Department of Agriculture. A general indication of what has happened in cotton growing since 1925 is given below:

COTTON FERTILIZATION AND YIELDS

Period	Av. acres harvested per year	Av. number of bales per year	Per cent acres fertilized	Per cent of 1925–29 average	Av. yield per acre (pounds)	Per cent of 1925–29 average
1925–29	42,600,000	15,268,200	35.4	100.0	179.2	100.0
1934–39	34,658,000	12,563,000	35.6	100.6	181.2	101.1
1938–42	23,219,000	11,977,400	43.4	122.6	272.4	152.0
1944	20,098,000	12,359,000	46.1	130.2	295.3	164.8

	1929	*1938*	*1944*
Cotton acreage in cultivation as of July 1	44,448,000	25,018,000	20,472,000
Cotton acreage fertilized	16,811,000	10,294,000	9,435,000
Per cent fertilized	37.8	40.4	46.1
Pounds of fertilizer applied per acre	265.4	282.4	328.0
Estimated plant-food content of fertilizer used	14 to 15%	16.0%	18.0%
Average yield of lint per acre	164.2	235.8	295.3

Increase in the average yield of cotton between 1929 and 1944 is due among other things to (1) an increase in the proportion of acres of higher fertility kept in cotton as the enormous cut in acreage took place; (2) an increase of nearly 10 per cent in the number of acres receiving fertilizer; (3) an increase of from 265.4 pounds applied in 1929 to 328 pounds in 1944; and (4) an increase of possibly 20 per cent in the plant-food content of the fertilizer used on cotton.

Other factors that should not be overlooked are more favorable weather, more general planting of higher-yielding strains and varieties, better placement of fertilizer in the soil (not in contact with the seed, which is injurious), and better cultivation, which improves the condition of the soil and helps to control weeds and to conserve moisture. In the longer view of the past, increased mechanization of all farming has also had a very great effect.

Recently Dr. Ralph W. Cummings, of the North Carolina Agricultural Experiment Station, stated that commercial plant food probably accounted for more than 50 per cent of the total cotton production obtained in his State in 1943. A crop of 500 pounds of lint with its accompanying 750 pounds of seed removes from the land about 27 pounds of nitrogen, 12.5 pounds of phosphoric acid, and 12.5 pounds of potash.

Correct fertilization improves both yield and quality of crops. Years of experimentation and statistical studies warrant the conclusion that a ton of the average grade customarily used on corn produces a yield increase of 125 bushels; on cotton, 2 bales of 500 pounds each; and on wheat, 85 bushels.

With the foregoing background of practical agronomic and economic facts, we proceed to sketch briefly the history of the art and science of crop feeding, the development of the modern chemical fertilizer industry, and its present status.

History of the Development of Crop Feeding

The use of animal manures as a means of enriching the land dates back to the dawn of human history. The agricultural writers of ancient Greece and Rome discussed the relative merits of various kinds of manures and composts made from manures and crop residues. Nearly 1900 years ago, Pliny the Elder, in his *Historia Naturalis,* discussed the value of green manures, stating that "it is universally agreed by all writers that there is nothing more beneficial than to turn up a crop of Lupines, before they have podded, with either the plough or the fork, or else to cut them and bury them in heaps at the roots of trees and vines. It is thought, also, that in places where no cattle are kept, it is advantageous to manure the earth with stubble or even fern." Thus the value of turning under legumes, as a means of increasing soil fertility, was known in the first century A.D., although the fundamental reason for this value was not discovered until nearly the end of the nineteenth century, when two German investigators, Hellriegel and Wilfarth, proved that the bacteria which live in the nodules on the roots of legumes are able to fix atmospheric nitrogen.

It is also apparent from the writings of Pliny and others that the use of marl and other forms of lime would improve many soils. He discussed the merits of different forms of marl and stated that it was used chiefly in the Gallic provinces and in the British Isles.

The science of plant feeding in any modern sense—knowledge of the soil and of the chemical elements essential to plant growth—had its real beginning only about 100 years ago, and it was also just about a century ago that the manufacture of chemical fertilizer was begun in England.

If we go back a little over 300 years we find van Helmont, a Belgian physician and scientist, growing a tree in an earthen pot. He weighed it at the start and after five years found that it had gained 164 pounds. The soil weighed practically the same as at the beginning, so he concluded that the increased weight must have come from water and that water must be the true plant food.

In England more than 200 years ago Jethro Tull (1674–1741), a landowner and a graduate of Oxford, published (1733) his *Horse Hoeing Husbandry.* He proposed that *earth* was the true food of plants; that by

better tilling, the particles of soil could be made fine enough to be taken up by plant roots; that tilling could be used as a substitute for manure.

During the next 100 years (1740–1840) great progress was made in chemistry, so essential to an understanding of plant growth. The French chemist Lavoisier, during the latter half of the eighteenth century, explained the true nature of combustion; that it is an oxidation process and that the end product is carbon dioxide.

Henry Cavendish (1731–1810), a famous English chemist and physicist, was the first to determine the comparative weight and quantity of gases produced by the decomposition of plant and animal substances.

Joseph Priestley (1733–1804) made his contribution by discovering oxygen and also by carrying on many experiments dealing with plant and animal nutrition. Jan Ingen-Housz (1730–1799), a Dutch physician, proved that the absorption of carbon dioxide and the giving off of oxygen are plant functions that can take place only in the presence of light.

Jean Senebier (1742–1809), a Swiss clergyman and a scientist, was the first to explain the process which we now know as photosynthesis—the formation of carbohydrates in the plant from carbon dioxide and water with the aid of sunlight.

Theodore de Saussure (1767–1845) was, according to Dr. C. A. Browne, "attracted to chemistry by the work of Lavoisier, and his special interest in experimental plant chemistry was stimulated by the work of Priestley, IngenHousz, and Senebier." Dr. Browne says: "It was Lavoisier who chiefly helped to provide the foundation stones of modern agricultural chemistry, but it was Theodore de Saussure who put them in place." And Sir John Russell says: "To him is due the quantitative experimental method which more than anything else has made modern agricultural chemistry possible; which formed the basis of subsequent work by Boussingault, Liebig, Lawes, and Gilbert."

Carl S. Sprengel (1787–1859), a native of Hanover, Germany, made a larger contribution to the science of plant feeding than is usually accredited to him. Browne says of him: "It is strange that his name should have been so completely overlooked by later writers, although he was the first to announce many of the ideas that have been wrongly accredited to Liebig and other later investigators." His list of 15 chemical elements which he considered to be essential to crop growth includes 11 of those which are now regarded as necessary. The 11 elements were: oxygen, carbon, hydrogen, nitrogen, sulphur, phosphorus, potassium, calcium, magnesium, iron, and manganese. We now add only copper, zinc, and boron to this list. He pointed out that if only one of the elements necessary for growth be lacking the plant will not thrive, even though all the others occur in abundance—a statement which is as true today as it was then.

The first scientist to conduct actual field experiments was Jean Baptiste Boussingault (1802–1887). He began his experiments on his farm in Alsace about 1834 and described them in a series of articles published in 1836 and succeeding years. In discussing his work Dr. C. A. Browne says that "until the third decade of the 19th century, agricultural chemists had occupied themselves chiefly with the problems of chemical composition and very little with the problems of mutual chemical relations. Much knowledge had been accumulated concerning the elementary composition of agricultural products, but no one had yet attempted to follow the balance of chemical elements in the growing of a crop or in the feeding of an animal. He first made agricultural chemistry a true experimental science by transferring it from the laboratory to the field and stable." To him, says Sir John Russell, "belongs the honor of having produced the method by which the new agricultural science was to be developed. He reintroduced the quantitative method of de Saussure, weighed and analyzed the manures used and the crops obtained, and at the end of the rotation drew up a balance sheet showing how far the manures had satisfied the needs of the crops, and how far other sources of supply—air, rain, and soil—had been drawn upon. . . . Boussingault's work covered the whole range of agriculture and deals with the composition

of crops at different stages of their growth, with soils, and with the problems of animal nutrition. Unfortunately the classic farm of Bechelbronn did not remain a center of agricultural research and the experiments came to an end."

The year 1840 has great significance for the student of agricultural chemistry, particularly to the student of plant nutrition, and also to the manufacturer of chemical fertilizer. It was in 1840 that Justus von Liebig, the famous German chemist, was invited to report to the British Association for the Advancement of Science on the state of organic chemistry. This report was amplified and published the same year as *Chemistry in its Application to Agriculture and Physiology*. Sir John Russell states that "this report came like a thunderbolt upon the world of science. With polished inventive and a fine sarcasm he (Liebig) holds up to scorn the plant physiologists of his day for their continued adhesion, in spite of accumulated evidence, to the view that plants derive their carbon from the soil and not from the carbonic acid of the air. . . . Liebig's ridicule did what neither de Saussure's nor Boussingault's logic had done. It finally killed the humus theory (the theory that plants derived their carbon from the organic content of the soil)."

After taking his degrees at Bonn and Erlangen, Liebig through the sponsorship of von Humboldt was enabled to spend two years in Paris with Gay-Lussac. In 1824 he became professor of chemistry at the University of Giessen. There he became famous as a teacher and investigator in organic chemistry, serving until 1852, when he went to Munich. His teaching and personality attracted pupils from all parts of the world.

His great ability as analyst, teacher, and editor enabled Liebig to include in his book a review of existing knowledge which attracted immediate attention. By 1848 the book had gone through 17 editions and translations and was constantly revised by the author. It contained over 200 references to the work of approximately 100 different authors but did not report much in the way of original research. Liebig stated that the hydrogen and oxygen used by plants came from water; the nitrogen from ammonia; that certain mineral substances were essential; alkalies were needed for neutralizing the acids developed within the plant; phosphates were necessary for seed formation, and potassium silicates for development of grasses and cereals. He stated that the fertility of the soil could be maintained by returning, in the form of manure or chemicals, the mineral constituents and the nitrogen removed in the crops; that when sufficient crop analyses had been made it would be possible to tell the farmer exactly what he should add in any particular case. (Present-day agronomists would be happy if that last suggestion of Liebig's were really true.)

Perhaps the greatest contribution made by Liebig was the stimulus which the publication of his book gave to other research workers in the field of plant feeding. Even the controversies which he started bore valuable fruit in later years.

One of those who listened most attentively to Liebig's report in 1840 was a young Englishman, Sir John Bennet Lawes, who even then had begun his famous experiments at his country estate at Rothamsted. Fittingly, Gay-Lussac's student Liebig had treated bones with sulphuric acid in order to make the phosphate more available to plants, but Lawes figured that, since the supply of bones was limited, natural phosphates might be so treated with equally good results. He experimented, and in 1842 patented his process for the manufacture of superphosphate. For the next 30 years he engaged in the manufacture of superphosphate on a large scale and spent a great deal of his wealth in establishing the Rothamsted Experimental Station, the forerunner of all the agricultural experiment stations that we have in the world today. Thus the science of plant feeding and its close ally—the manufacture of chemical plant foods—really began to develop in a modern sense during the years 1840 and 1842.

Experiment Stations and Other Agencies Enter the Field

The establishment of the now famous Rothamsted Experimental Station at Harpenden, England, in 1843 is customarily re-

garded as the beginning of the modern period in soil and plant research, particularly as it relates to the maintenance of and increase in crop yields through crop rotation, the use of manures and fertilizers, and other soil management practices. Speaking of this early period, Sir John Russell says that "farmers were slow to believe that 'chemical manures' could ever do more than stimulate the crop, and declared they must ultimately exhaust the ground. The Rothamsted plots falsified this prediction; manured year after year with the same substances and sown always with the same crops, they even now . . . continue to produce good crops . . ."

Interest in soil improvement and in plant nutrition developed rather more slowly in the United States than in Europe, largely because our land was newer and there was still plenty of land that could be had almost for the asking. Many of the leading farmers belonged to agricultural societies and were thus able to keep in touch with the development of new methods and practices. The value of liming the soil was fairly well known prior to 1840, due largely to the experiments and the writings of Edmund Ruffin of Virginia. His essay on *Calcareous Manures*, first published in 1821, was amplified and published in book form, the fifth edition appearing in 1852.

Two editions of Liebig's book were published in this country before 1848 and did much to arouse interest in soil chemistry. Also a considerable number of students who went abroad to study in European universities were coming in contact with the work of Liebig and other scientists on the Continent and with the work of Lawes and Gilbert at Rothamsted. Returning, many of these students became leaders in agricultural research in this country. Space limits forbid details, but some experiments with fertilizers were conducted prior to 1860. Experiments were conducted in Pennsylvania in 1857, in Maryland in 1858, in Michigan in 1863, in New Jersey in 1865, in Massachusetts in 1867, in Maine in 1868, in Illinois in 1871, and in Kansas in 1872.

Passage of the Morrill Act by Congress in 1862 made possible the eventual establishment of an agricultural college in each of the 48 States and provided a strong stimulus to agricultural education and research. During the next 25 years experiment stations were established in a number of States without Federal aid, and a number of experiments with fertilizers were conducted. The best known of these are the Morrow Plots, established at the University of Illinois in 1876, and the Jordan Plots at Pennsylvania State College, started in 1881. These are the oldest permanent experimental plots in this country, and they are still being continued.

In 1887 the Hatch Act was passed by Congress providing for the establishment of an agricultural experiment station in each State. This law established the principle of matching Federal with State funds and has made possible the building of an organization for agricultural research in this country which is not equaled by that of any other country.

After 1887 experimental work in the field of plant nutrition grew rapidly. Experiments involving the use of fertilizers, liming materials, manures, and crop rotations were started in most of the eastern, southern, and middle-western States. This work was supplemented by many experiments conducted in the greenhouse and by a vast amount of chemical analysis of soils and crops. For a long time agricultural chemists believed, with Liebig, that by analyzing the soil and the crop they should be able to tell the farmer exactly what fertilizer he should use. But field experiments indicated clearly that chemical analysis alone was not a practical solution to the problem. It was shown that although a soil may contain a relatively large quantity of total phosphorus or potassium only a small amount might be available during any crop year. The chemists then tried to devise means of determining the *available* plant-food content of the soils, and work along this line has continued to the present time. So-called "quick methods" of analyzing, or testing, soils for their available plant-food content are now in use in nearly every State.

Progress has also been made in plant analysis, and many methods are now avail-

able which indicate the nutrient status of plants at any stage of growth. Often the use of these methods makes it possible for the grower to make supplementary applications of fertilizer to correct a specific deficiency.

During the past 25 years agronomists, horticulturists, plant physiologists, and plant pathologists have made an intensive study of symptoms that develop in plants as a result of deficiencies of the various essential elements. Many of these symptoms are accurately described and illustrated in *Hunger Signs in Crops*, published by the American Society of Agronomy and The National Fertilizer Association. It is not yet possible for the agricultural scientist or the extension teacher to tell a farmer precisely what fertilizers will give the best results on a particullar field and crop, but it is possible to make very intelligible recommendations, far more accurate ones, in fact, than could have been made even 10 years ago. To make a good recommendation it is necessary to know the characteristics of the soil, its previous treatment, its degree of acidity or alkalinity, and its available content of the more essential mineral elements, particularly phosphorus and potassium. The results of experiments on the same soil type are always helpful, as are, of course, visible symptoms of the plant-food deficiencies. Plant-food analyses and tests are often employed when diagnosis is difficult.

The Modern Fertilizer Industry Develops

We have stated above that the manufacture of commercial fertilizers in a modern sense really began in 1842, when Sir John Lawes patented his process for the manufacture of superphosphate. The art of crop feeding could not develop until science had pointed the way. Up to that time farmers had depended upon animal manures supplemented, to some extent, by lime. Animal bones had also been used to some extent. In 1824 two barrels of Peruvian guano arrived in Baltimore for someone to try as a fertilizer, and in 1832 the first commercial importation of Peruvian guano was made. In 1830 the first Chilean nitrate arrived in Norfolk, Va. In 1840 the first by-product ammonia salts were produced in England, and sulphate of ammonia was sold as a fertilizer in England from that time on, but it was not produced in this country until 50 years later. In 1845 Liebig demonstrated the value of potash as a fertilizer, but its actual use dates from about 1860.

The first mixed fertilizers were made in Baltimore about 1850, and by 1855 "manipulated guanos" were being sold up and down the Atlantic coast. These fertilizers contained only nitrogen and phosphoric acid. As late as 1868 only about 50,000 tons of mixed fertilizers were used in the United States.

Peruvian and other guanos were the principal fertilizers used from 1840 to 1870. Guano was a good fertilizer, but it varied greatly in composition. The supplies were rapidly depleted, although small quantities have been imported all through the years, and still are.

The American fertilizer industry really began to develop in earnest when the value of the South Carolina phosphate deposits was recognized in 1867, and the production of superphosphate on a large scale was thus made possible.

Potash salts were first imported from Germany in 1869 or 1870, and from 1870 until the World War began in 1914 manufacture of fertilizer and its use on American farms developed rapidly.

Animal by-products and cottonseed meal became available for use in fertilizers in increasing volume, potash salts were imported from Germany and nitrate of soda from Chile, and the Florida and Tennessee phosphate deposits were discovered and worked on a large scale. Superphosphate production increased from 31,000 tons in 1868 to more than 3,000,000 tons in 1913.

Until 1914 the nitrogen in mixed fertilizers was derived mainly from animal by-products, cottonseed meal, and Chilean nitrate. Sulphate of ammonia production had been increasing gradually since 1893, and cyanamide produced at Niagara Falls, Canada, came on the fertilizer market about 1910. All our fertilizer potash, prior to

World War I, came from Germany. The American potash industry was not yet born.

The year 1914 marked the beginning of vast changes in fertilizer manufacture, with the result that fertilizers became better and relatively cheaper.

As the use of fertilizers developed, the industry grew until at present there are about 900 plants located in consuming areas. The table below shows that the total consumption of mixed fertilizers and of all fertilizer materials has increased from 1,150,000 tons in 1880 to about 12,000,000 tons in 1944.

CONSUMPTION OF FERTILIZER IN THE UNITED STATES

Mixed Fertilizer and Materials

Year	*Tons*	*Year*	*Tons*
1880	1,150,000	1925	7,334,000
1890	1,950,000	1930	8,222,000
1900	2,200,000	1935	6,276,000
1910	5,453,000	1940	8,303,000
1920	7,177,000	1944	12,000,000*

* Preliminary estimate.

About 70 per cent of the fertilizer used in the United States is applied in the form of mixed fertilizers, the rest as single materials. Most mixed fertilizers supply nitrogen, phosphorus, and potassium, as these are the three plant foods most frequently needed on the great majority of soils, but in some areas and for some crops other elements are needed, for example, magnesium, boron, manganese, zinc, copper, and iron, and these are often included in fertilizer mixtures. Large quantities of calcium and sulphur are supplied in most mixed fertilizers and in several of the fertilizer materials. These elements are essential to plant growth, but deficiencies are not commonly observed in the field because of the large quantities present in most soils and applied in fertilizers.

Nitrogen. Nitrogen is an essential constituent of powder and other explosives, a necessary constituent of animal and plant proteins, and, of course, of animal feeds. It is also one of the three major plant foods. Thus, as powder, protein, or plant food, nitrogen plays a big part both in war and in peace.

World War I created an enormous demand for nitrates, both for use in making explosives and for fertilizers. Other countries could still get Chilean nitrate, but Germany could not, so she developed a great air-nitrogen industry. Following the war, our air-nitrogen industry developed rapidly, but by-product sulphate of ammonia continued to be the principal source of nitrogen used in mixed fertilizer, and we are still importing large quantities of Chilean nitrates and, to a less extent, cyanamide and, recently, ammonium nitrate from the war nitrogen plants of Canada. Synthetic nitrate of soda produced by our domestic air-nitrogen industry has been on the market for some 15 years. It is produced in substantial quantities. Nitrogen solutions, containing ammonia, ammonium nitrate, and urea, are important constituents of mixed fertilizers.

A tremendous expansion in the production of nitrogen from the air was planned at the very beginning of the defense program in order to insure ample nitrogen supplies for munitions, for industry, and for agriculture. Despite competitive demands, agriculture has fared well, as indicated by the figures in the following table. Measured by the tremendously expanded demand, there was some shortage of nitrogen in 1942 and 1943, and there is a slight shortage at present, due to the exceedingly heavy demand for explosives.

CONSUMPTION OF NITROGEN IN THE UNITED STATES

(Short Tons)

1880	18,800	1930	375,800
1890	45,000	1935	306,100
1900	71,800	1940	413,100
1910	145,400	1944	600,000
1920	227,800		

About half of the nitrogen used as fertilizers is used in mixtures and about half of it is used alone as side and top dressings. Prior to World War I large quantities of by-product organic materials, such as cottonseed meal and animal tankages, were used as fertilizer, but these materials are now used almost entirely in animal feeds. Fertilizer

nitrogen now comes mainly from chemical sources, such as ammonium sulphate, nitrate of soda, ammonia solutions, ammonium nitrate, cyanamide, and urea.

Phosphoric Acid. Available phosphoric acid used in fertilizers is obtained almost exclusively from superphosphate. Bone meal, once important as a fertilizer, is now used almost entirely in animal feeds. Some ammonium phosphates and basic slag are used, but the total quantities are negligible when compared with superphosphate. Superphosphate is now produced in about 170 plants, which are located in regions where consumption is largest. For example, the Carolinas, Georgia, Florida, and Alabama use about half of the fertilizer and have about half of the superphosphate plants. The raw materials used in the production of superphosphate are phosphate rock and sulphuric acid. Many plants are equipped to make their own sulphuric acid; hence, their raw materials are phosphate rock and sulphur.

Superphosphate has been produced in this country since about 1850. It was first made from spent bone black, later from rock from West Indian phosphate deposits. When the South Carolina deposits were discovered in 1867 production began to increase rapidly. In 1920, annual production for the first time exceeded 5,000,000 tons. It did not again reach that level until 1941. Plant capacity has always exceeded production and demand, and the big increase in production during the past few years was accomplished with little new plant construction.

Our reserves of phosphate rock, which are located mainly in Florida, Tennessee, and the intermountain States of Montana, Idaho, Wyoming, and Utah, are sufficient to last from 2,000 to 5,000 years. In addition, there are some 2,000,000 acres yet to be examined in the intermountain States, and geologists believe that additional phosphate deposits may be found.

The production of superphosphate since 1880 is shown in the accompanying table.

Potash. The cutting off of our potash supply from Germany at the outbreak of World War I intensified the search for potash in this country by Government agencies, private firms, and individuals. The development of our potash industry from a small beginning in 1915 to our present annual production of more than 800,000 tons of actual potash (K_2O) is a most interesting story and a record of which American private industry can well be proud. As recently as 1938, 40 per cent of our potash was still coming from Europe, and although our imports are now practically nil, both American farmers and American industry are using potash at the highest rate ever attained. The consumption of potash as fertilizer is shown below:

PRODUCTION OF SUPERPHOSPHATE IN THE UNITED STATES

(Basis 18 per cent P_2O_5)

1880	205,000	1925	4,040,000
1890	480,000	1930	4,415,000
1900	1,505,000	1935	2,950,000
1910	2,595,000	1940	4,865,000
1920	5,130,000	1944	7,000,000*

* Preliminary estimate.

POTASH CONSUMPTION IN AGRICULTURE, 1880–1944, IN CONTINENTAL UNITED STATES*

Tons actual potash (K_2O)

Year	*Imported*	*Domestic*	*Total*
1880	20,000	....	20,000
1890	30,000	....	30,000
1900	60,000	....	60,000
1910	270,000	....	270,000
1920	170,000	40,000	210,000
1925	250,000	25,000	275,000
1930	305,000	45,000	350,000
1935	165,000	135,000	300,000
1940	70,000	340,000	410,000
1944	....	610,000	610,000

* Data supplied by the American Potash Institute, Inc.

CONCLUSION

In 1798 Thomas Malthus, the English sociologist, promulgated his theory that poverty and distress are unavoidable, since population increases by geometric ratio and the means of subsistence by arithmetic ratio; that therefore war, famine, and disease are necessary as checks on population increase.

A century later (1898) Sir William Crookes, in his presidential address before the British Association for the Advancement of Science, startled the world by calling attention to the seriousness of the wheat problem. The acreage of wheatland could not be increased very much, he said, except at the expense of other crops also needed, and the alternative was higher yields per acre. This could easily be accomplished by the use of nitrogen fertilizers, but the exhaustion of the natural deposits of Chilean nitrate was in sight and supplies of nitrogen from other sources were not sufficient. Crookes was a great scientist and was thinking far in advance of his time. What he was really trying to show was that some means must be found for fixing atmospheric nitrogen; in fact, he had shown how this could be done by the electric-arc process, the first process used for fixing atmospheric nitrogen and long since discarded because of its high cost. Nevertheless, Crookes stimulated research and pointed the way to a great development.

Since 1898 the whole nitrogen picture has changed completely. We now have within relatively easy reach an unlimited supply of air nitrogen to supplement our by-product production and our importations of Chilean nitrate—not to mention our enormous soil reserves of nitrogen, or the nitrogen fixed by the bacteria in the nodules that grow on the roots of leguminous plants and by other soil organisms, or the large revolving supply in manures and crop residues, or even that which is brought down in rain. Our known reserves of phosphate rock are sufficient to last at least 2,000 years, and we have enough potash in sight to last at least 100 years, with the probability that much more will be found.

Our farmers, aided by science and extension teaching, are now able to utilize commercial fertilizers efficiently, and we estimate conservatively that 20 per cent of our present agricultural output can be credited to fertilizer use. This percentage varies greatly from State to State, also for the various crops. We have been able to meet the greatly increased demand for food, feed, fiber, and raw materials by expanding our agriculture *vertically;* that is, by increasing yield per acre, rather than by horizontal expansion, which means more acres and an increase in manpower, horsepower, and machine power, which we did not have. We estimate that fertilizer use has obviated putting into cultivation at least 50,000,000 acres of additional cropland.

Fertilizer is now being used on about 70,000,000 acres, one-fifth of the total harvested acreage. There are approximately 175,000,000 acres of other cropland on which fertilizer could be used to advantage, and besides that there are 175,000,000 acres of pastureland which could be improved through fertilization as the need develops for increased livestock production. Thus there is ample opportunity for expanding our agriculture on present acreage to meet our needs for a long time to come.

THE LEGEND OF CINCHONA

By GEORGE URDANG

DIRECTOR, AMERICAN INSTITUTE OF THE HISTORY OF PHARMACY

AMONG the many blessings which the American continent presented to the people of the Old World, the bark of that group of evergreen trees called *Cinchona* has certainly been one of the most wonderful.

Everybody knows that this bark and its alkaloids, especially quinine, first isolated by the French pharmacists Pelletier and Caventou, have proved to be still irreplaceable for the treatment of intermittent fevers, of ague and malaria, and that the white man's settlement and warfare in large areas of the world would have been impossible without these drugs. The question arises and has, indeed, been asked throughout the centuries: how came the people of the Old World to know of and learn to use the miraculous drug?

The beginnings of the use of *Cinchona* bark as an antifebrile are still clouded in mystery. There is no evidence that the natives in Peru knew of the unique qualities of its contents and employed it as a drug before the arrival of the Spaniards. Furthermore, there was much confusion as to the real fever bark. For centuries the bark of the Peruvian balsam tree was used illicitly, as well as in good faith, instead of that of *Cinchona*, and the nomenclature was uncertain. Flückiger and Hanbury stated in their excellent *Pharmacographia* (second edition, London, 1879) the following:

> Humboldt [who visited the areas concerned in 1802] declares that at Loxa the natives would rather die than have recourse to what they consider so dangerous a remedy. Pöppig (1830) found a strong prejudice to prevail among the people of Huanuco against *Cinchona* as a remedy for fevers, and the same fact was observed farther north by Spruce in 1861. The latter traveller narrates that it was impossible to convince the *cascarilleros* of Ecuador that their Red Bark could be wanted for any other purpose than dyeing cloth; and that even at Guayaquil there was a general dislike to the use of quinine.
>
> Markham [1862] notices the curious fact that the wallets of the native itinerant doctors, who from father to son have plied their art since the days of the Incas, never contain *cinchona* bark.
>
> Although Peru was discovered in 1513 and submitted to the Spanish yoke by the middle of the century, no mention has been found of the febrifuge bark with which the name of the country is connected ["Peruvian bark"], earlier than the commencement of the seventeenth century.

There is a growing tendency to assume that malaria was not known in the South Americas before the arrival of the Spaniards, but was one of the diseases imported by the conquerors into the new country. This theory explains the lack of knowledge of the antifebrile effect of the *Cinchona* or Peruvian bark on the part of the Indians. However, if it was not them, who then did find out what nature had in store for suffering mankind in these wrinkled brown external coverings of a group of tropical trees?

It is known that the Spaniards learned from the Indians very early about the medicinal virtues of the balsam obtained from the Peruvian balsam tree, and that the Spanish Jesuits in their systematic endeavor to find some drug for the treatment of intermittent fevers subjected the bark of this tree, the bitter taste of which intimated some effective constituent to what would be called today clinical experiments. Achieving some, although limited, results, was it not logical to investigate other bitter barks as to similar, if not even better, effects, and that in this way finally the *Cinchona* bark and its miraculous qualities were discovered? After all, the prominent part played by the Jesuits in the early distribution and advocacy of *Cinchona* bark as a remedy for intermittent fever belongs to the few facts concerning the early history of *Cinchona* that are definitely assured.

The first mention of the genuine *Cinchona* bark by a European writer is contained in the *Cronica Moralizada del Orden de San Augustin en el Perú* of the Augustinian monk Antonio de la Calancha, bearing an ecclesiastical imprimatur dated 1633 at Lima. In this booklet Calancha writes:

> A tree grows which they call "the fever tree" in

the country of Loxa, whose bark, of the colour of cinnamon, made into powder amounting to the weight of two small silver coins and given as a beverage, cures the fevers and tertianas; it has produced miraculous results in Lima.

Calancha does not use an Indian term for the tree. He reports the meaningful Spanish name *Arbol de Calenturas* (fever tree), coined apparently by the grateful Spaniards who were cured by the wonder bark. There is attached no romantic story concerning the discovery of the miraculous drug or special circumstances preceding its introduction. The same holds true for the report on the "fever tree" given by the Jesuit Bernabe Cobo, who lived in Peru and Mexico from 1596 to 1653, in his *Historia del Nuevo Mundo*.

Every one familiar with folklore knows that very soon the one or the other story had to be invented in order to satisfy the deeply rooted human need for the extraordinary embellishment of the extraordinary.

There was very early the story of a lion who, shaken by fever, was observed drinking from a pond into which *Cinchona* bark was fallen, and immediately recovered. This was too legendary, of course, to be accepted as the truth not only by the plain people but also by scientists. There was, however, another story which was brought to public attention in the second half of the seventeenth century and which was believed by the learned as well as the unlearned until quite recently. It was the legend of the cure of the Countess of Chinchon, the wife of the Viceroy of Peru, by samples of the miraculous bark sent to her husband by the Governor of Loxa. Sebastiano Bado, an Italian physician and one of the most ardent promoters of the use of the antifebrile bark who gives the first account of this cure in a booklet published in 1663, says that he derived his information from a letter written by an Italian merchant who lived many years in Peru and redated the event "thirty or fourty [sic] years," meaning the time between 1623 and 1633.

When this [the happy cure] was learned in the City of Lima, the people approached the Vicereine by intermediaries, not so much joyfully and congratulatory, but supplicatingly, begging her to deign to help them, and say, if she would, by what remedy she had at last so marvelously, so quickly, recovered, so that they, who often suffered from precisely this fever could also provide for themselves.

The Countess at once agreed. She not only told them what the remedy was, but ordered a large quantity of it to be sent to her, to relieve the sufferings of the citizens, who often suffered from the fever. Nor did she only order this great remedy the Bark to be brought, but she wished to dispense it to the many sick with her own hands. And the thing turned out so well that just as she herself had experienced the generous hands of God in that miraculous remedy, so all the needy who took it marvelously recovered their health. And this bark was afterwards called Countess's Powder, which in Spanish is *los polvos de la Condeça*.

Since then this story has had a fixed place in all reports on *Cinchona* and its history. Following a well-known rule of folklore, it has been enhanced in the course of time by new and even more touching features. First more attention was given to the Governor of Loxa to whom the Countess allegedly was indebted for the remedy. It was stated that it was Don Juan Lopez de Canizares, that his own cure from malaria took place in 1630 and—this is the new feature—that the mysterious drug was administered to him by an Indian *cacique*. Scientists like Charles-Marie de la Condamine, who was in Peru from 1736 to 1743, perpetuated the story of Bado, and the great Linné gave it his blessings by naming the botanical genus "*Cinchona*" after the Countess, basing his new term on Bado's Italian spelling of the name which in correct Spanish reads Chinchon. Finally, in 1874, Sir Cl. R. Markham wrote an elaborate Memoir on the family of Chinchon in which he, without any evidence, dated the alleged cure of the Countess as 1638 and identified the heroine of the story as Ana de Osorio who married the Count of Chinchon on August 11, 1621. Markham furthermore added another detail, namely that the Countess on her return to Spain "administered Peruvian bark to the sufferers from tertian agues on her lord's estates, in the fertile but unhealthy *vegas* of the Tagus, the Jarama, and the Tajuna. She thus spread blessings around her and her good deeds are even now remembered by the people of Chinchon and Colmenar in local traditions."

This touching story is based entirely on reports received by Sir Markham from the administrator of Chinchon who himself related "local traditions" without any documentary proof whatsoever. It is understood that such "information" cannot be regarded as a very reliable source.

Nevertheless, in 1930 the story of the cure of the Countess of Chinchon, with all its embellishments, was generally believed with the only change that now the second and not the first wife of the Count was thought to have been the heroine. The likewise very dubious story of Don Juan Lopez de Canizares, Governor of Loxa, having been the first European to be cured through the use of *Cinchona* bark was even made the basis of a *Cinchona* Tercentenary celebration that, in 1930, was observed all over the civilized world.

It was Henry S. Wellcome, born in 1853 in Almond, Wisconsin, U.S.A., as the son of an itinerant missionary preaching among the Indians, and at the time of his death in 1936 an English baronet and the head of one of the world's biggest pharmaceutical houses, who initiated the observance of this celebration and arranged a unique *Cinchona* exhibition within another unique creation of his, the Wellcome Historical Museum, in London. Although this celebration was based on historically uncertain ground, the world of science has all reason to be grateful for it. At no other time and occasion would it have been possible to collect and secure so much valuable material concerning the history of, and the scientific work done on, *Cinchona*.

If there were such a readiness even in the world of science to submit to the allurement of a touching story, what could be expected from the laymen? Thus there appeared reports and finally novels and plays that gave the story still another turn, making it a dramatic event in the relations between the Indians and the Spanish conquerors. Now it was not the Governor of Loxa who, cured by the benevolence of an Indian, conveyed the miraculous drug to the Countess of Chinchon, but there was a direct relation between the Indians and the wife of the Viceroy of Peru. A story of this kind was written by a female Dutch novelist, Madame de Genlis, about 1800. It was dramatized by Jan de Quack whose play was published in 1819 under the title *Zuma, Of De Ontdekking van den Kina-Bast.* The legend of the first use of *Cinchona* bark by Europeans had reached its sentimental and sensational climax.

Enmity prevailed between the Inca and the representative of the conquerors, the Count of Chinchon. The Spaniards died from tertian fever by the hundreds while the Inca, having the means to cure them in his hands, observed the death of his enemies with diabolic pleasure. Then the Countess herself, the wife of his main adversary, was stricken with the illness and her death seemed to be unavoidable. In this moment Zuma, the daughter of the Inca, who secretly adhered to the Christian faith felt herself compelled to help. She furnished the Countess with the powdered bark of *Cinchona,* the Countess immediately recovered, and the play of Jan de Quack closed with a splendid scene of fraternization between the Spaniards and the Indians. There are frescos in the pharmacy of the S. Spirito Hospital at Rome of an apparently earlier period picturing the delivery of the *Cinchona* bark to the Count of Chinchon and his administration of the life-saving potion to his Countess.

It was in the same year in which the *Cinchona* Tercentenary Celebration spread all these stories once more all over the world that a discovery was made which once and forever deprived the romantic texture woven about the first use of *Cinchona* by Europeans of its historical fundamentals. The irony of history made the same man who had initiated the celebration the initiator of this discovery. It was in behalf of Sir Henry S. Wellcome that Miss I. A. Wright undertook the research in the Sevillian *Archivo Generale de Indias* in the course of which she found the official diary of the Count of Chinchon relating to his term of office as Viceroy of Peru and meticulously reporting every detail in the life of the Count and his family. A. W. Haggis to whom the world of science is indebted for his masterly investigation and clarification of the history of *Cinchona* published in the *Bulletin of the History of Medicine* (vol. X, 1941) concludes:

2. That the absence of any mention of the remedy or of any serious illness of the Countess in the official

Diary of the Count of Chinchon strongly suggests that the romantic story of the cure of the Countess by *Cinchona* is no more than a fable. 3. That she never returned to Spain, but died at Carthagena, Colombia, on 14 January 1641, and so could never have brought the remedy to Europe, nor distributed it to the poor of her native country.

Thus, the touching story of the part of the Countess of Chinchon in the discovery of the medicinal qualities of *Cinchona* bark exploded about three hundred years after it was reported for the first time.

But was there not still another possibility? Since it was proven beyond any doubt that the Countess could not have been the heroine, what about the Count as the hero? It may be that only the roles of the two had been confused, that she helped him to the life-saving drug instead of him doing that to her. It speaks for the immortality of well-told legends that this version forms indeed the last line of defense of those whose hearts long for romance as an integral part of life. True, the Diary of the Count in reporting the attempts to fight the fevers from which he was suffering tells only of bleeding and purging and there is not the slightest intimation of a new and miraculous drug like *Cinchona*. But could not the whole thing have happened nonetheless, maybe after the termination of the Dairy and, above all, were there not other events or considerations offering at least circumstantial evidence?

In his book *La Introduccion de la Quina en Terapeutica* published in Mexico in 1941, the eminent Peruvian historian of medicine, Dr. Carlos Enrique Paz Soldán, tries to furnish this evidence. He refers to the report of "a credible historian" telling that "having received marked favor from Our Lady of the Prado, the Count of Chinchon decided to erect her a church and gave for this purpose 80,000 pesos," and later on sent to the Virgin from Cartagena other gifts valued at 100,000 pesos. In the opinion of Dr. Soldán these extraordinary donations have to be linked with the greatest events in the life of the Count: his improved health since 1638 and the death of the Countess in Cartagena (1641).

The following quotations from the book of Soldán are taken from the translation rendered by Georgianna Simmons Gittinger and published in the *American Journal of Pharmaceutical Education* (vol. VIII, 1944, January):

> Who cannot see . . . that the most solemn and extraordinary part of his [the Count's] life in Lima, [says the eloquent Soldán,] was to find himself free of the implacable malarial relapses. . . . How can we fail to believe that this fact was not due to the medicine of his doctors but to the bark sent from distant lands. . . . How can we doubt that if he took the quina, a therapeutic adventure then, he did it on the counsel of his faithful wife, unselfish and devoted nurse to whom was sent the precious unknown substance?
>
> There were Don Juan de Vega and his colleagues who for years supported the Count with their Latin aphorisms and their lancets diminishing by daily bleedings the strength of his organism. How could they view the liberating quina other than something supernatural?
>
> The miracle happened and all had to accept it.

These sentences are beautiful, but they put sentiment in the place of evidence. They do not explain why there was no mention of the wonder bark as the source of the miracle, and donations to the church by wealthy persons were too frequent in the times concerned to be taken as proof for extraordinary events causing them.

Do we really have to accept the miracle? This question may well be answered in the affirmative if we rise to the concept that the miracle lies just in the so-called natural course of life and not in the supernatural, in truth and not in fancy.

AN ANTHROPOLOGICAL CAMPAIGN ON AMCHITKA*

By CAPTAIN PAUL GUGGENHEIM

MEDICAL CORPS, ARMY AIR FORCES

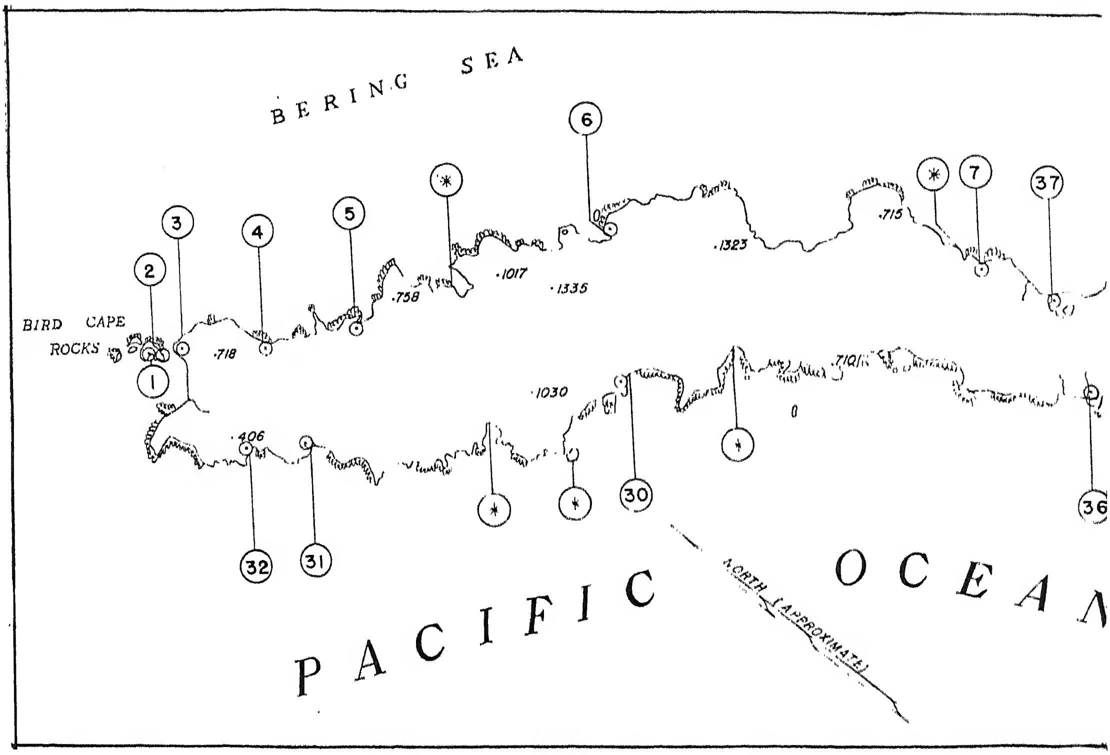
Army life in the Aleutian Islands is different from army life anywhere else in the world. It is unsurpassed for loneliness, isolation, and foul weather. Spending a summer there on an expedition is romantic, but the deadly routine of military duty gives the islands the aspect of a Siberian prison. There are no natives (since the war), absolutely no feminine society, and only the most primitive of physical comforts. The islands are not without an awesome, primeval beauty with their magnificent volcanoes and rolling, treeless hills, although somewhat the worse for military occupation, which everywhere raises mud and banishes the life of the earth. Winter and snow greatly improve the appearance of the regions occupied by the troops. Not until I had spent a winter in the Aleutians did I appreciate a certain antinaturalistic and, to me, repugnant line in Milton's hymn *On the Morning of Christ's Nativity*, which refers to snow covering the "foul deformities" of nature.

Amchitka is one of the major islands of the Rat group, lying near the 180th meridian. Before the taking of Attu and Kiska it was our most advanced Aleutian base. I landed there on May 1, 1943, my twenty-eighth birthday. Although I was part of a busy tactical organization, for me, a mere medical officer, the campaign was largely an affair of *Sitzkrieg*. I could not help thinking what a coincidence it was—my being in the Aleutians again. I had been with Dr. Aleš Hrdlička in 1937 on the Smithsonian Aleutian Expedition and had noted in my journal: "Our trip may be just a prewar interlude." War clouds hung heavily over the world that summer, but little did I dream that I would return six years later as a soldier. Being on Amchitka now, I thought, afforded a rare opportunity to do some scientific work, for the island was anthropologically unexplored, except for the region of Constantine Harbor, which Dr. Hrdlička investigated in the summer of 1938. In his report of that summer's work one paragraph was devoted to Amchitka:

> The next stop was on the island of Amchitka. After a stormy night on the little *Ariadne* we were put off in Constantine Harbor, where there are a couple of small houses recently constructed by the Bureau of Fisheries and four native trapper dwellings, with an attractive little native church. There are no inhabitants on the island in summer. We found here two good sites, largely pre-Aleut, and had three weeks of assiduous excavation. This yielded, aside from a variety of cultural objects, several of the oblong headed pre-Aleut skeletons, and, deep in the deposits, the first specimens found in these parts of the world of well wrought deep stone pots and dishes.

I had been corresponding with Dr. Hrdlička, but not until the end of the Attu-Kiska campaign was I able to tell him that

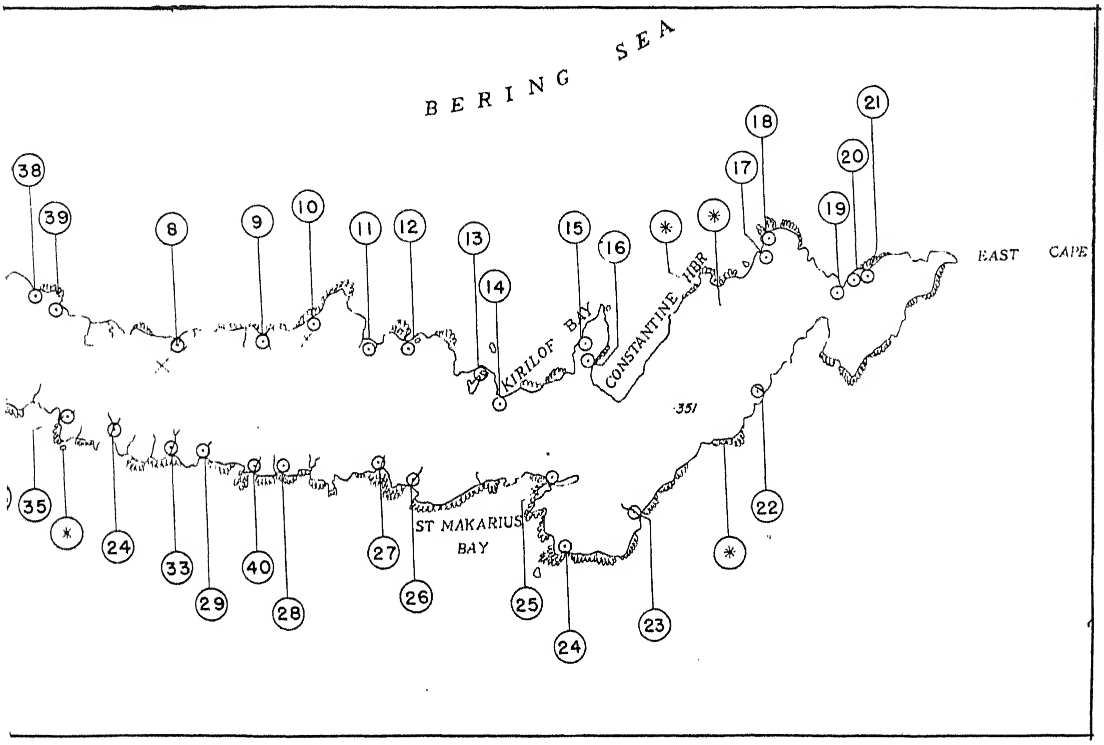
* The author acknowledges with gratitude the assistance rendered in the work here reported upon by many persons whose names will not be repeated throughout this account. They are: T/Sgt. Ralph Tierney and M/Sgt. John Horubeck, who discovered site 9; Lt. Berniece L. Garner, who reported sites 11, 12, 13, 14, and 26; Capt. Niel Ritchie, who independently discovered and worked site 13 (Pistol Lake); M/Sgt. Frank Penn, who reported site 27; officers and men of my old outfit: Capt. O. E. Hanes, M. C., Capt. Dick Maskal, D. C., S/Sgt. Dick Laskey, Cpl. Dalton Pile, Pvt. Edmund Highlander, Pfc. Lester Brown, Cpl. Willie Briggs, Pvt. Frank Muller, M/Sgt. Opie Wellborn, Sgt. Jerry Van Reusaeller, and the indefatigable Cpl. René Wendell, who discovered sites 17 and 18; Cpl. George Frazier; Capt. Dean T. Henry and M/Sgt. Clark W. Bowman, who did valuable work on the Clevenger-Makarius site (25); Pvt. Michael Koker, who was the authority on site 10; Cpl. George A. Blahuta; Lt. James W. Graves; S/Sgt. Bayless Blake, who discovered site 29; Cpl. Russell E. Hemmingway and Sgt. Niel Stinner, who showed me the first Aleut skulls (from site 29); Capt. Bryan Ralph Hoover; Lt. Lewis Turner; Cpl. Joseph Ernest; Lt. Donald Arnold; Lt. Bill Pearson, post special service officer who acted as central coordinator; Lt. Ray Van Slyke; Cpl. Carleton King; Sgt. Bob Ruelle; Capt. Richard D. Rucker, D. C.,; Pvt. Lee Malone; Lt. Robert Jones; Sgt. Jerry Ippolito; Lt. (j.g.) Paul Griffeths and Lt. (j.g.) Marcus Stoddard; Capt. Ralph DeBit, M. C.; Lt. Noble Drennen; Capt. (Chaplain) Henry Zenter, who reported site 22; Capt. Seaholtz, who reported site 19; Pvt. Tope, who excavated the wonderful war club from site 28; three civilians of the West Construction Company who did much work on Dr. Hrdlička's old site: Hans Mross, Tom Hinchy, and Bill Hunt; and finally my faithful helpmate, Lt. Col. Delmar R. Hughes, who assisted with extensive work on this same site.



PREHISTORIC VILLAGE SITES
SITES ARE NUMBERED CLOCKWISE AROUND THE ISLAND BEGINNING ON BIRD CAPE, EXCEPT NOS. 33–40, WHICH

I was in the Aleutians again. He was most encouraging and wrote many letters exhorting me to make investigations and report the results to him. He died on September 5, 1943, before I returned to the States.

Administrative difficulties. Owing to the tactical situation and to the fact that I had no official status as an anthropologist, I wrote to the commanding general of the island requesting permission to reconnoitre the island and excavate archeological sites. This was granted, but many difficulties arose. Diaries were not permitted, and so what records I was able to keep were mostly fragmentary and vague. We were not allowed to stake off sites or erect signs. Worst of all, there was no way to control the vandalism of casual collectors. Without orders from Washington the general rightfully deemed it inadvisable to place excavation under rigid military control. In a misguided effort to encourage a scientific approach the Force Headquarters issued several memoranda containing my suggestions for making excavations in an orderly and scientific manner. These publications served to create a widespread interest among both officers and enlisted men. "Digging" reached almost epidemic proportions. Needless to say, scientific procedures were not followed. It was every man for himself, using the foxhole technique. At no time during my stay on the island was it possible to do any extensive, systematic excavating without "sabotage." Repeatedly I spent days or weeks making a beautiful exposure on some quiet, isolated site, only to have it tumbled down and filled in by poachers. It was foolish to become embittered over this, for "souvenir hunting" did provide an interesting and profitable outlet for the "prisoners of Amchitka." But realizing how little could be done without more support from higher up, I kept writing to Washington urging that advantage be taken of the unique opportunity offered by the military occupation of the Aleutians to conduct widespread anthropological investigations and describing the practical difficulties caused by the soldiers' depredations. In February 1944 the



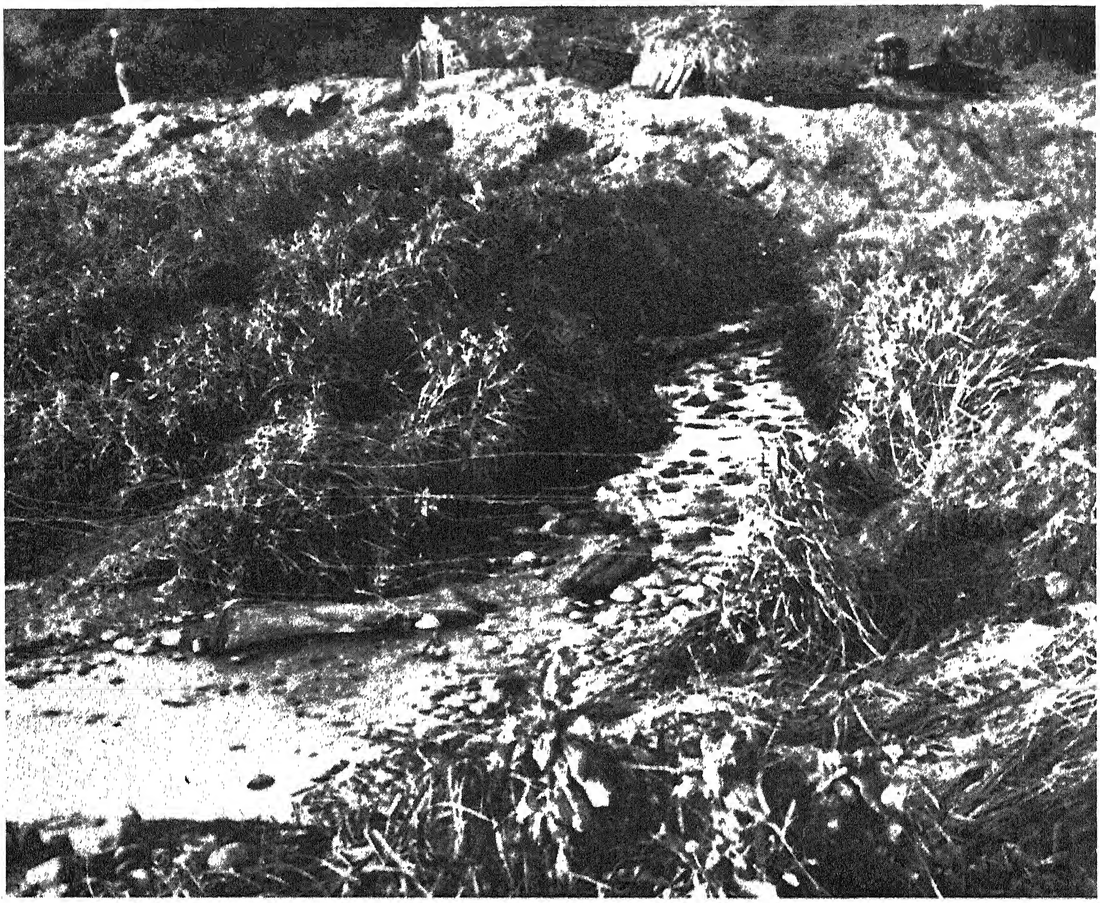
ON THE ISLAND OF AMCHITKA
WERE INTERPOLATED AFTER THE OTHERS WERE NUMBERED. THE *asterisks* DESIGNATE PROBABLE VILLAGE SITES.

National Research Council brought the matter to the attention of the War Department, and an order was issued by Headquarters of the Alaskan Department proscribing indiscriminate excavating by military personnel, invoking the Antiquities Act of 1906. Some provisions were made for the collection of specimens unearthed in the process of necessary military excavation, but no program for carrying out deliberate scientific work was mentioned.

General Description of the Island. Amchitka is a long, slender island, approximately thirty-three miles in length and nowhere much wider than two and a half miles, so that in many places one can view the Pacific and the Bering simultaneously. The western part is hilly, and there is a low range of weird-looking mountains whose desolateness suggests a Doré illustration of Dante's Inferno. The central and eastern parts are flat. The coast is rugged, with hills or cliffs dropping directly into the sea or overhanging scant, rocky beaches. The hills are separated by draws or defiles, which begin some distance inland and run down toward the sea. Streams, many of them subterranean, run in some of these draws. There are many small, fresh-water lakes, especially in the central and eastern portions of the island. The flat part is covered for the most part by muskeg. Tundra is not so common. The old village sites are almost invariably covered by tundra, which can be distinguished from a considerable distance away because of its coarser texture and deeper color. Without exception the sites are located on the coast.

Amchitka is one of the great breeding grounds of the sea otter. There are many birds—chiefly gulls and other sea birds, ptarmigans, and enormous black ravens. In summer there are many wild flowers growing in the muskeg. The island is notoriously foggy, even by Aleutian standards.

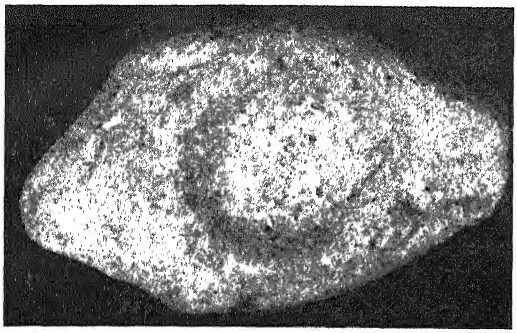
During my ten months on Amchitka I had opportunity to explore much of the island personally. Many of the archeological sites, however, were discovered by others and reported to me. I gathered information from



CLEVENGER-MAKARIUS SITE (NO. 25), SHOWING EFFECTS OF INDISCRIMINATE DIGGING

all available sources, making innumerable trips to explore new sites, interviewing soldiers, and examining their collections. It was usually a simple matter to determine the exact locations of the sites, by using the 1,000-yard grid map of the U. S. Army Engineers. Some of the sites have been named after their discoverers or other individuals prominently associated with them. In all, forty sites were catalogued (see map). The more interesting of these will be described in the following pages, along with some experiences and finds. It should not be forgotten that our archeological operations on Amchitka were fragmentary, constantly interrupted, and, in general, conducted on a "catch-as-catch-can" basis. Many men participated at various times (see footnote, page 21).

Clevenger-Makarius-Pacific Site (No. 25). On May 18, with two companions, I made my first reconnaissance hike. We headed down to St. Makarius Bay and at the bottom of it came upon a stream draining a large lake (Clevenger). There was a village site occupying both banks. Facing away from the Pacific, the right or east bank presented a flat appearance and could hardly be called a midden, but on the left or west bank was a good-sized midden. The stream and its site lay in a narrow valley that broadened out inland toward the lake. On the east was higher ground. To the west the site extended from the midden up onto some high cliffs for a considerable distance. On the midden were two habitable barabaras, one fairly large, in which some enlisted men were living. There was also a little, one-room, gray, frame house (since destroyed) built by the Bureau of Fisheries. Depressions left



STONE LAMP FROM SITE NO. 25
PROBABLY FROM A DEEP LAYER. NOTE THAT IT IS IN FORM OF SEAL PUP—MOUTH, FLIPPERS, TAIL VISIBLE.

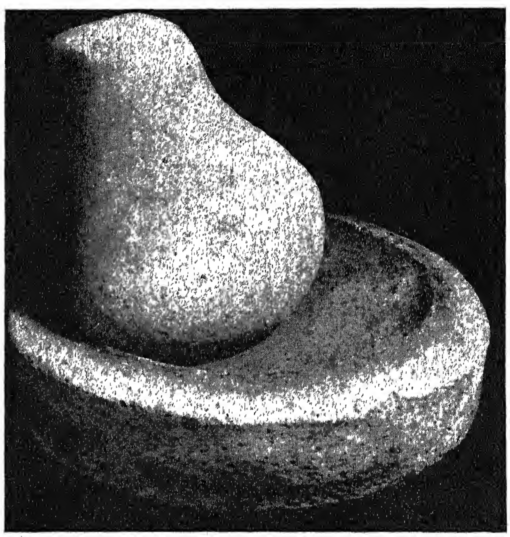
by old barabaras were evident in various places on the midden. In the stream below were pieces of the skeleton of a large whale. We began to excavate the midden on the side facing the stream.

The inhabitants of the barabara agreed not to disturb our exposure. In a short time, however, everyone in that part of the island seemed to know of the site, and on good days it was actively vandalized by as many as thirty persons at a time, ranging from privates to lieutenant colonels. In working there with our medics I gradually rediscovered the pleasurable sensations of performing certain archeological motions—tumbling down muskegs, undercutting, floor-sweeping, picking in the shell deposits. It made me feel as if something long dormant was awaking to life. The desert bloomed, and I forgot about the war.

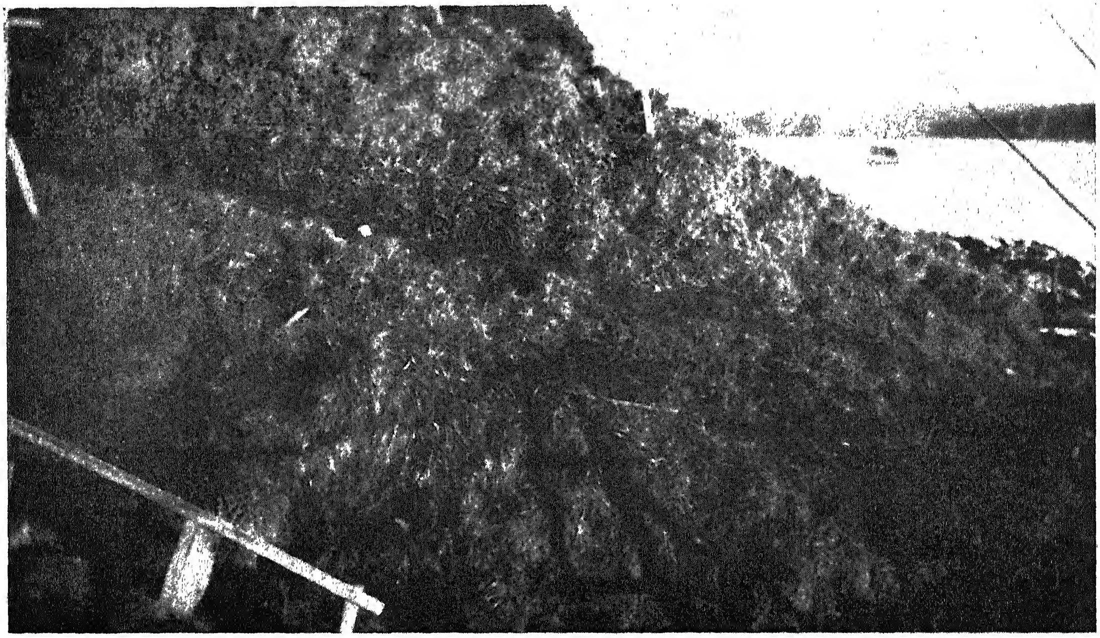
Late in June I received a phone call from Capt. Dean Henry, a former Missouri school teacher, who invited me over to dinner and presented me with a fine skeleton that he had excavated from the northern side of the midden, 4 feet beneath the muskeg. "Oscar," as we called him, was a long-headed male, stretched out in a casual position in deposits of greenish shell mixed with dirt. Some bone harpoon points were found near him. This represented a moderately deep, but not the deepest, deposit. Not long afterward "Mable" was found. She was a long-headed, rather horse-faced female and was uncovered near a barabara just beneath the muskeg on the flat side east of the stream. She lay in the knee-chest position, on, but not in, the shell deposit. Nearby was a badly shattered cranium ("Mable's friend"). In August another soldier contributed the complete skeleton of a young infant from the deepest layer of the midden. Both fontanels were open; the cortex of the right mastoid tip was gone, and one could view some large cells within. It is possible that this infant died of mastoiditis with a Bezhold's abscess.

All the skulls found in this site were long-headed and therefore probably pre-Aleut. I saw no brachycephalic skulls corresponding to the Aleut post-Russian culture, later discovered on the west bank. The site is probably an old pre-Aleut one, with a thin overlay of post-Russian culture in the midden. This excellent site was literally torn to pieces by the soldiers, and many skeletons were scattered and lost.

Most unique of the cultural objects found in the Clevenger-Makarius site were fine ivory needles, which so far as I know were collected nowhere else on the island. They were of various lengths. The commoner type had a groove at one end around which the thread was tied. Of rarer occurrence was a needle having a minute but perfect eye for threading. Occasionally a double-pointed needle (toothpick) was found. Other objects included two unique stone lamps, carved in



PESTLE AND STONE LAMP
FOUND TOGETHER IN THE DEEPER LAYERS OF THE CLEVENGER-MAKARIUS SITE (NO. 25) ON AMCHITKA.



VIEW OF KIRILOF CLIFF SITE (NO. 16)

the form of a seal's head and a complete seal pup; carved links of a bracelet or necklace; a finely tooled shard, apparently part of a dish or lamp; polished stone knives; bone wedges; and a beautiful little ivory dolphin, evidently worn as a pendant. The existence of a thin overlay of post-Russian culture in the midden of the west bank was demonstrated in January 1944 by Cpl. René Wendell, of Pittsfield, Mass., who found in the superficial layers of the midden several bone knife handles with rusty copper blades (much patina). He also had some round, glass, blue-and-white beads that a friend had found there.

During the winter, excavation was difficult because of the frozen ground. I visited the site occasionally, poking about, but it was a shambles. The yield of specimens, once rich, showed signs of exhaustion. Undoubtedly, however, work could still be done there under favorable conditions.

Kirilof Cliff Site, Constantine (No. 16). On May 20 I set out with two other soldiers to locate the two sites that were described by Dr. Hrdlička in 1938. We approached Kirilof peninsula from the harbor side and found a large site perched on the first cliff overlooking the harbor. This, we presumed, was Dr. Hrdlička's "Hill Site." Part of the cliff had been blasted away in road building, and there was no sign of any scientific excavation. We found, however, that three civilians had been working the site, and though they used the foxhole method of digging they were careful, diligent workers. They had exhumed several skeletons and had showed them to medical officers at the station hospital, who, I was told, did not know what to make of them. The civilians, therefore, thinking that the skeletons were valueless, discarded them. I explained to them the significance of their finds and they were keenly interested, showing me their beautiful collections of artifacts. They had much carved ivory and a number of beautiful labrettes, some larger than any I had seen in 1937.

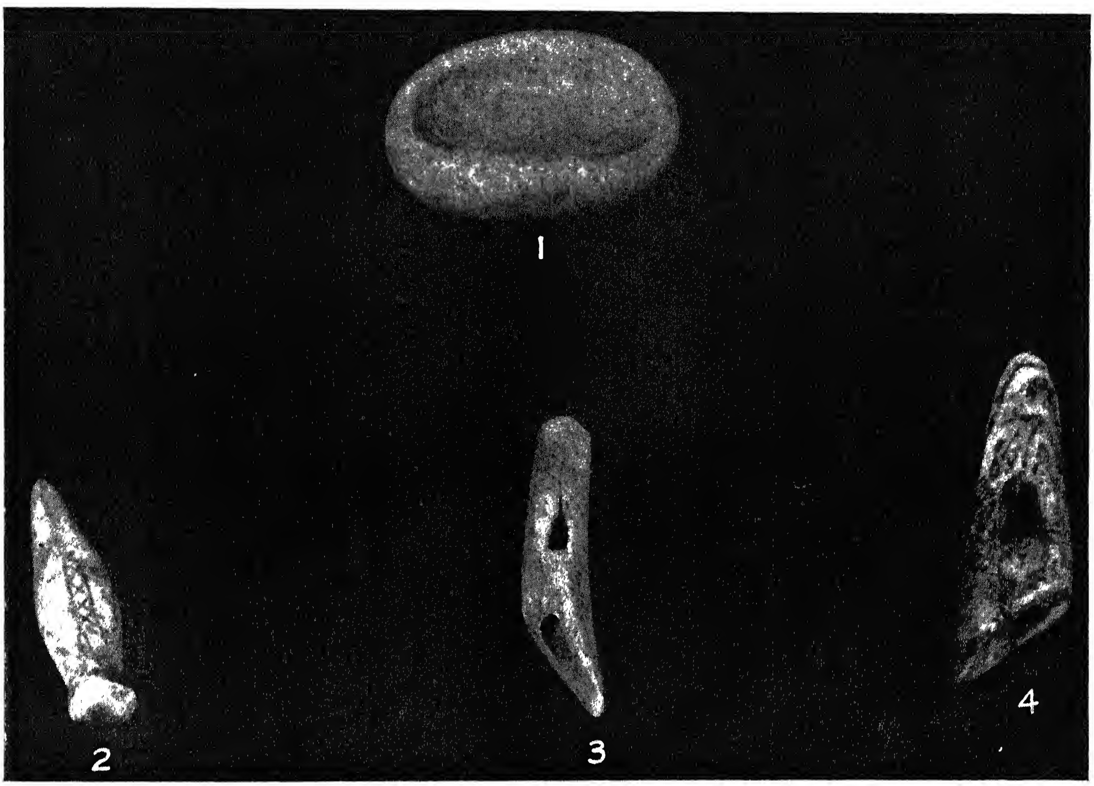
On Memorial Day we began to work the cliff site and the next day found two interesting specimens: one an elaborately carved awl or sewing instrument of fossil ivory and the other a tiny stone lamp. This lamp was the smallest one I had ever seen, measuring only $2\frac{1}{2}$ by $1\frac{3}{4}$ inches. We called it our "5-watt night light." The sewing instrument was one of the finest pieces found on

the island. As we worked that day we witnessed the burial of the post inspector who had been killed in a vehicle accident. One could not help an embarrassing sense of the bizarre cycle here being enacted as we interrupted our unearthing activities to stand at attention while the coffin was interred.

Late in the summer and fall my faithful companion Lt. Col. Delmar Hughes and I worked this cliff site nearly every evening after supper. We managed to start a fairly extensive excavation, but late in the fall poachers destroyed our work and thereafter we limited ourselves to "poking about." To complete our discouragement some sailors one day dug up an ivory doll made from a sperm-whale tooth, and this was their first visit to the site! The colonel seemed to have considerable luck picking up artifacts, many of them very fine ones, along the beach. His private collection was one of the finest on the island.

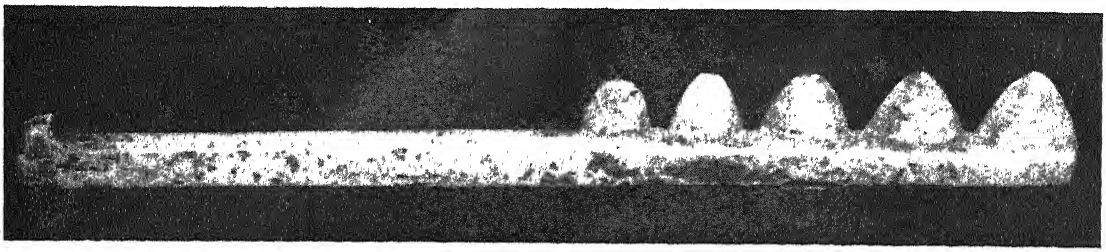
Exploring the Central Portion of the Island. Several other promising sites were located on the Bering side of the island. One evening our mess sergeant and I set out for a site (No. 9) which was only about three-quarters of a mile from camp and in which 14 skeletons were reported to be lying exposed. We rounded one point on the Bering and came upon a huge green midden almost filling the bottom of a fair-sized bay. There were many barabara holes, and in one particularly large one were the skeletons. Unfortunately someone had taken two-thirds of them and left the rest scattered about in utter confusion. I sorted them as best I could.

Though I was never able to start anything systematic at this site, I managed to buy some fine artifacts from some of the men who had merely scratched the surface and been rewarded by a virtual shower of specimens. These came from the barabaras. There were some very long bone harpoon



AMCHITKA ARTIFACTS

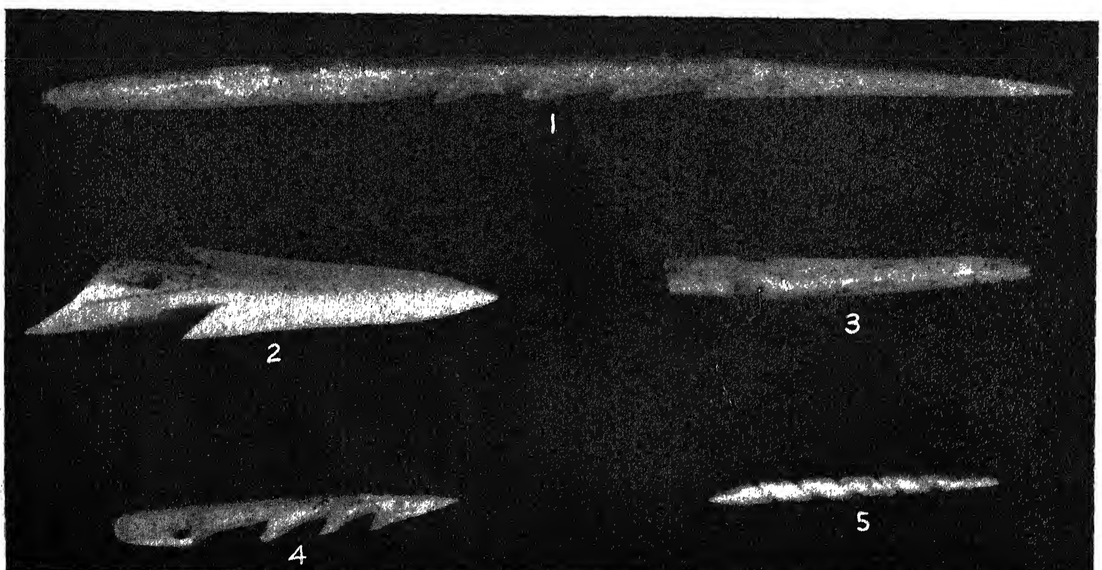
1, TINY STONE LAMP FROM KIRILOF CLIFF SITE (NO. 16); 2, IVORY CARVING RESEMBLING A FISH HEAD; 3 AND 4, FOREPIECES OF A TOGGLE HARPOON ASSEMBLY. SOLDIERS FOUND, BOUGHT, AND SOLD SUCH ARTIFACTS.



WAR CLUB FOUND AT BOTTOM OF SITE NO. 28
REPRESENTING THE VERY ANCIENT PRE-ALEUT CULTURE. MADE OF A BONE OF A WHALE AND HIGHLY POLISHED.

points, measuring up to 1¾ feet; shorter harpoons and bird darts; and an interesting midpiece of a spear, made of ivorylike bone and perfectly split in the longitudinal axis, the two halves being designed to be tied together by thongs. [Interestingly enough, I later acquired an identical object from site No. 28 directly across the island from No. 9. With it came what is probably the finest object ever found on Amchitka—a highly polished, finely worked, and very lethal war club about 1¾ feet long, made of dense, partly mineralized bone of a whale and having five vicious teeth of graduated size. This was found at the bottom of a hole 16 feet deep dug at the edge of the site adjacent to the Pacific, extending down through all layers of the deposits into dirt and rock. It is, therefore, undoubtedly very ancient.]

In August I spent a day at Pistol Lake site (No. 13), a picturesque spot on the Bering Sea. The lake and its site lay at the end of a sloping valley and could be seen from far inland. The soldiers had dotted the place with foxholes. In the evening I gave a lecture to the men on archeological matters, but having given several previous lectures I was becoming very skeptical about the value of my educational efforts, which seemed only to encourage more vandalism without yielding any material for the collection. The soldiers were always keenly interested but clung possessively to their bones in most instances. It was not unusual to see skulls, with eye and nasal sockets painted red, adorning tent posts on Amchitka, and to me it was a painful sight even though it furnished some amusement.



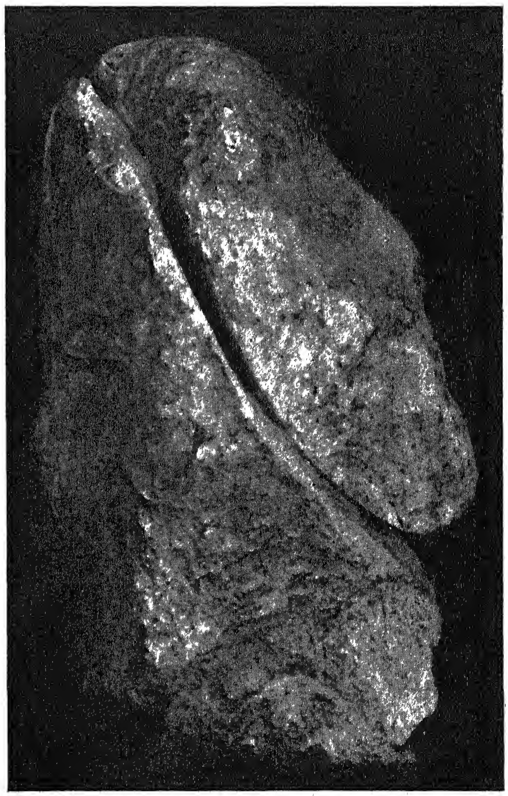
ARTIFACTS FROM CENTRAL AMCHITKA
1, BIRD DART; 2 AND 3, HARPOON POINTS; 4, MID-PIECE OF TOGGLE SPEAR ASSEMBLY; 5, BONE TOY (?).

EXCAVATING AT BIRD CAPE

Bird Cape (Nos. 1–3). The community at Bird Cape was something a little out of this world. The scenery there is the grandest on the island. The narrow coastal lowland is sandy, a welcome relief from the ever-present mud. Just inland great hills arise, farther eastward passing into a range of mountains. One great hill, which dominates the cape itself, is of the truncated mesa type, a formation most unusual in the Aleutians but characteristic of the Commander Islands.

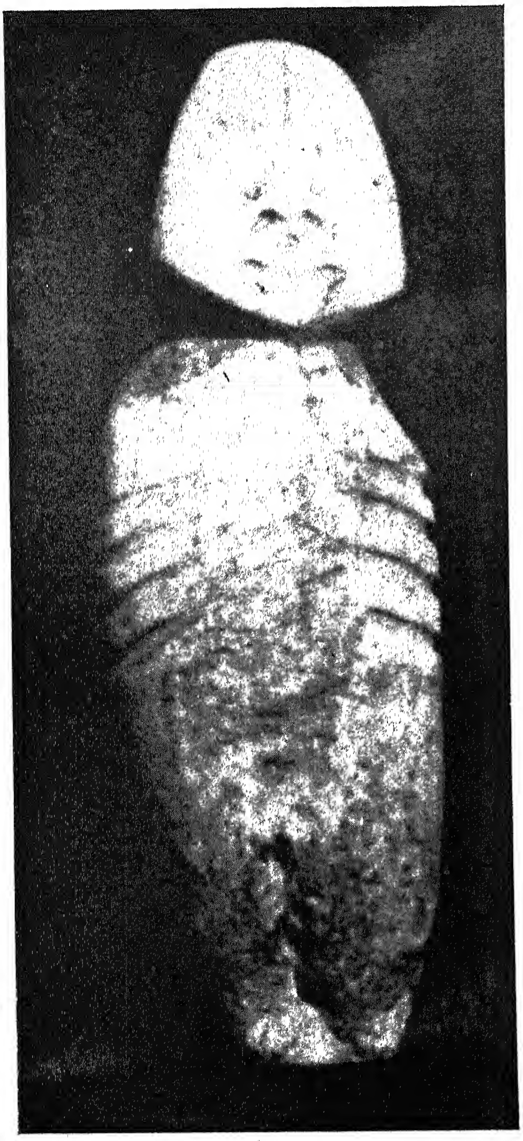
My first visit to the Cape was in the latter part of July, and during the four days I was there the weather was beautiful and sunny—a remarkably rare phenomenon. The region was utterly peaceful and idyllic. The view to the westward was magnificent, with four volcanic islands visible and on very clear days a fifth—Kiska. Every day as I excavated on site 3 the Kiska bombing missions flew over, reminding me that after all there was a war going on. On July 22 Kiska was shelled by our battleships. The great salvos shook the earth, even where I stood. The peacefulness of Bird Cape in the midst of all this was bizarre and incongruous. My excavations during this period yielded the complete skeleton of a child about six, at a depth of only 2 feet. I found also polished stone knives, stone lamps, and two very interesting double rubbing stones, which are supposed to be a cultural link with the tribes of northeastern Siberia.

In October I was able to return to the Cape for a five-week stay. I lived at the "country club," an elegant barabara that was really a subterranean log cabin. We had a fine radio and running water, hot and cold. The club has since been lined with celotex and furnished with a real porcelain wash basin and a shower. The local "elite" lived there, which always included the resident medical officer and two of the permanent party. It was the social center, and it was built on the edge of site No. 3—an ideal arrangement. I spent my leisure time excavating a long cor-



DOUBLE-HEADED (JANUS) DOLL

FROM LOWEST DEPTHS ($5\frac{1}{2}$ FEET) OF SITE NO. 40, EVIDENTLY AN ALEUT SITE AND SHOWING EVIDENCES OF POST-RUSSIAN CULTURE. DOLL SHOWN IN SIDE VIEW.



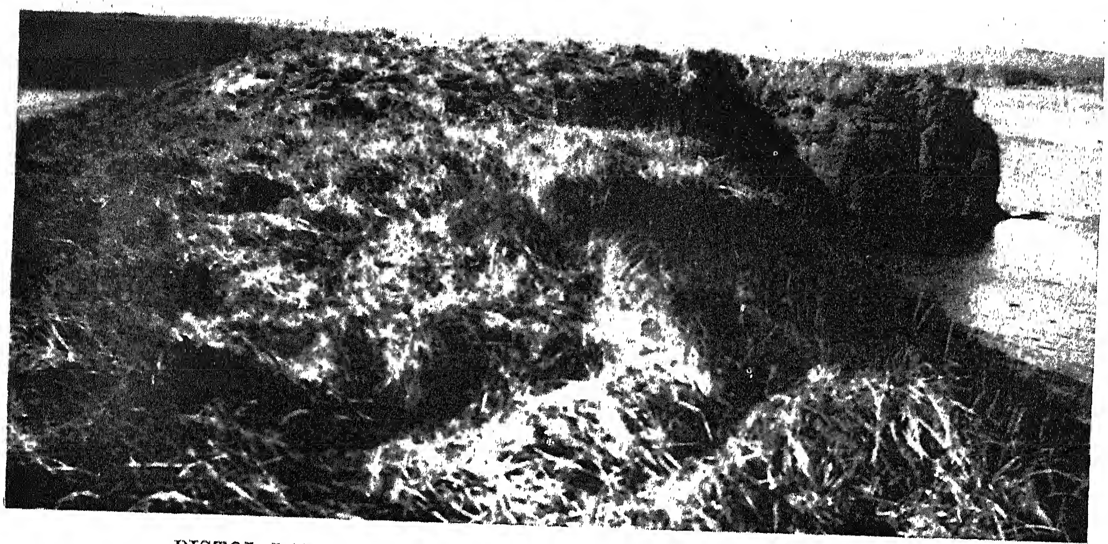
IVORY DOLL
CARVED FROM TOOTH OF SPERM WHALE. SITE NO. 29.

ridor about 6 feet wide, from the sea inward, in an effort to work my way completely across the site. I got perhaps a third of the way through, having arrived on the threshold of a barabara hole.

At the end of October we had a hurricane, and the day after the storm we crossed the Bird Cape channel in an amphibian and landed on the Rocks (Fox Island) to conduct some salvage operations on a wrecked ship, the crew of which got safely ashore. I took the opportunity to reconnoitre the island and found two sites (Nos. 1 and 2). No. 2 was the best one and was exactly opposite No. 3 across the channel and may have been occupied contemporaneously with it. Site No. 1 was on a narrow spit of land between a small fresh-water lake and the ocean. I made several trips thereafter to the Rock to work site No. 2 and found a few artifacts but no skeletons. The site may still be considered practically virgin. It was dotted by a great many square barabara holes, one next to another, in the centers of which were clusters of dried stalks of some tall flower resembling a sunflower. My little excavation faced the channel and may possibly still be visible from the Cape where I saw it stand out against both green tundra and winter snow.

I returned from the Cape on November 17 and, after this diversion, was glad to be back with my squadron. The snow had arrived while I was away, and I was amazed at the difference in appearance of our part of the island. Christmas was already in the air.

The Hobby Fair; Traffic in Specimens. In January we had a hobby fair on the island, sponsored by the commanding general. To my surprise there were many unusual exhibits of archeological specimens. First prize was won by an organization with about a dozen such displays. Our squadron won second prize. The fair gave me a chance to meet many men, hitherto unknown to me, who were successful diggers. Many of them were soon returning to the States and were willing to sell me some of their artifacts. If the reader wonders why so many specimens were acquired by gift or purchase, the reason is not far to seek. When, as was usually the case, I was unable to excavate properly without interference from poachers, I confined my activities to reconnaissance and inspection of the workings of others. Many soldiers had much better opportunities than I to work isolated sites intensively, and it is not surprising that some of them accumulated really remarkable collections. Furthermore, since money could not be spent in ordinary ways on Amchitka and since the men were reluctant to part with their prized artifacts on a donation basis, I invested over a considerable period of time about $500 in specimens that

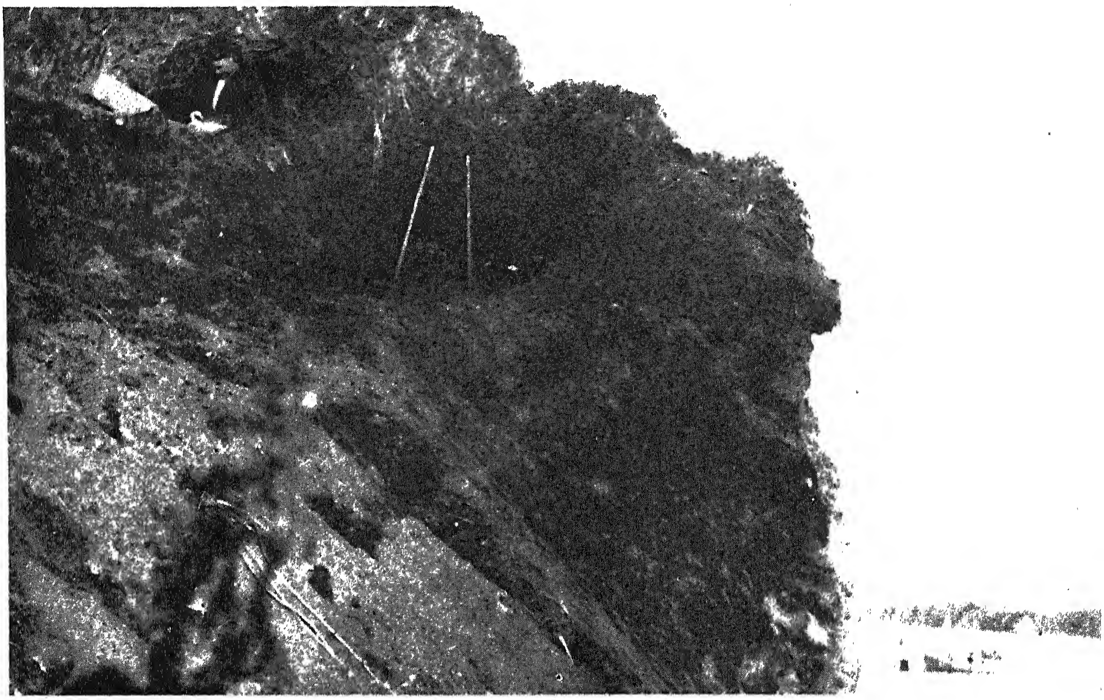


PISTOL LAKE SITE (NO. 13), POCK-MARKED WITH FOXHOLES

otherwise would have been scattered and lost. My personal collection largely consists of these purchased objects.

I could not bring myself, however, to buying skeletons, although some of the men got the idea that there might be a market for them. One day our mess sergeant showed me a fine dolichocephalic male skull he had obtained at site No. 25. He wanted $30 for it because of the unique fact that a stone point was completely embedded in it just behind the left orbit. I told him that I did not buy skeletons and did not consider it very Christianlike to traffic even in heathen bones. Later he reduced the price to $10, and again I declined, though very anxious to acquire the specimen. Finally he decided to send it home, and I asked permission to

AMCHITKA ISLAND—WINTER LANDSCAPE



A CORNER OF THE KIRILOF SITE (NO. 16) BEING EXCAVATED

borrow it in order to take some photographs. When I returned it, the sergeant broke down. "Gee, Doc," he said, "I haven't got the heart to keep it, knowing how bad you want it"; and he insisted on giving it to me despite my token resistance.

Concluding Days on Amchitka. Interestingly enough, it was only during my last month on Amchitka that evidences of Aleut occupation were forthcoming. All our previous finds had been pre-Aleut. A whole necklace of blue-and-white glass beads was found in site No. 17, east of Constantine Harbor, from a depth ranging from 1 to 3 feet. A harpoon head consisting of a rusty metal point held in a groove at the end of a bone piece about 6 inches long, was found in site No. 19 at a depth of 1 foot. From site No. 29, on the south side of the island, came five crania, three of them with mandibles, from 1 to 2 feet from the surface. These seemed to be unquestionably Aleut. Late in February I made a personal trip to this site and was further convinced that it represented Aleut and pre-Russian culture. It was an extensive site, beginning at the draw in the bottom of the inlet (see map) and extending along the whole western shore of the inlet. It is evident that there must have been an extensive occupation of Amchitka by Aleuts, although the dominant motif is pre-Aleut.

On March 6 I left the island, flying by way of Adak and Kodiak and reaching Seattle on March 28. Glad as I was to be home again, it was painful to leave my squadron and the dear friends who had shared so many privations and memorable experiences. As I have indicated, our archeological activities on the island were desultory and fragmentary, and it was not always possible to pursue them strictly according to scientific procedures. They showed, however, what might be done archeologically on Amchitka, as well as on other islands of the Aleutians. Most of our specimens have now found their way into the collections of the Smithsonian Institution, and when the history of man in Alaska is finally written it may be that our crude efforts will have contributed in some way.

THE PROBLEM OF THE AMAZON*

By F. FERREIRA NETTO

ASSISTANT GENERAL MANAGER, SERVICE OF AMAZON NAVIGATION AND ADMINISTRATION, PORT OF PARÁ, BRAZIL

[Translated by W. Andrew Archer]

THE REGION OF THE AMAZON RIVER BASIN

* TRANSLATOR'S NOTE.—This article by Lieutenant Netto, here presented in two installments, was first written primarily for Brazilians and was published in pamphlet form in Belém in 1942. Only 500 copies of the original Portuguese edition were issued, these mostly for official use and for local distribution to municipal offices. A mere 60 copies were offered for sale in Belém, and the pamphlet is now out of print. The present translation, therefore, seems justified, in order to bring to a wider reading public what seems to me a commendably clear and concise presentation of some of the facts concerning the great Amazon region.

In converting the original text into English I have taken considerable liberty in editing and rearranging sentences and paragraphs. A few sentences and one or two paragraphs have been omitted as not being of interest to North Americans. Furthermore, some of the scientific names of animals and plants have been revised to accord with usage in the United States. The original bibliography has been reorganized and complete citations given. These changes were made not from mere zealous dissension but rather in the hope that the resulting text would conform more nearly to English idiom and literary style.

In reviewing the section dealing with the fauna, Dr. Remington Kellogg, curator of mammals, U. S. National Museum, remarks, "The killing of white-tailed deer, brockets, and capybara for skins alone will result in the loss of the only available meat for the rubber hunters in the Upper Amazon. The trade in hides of these animals has reached such proportions that some notice should be taken and reference made to the need of some conservation measures. Possibly this can best be applied to the sale of such hides through commercial dealers."

In revising the plant names, Dr. S. F. Blake, of the U. S. Department of Agriculture, points out that the name *Nectandra puchiri* has not been located in the botanical literature. LeCointe refers the vernacular name "puchury" to two species, *Acrodiclidium puchury-major* and *Aniba puchury-minor*, but it is not possible at present to determine if the name as used by Netto applies to only one or both of these. Dr. Blake further indicates that the name "jarina" applies also to *Yarina microcarpa*.

All footnotes have been added by the translator.

Gratitude is expressed to the various persons, both Brazilian and North American, who have assisted and encouraged this work. Among these have been: Dr. Felisberto Cardoso de Camargo, director of the Instituto Agronômico do Norte; Zito Brígido; Fred L. Downs; Dr. Norman Bekkedahl; Walter Mors; B. Y. Morrison; Francis B. Thorne; Ralph R. Shaw; Philip Leonard Green; Louis C. Nolan; Prof. Th. Dobzhansky; Marcus Childs; and Forrest Sherwood. Special acknowledgment is due to Drs. S. F. Blake, Rogers McVaugh, and D. S. Correll, of the Division of Plant Exploration and Introduction, U. S. Department of Agriculture, for revision of the plant names; and to Drs. Waldo L. Schmitt, Remington Kellogg, Leonard P. Schultz, Herbert Friedmann, and Doris M. Cochran, all of the Smithsonian Institution, for like service in checking the animal names.

Lieutenant Netto's work is dedicated to His Excellency, Dr. Getulio Dornellas Vargas, President of the Republic of Brazil, creator and director of the reclamation program of the Amazon region.—W.A.A.

INTRODUCTION

ELDORADO or Green Hell? The mere fact that these two contradictory terms exist to designate this vast region watered by the largest river system of the world indicates how little is known of the Amazon Valley. In reality careful observation reveals the impropriety of either of these frequently used terms. Even though the Amazon is not a place where life is easy for everybody, neither is it a reproduction of the realm where *Lasciate ogni speranza* was inscribed over the entrance. Natural riches exist, but they are not to be obtained without work. It is as possible to live there as in any other part of the world, provided that elemental hygienic measures are observed.

The area of this least-known and least-inhabited part of Brazil is nearly 1,235,000 square miles, or 40 percent of the entire country. This fact alone deserves special attention in this day of concern with the grave subject of living space. Never before has the region been so much in focus as now, owing to the perturbed conditions prevailing throughout the world; and the Federal Government has pledged itself to study the varied problems which heretofore have defied solution.

The literature of the Amazon is enormous and deals with the most diverse aspects of the region. But even this varied and abundant literature, both difficult and costly to acquire, will not give everyone a perfect and

ready knowledge of the situation, especially in these times of speed and haste.

This little essay, without pretensions, is meant to give a general review of the subject as well as to include some personal opinions, which are the fruit of long study and impartial analysis.

The Factors

Land. The fertility of the Amazon Valley has been the subject of praise since the time of Humboldt. The "future cellar of the world" is a set phrase customarily used in referring to this vast region. We are led to this inference upon viewing the exuberant vegetation which extends for thousands of miles. However, this characterization is somewhat exaggerated and needs to be refuted for the good of the region itself. The fertility of the Amazon is a relative term and applies at best only to the native and spontaneous vegetation. Not that there is a lack of humus or a favorable environment. The density of the forest at once demonstrates that these do exist, and in abundance, but for systematic agriculture a tangled and heterogeneous forest is a hindrance. When the forest is cut down and the soil exposed to the copious rains of the wet season, all the fertile organic matter disappears completely, leaving the soil depleted and subject to the sterilizing action of the rays of the equatorial sun. For this reason, in order to continue cultivation, it is necessary to use fertilizers and to follow scientific methods as the conditions require. One cannot trust solely to Nature to obtain that which must be got by hard work.

Hundreds of species of detrimental plants, easily disseminated, are waiting to invade and smother any sort of cultivation if there is the least lapse in the continuous diligence that is necessary.

The greater part of the Amazon Basin, geologically the result of alluvial deposits, has not yet reached the final phase of stability, and consequently erosion appears just as soon as the protective vegetation is removed. If lines were drawn uniting all the principal waterfalls or rapids found in the tributaries on both sides of the majestic Amazon River, we would have an exact outline of the primitive, internal sea of a remote age. This body of water, with the passage of time, was being slowly converted into land; a process that continues even today, tending to the final transformation of the Valley into vast plain some thousands of years from now.

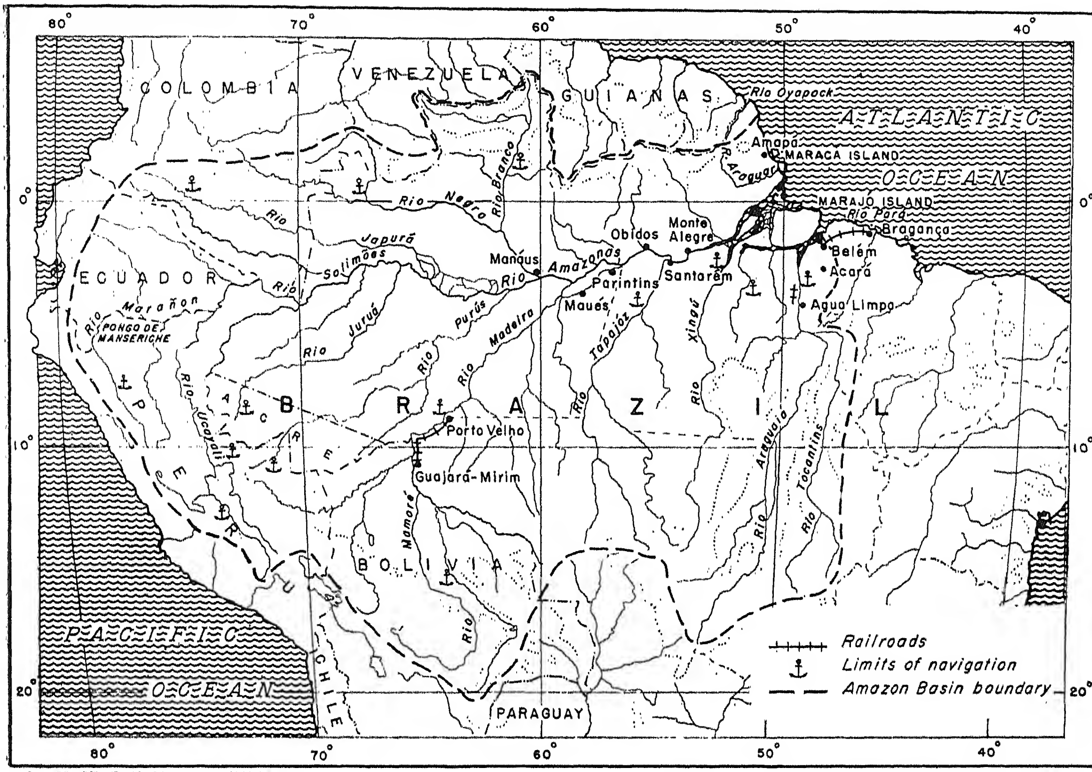
Several islands must have existed in that ancient, internal sea, but they have now been incorporated into the continent. Their vestiges are represented by the small elevations existing in the Lower Amazon region. Certain granite formations, sometimes isolated and surrounded by alluvial soils, as for example in the Bragantine region a few miles from Belém, undoubtedly have the same origin.

As yet no profound geological studies have been undertaken, but from the investigations already made the major part of the Amazon can be assigned to Cenozoic Era, while the marginal lands of most of the rivers, all the region of Purús, Juruá, and Japurá, as well as the islands situated in the estuary of the Amazon River, belong to the Recent and the Pleistocene periods. Paleozoic and Proterozoic zones, about 30 miles wide, traverse the region from east to west, in the heights of the central portion of the Amazon, and to the south and north of this, between the meridians 51 and 60 W. of Greenwich. Contained in these zones are the elevations existing in the municipality of Monte Alegre, in the State of Pará, which are known in the regional literature under the names "Lookout of the Lower Amazon" and "Amazon amphitheatre."

Many parts of the Amazon are still unknown to white men and await future explorations, the principal of these being the headwaters of the Xingú and the Tapajós. These rivers, including the Tocantins, in the region of the waterfalls, traverse a zone characterized by crystalline formations, where gold and diamonds are found.

Aside from sandstone, and not considering the occurrence of granite, the probable origin of which has been already explained above, no rock is found in the entire Amazon Basin up to the first waterfalls, this being equally true in the north and in the south and the east, where the slopes of the great Guiana, Brazilian, and Andean massifs begin.

This enormous drainage area, reaching



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THE REGION OF THE AMAZON RIVER BASIN

some points nearly 3,000 miles away, daily discharges thousands of tons of earth into the river. The eroded material carried into the ocean produces new accretions of land during the course of centuries. This is most pronounced in the vicinity of the north coast, between the Island of Maracá and the continent, where ships of more than 5 feet draft cannot pass, even at high tide, although they could do so only a few years ago.

Innumerable islands are in constant formation along the entire extension of the Amazon River, new land being formed by the slow accumulation of deposits carried by the current.

Seeing the outline of the river and its principal tributaries on a map does not give one a very good picture of the actuality because nearly all the watercourses that discharge into the Amazon are united with one another by a network of channels and creeks forming a veritable labyrinth. A multitude of lakes exists along all the lesser rivers, especially in the middle portion of the river-ocean.[1] There is almost as much water as land, and enormous portions of land are covered for a part of the year by shallow water, forming what are called locally *igapó*.

The margins of the rivers may consist of banks, which are being slowly eroded, but usually they are beaches of thick mud, covered by a characteristic vegetation, where alligators hide.

The slight declivity of the Amazon, extending almost to the Pongo de Manseriche, less than 1 foot to every 10 miles, permits the effect of the sea to be felt for about 500 miles from the mouth with reference to the ebb tide, and for a third of this distance for the flood tide. The antagonistic movements of the tide and the river current produce, in the estuary and in the tributaries of the left bank, the phenomenon known as *pororóca*,[2]

[1] "River-ocean" is a term used to express the enormity of the Amazon River.—W.A.A.

[2] The *pororóca* is a huge wave, several feet high, which appears suddenly and with a great noise to break against the river banks.—W.A.A.

so greatly feared by smaller craft navigating that part of the river.

A careful study still remains to be made of the water flow in the entire basin of the Amazon. Generally December to May is the period of high water and April to November of low water, coinciding with the rainy and dry seasons, respectively.

Aside from this variation of the water level there has been observed, in various tributaries far from the mouth, a series of irregular movements with as yet little understood causes. Occasionally some streams empty when they should fill, and vice versa. Furthermore, these movements are not simultaneous for the entire course of the river, there being sections where an accentuated water rise contrasts with a distinct lowering in other parts of the same river, either upstream or downstream. Copious rainfalls in the headwaters, which augment the volume of water and then cause the sudden rise in level, might explain in part these phenomena.

Constant shifting of the main courses of certain rivers, together with the continuous changing of the banks of mud or sand, gives an idea of the complications existing in this intricate river system.

Geographically the division of all this vast extension into similar or uniform zones is not possible, and so there has been adopted the more obvious entity—the river itself. Thus the name of a particular water course is generalized to indicate a certain region; accordingly the names Juruá, Madeira, Xingú, Tapajós, etc., indicate the zones bordering those particular bodies of water. This arbitrary delimitation, imposed by the circumstances, however, applies only to length and not to the depth of land, the latter having but small importance at present considering how little penetration there has been into the forest from the river banks.

In order to specify better the region of a certain locality, a still finer, though inexact, subdivision in terms refers to an area in relation to a certain part of the river, i.e., "upriver," "downriver," "midriver," and the "mouth."[3]

Only in the mouth of the Amazon River do lands take on a special denomination, not following the system just described. Thus we have the name "Region of Amapá" for the area along the left bank, between the Araguarí and the Oyapock Rivers. The term "Islands" applies to all islands existing in the mouth with the exception of the "Marajó," which is considered as a separate entity. Finally, the "Bragantine Region" applies to the right bank of the Pará River, which, according to different geographers, may be either the right branch of the Amazon as a drainage to the sea, or an independent river formed by the Tocantins.

Physiographically almost 90 percent of the total area of the terra firma of the Amazon region is in forests, the remainder being savannas.[4] These are encountered in various places, isolated and usually enclosed by the forest, forming islands in the sea of luxuriant jungle. The most extensive of these islands are found in the Middle Amazon, about 125 miles from the town of Obidos and in the region of Amapá. The Island of Marajó and the Upper Rio Branco contain still others.

Landholdings in the Amazon are exceedingly easy to obtain for anyone interested, private properties often being larger in area than some nations. Leasing is the common mode of exploiting the soil, though this contributes nothing to local progress. No conditions are imposed on the renters or owners, such as compelling them to plant annually a definite number of trees to improve the land. The system favored instead could be called the rape of Nature, because up to the present there has been nothing but the most excessive exploitation, without the slightest regard for conservation of natural resources. This aspect of the Valley has remained unchanged for long years owing to the negligence and shortsightedness of the administrators of public affairs, many of whom are interested in maintaining the *status quo*. What could have been done toward the improvement of the Amazon economy, little by little, during the past decades, is now a load on the shoulders of the new generation, which, as always, has to pay for the errors committed in the past through cupidity, greed, or laziness on the part of a few. The new genera-

[3] For example: Middle Tapajós, Upper Tapajós, or Lower Tapajós.—W.A.A.

[4] Savannas are open areas, characterized by grasses and shrubby plants.—W.A.A.

tion will need to struggle mightily if it wants to conquer this enormous portion of national soil.

Flora.—Paradise of botanists and entomologists, there is perhaps no region in the world to compare with the Amazon in the richness of species, many still awaiting scientific description. Indeed no other country possesses such a large number of indigenous plants as susceptible of economic exploitation. Unfortunately one does not find there great stands of a single botanical species or variety as is the case in other parts of the world. There are regions where one can walk for miles without encountering the same tree species twice.

The native plants of the Amazon having immediate economic value can be grouped according to the type of product as follows: (1) elastic gums; (2) oils and waxes; (3) resins; (4) lumber; (5) fibers; (6) perfumes and essences; (7) special substances.

To the first group belong the following plants:

Seringueira, *Hevea brasiliensis.*
Balata, *Manilkara bidentata.*
Caucho, *Castilla ulei.*
Coquirana, gutta, *Ecclinusa balata.*
Massaranduba, *Manilkara huberi.*

In all these the product is obtained by extraction of the latex, which is later coagulated by various processes. The first three species are of the greater importance, while the last two are harvested only if easily accessible because the low quality of their product hardly ever supports any great cost of transportation.

The region of production serves as an important means of classifying the types of Hevea rubber; thus the standard is considered to be that coming from the higher rivers, particularly the Juruá and the Purús, of the Acre Territory. In addition there is further classification of rubber according to the quality, which is the result of the coagulation and smoking. The categories "fine," "medium," and "sernambí" are used. "Fine" applies only to rubber obtained from perfect coagulation and smoking. "Medium" is badly coagulated rubber, recognized by the milky appearance of the product. "Sernambí" consists of residues, naturally coagulated, unsmoked, and usually mixed with earth and other debris.

The process, used for long years in the extraction and coagulation of the latex to produce rubber, is so well known as scarcely to warrant description here. The method is preserved unaltered; and despite the enormous advantages of the system used in the Orient, no one here is as yet concerned with experiments to produce a better and more profitable product.[5]

In the second division (oils and waxes) the variety of plants is considerable. Of those important enough to figure in commercial statistics the following might be cited:

Andiroba, *Carapa guianensis.*
Babassú, *Orbignia barbosiana.*
Bacaba, *Oenocarpus distichus.*
Copaíba, *Copaifera reticulata.*
Curuá, *Orbignia spectabilis.*
Inajá, *Maximiliana regia.*
Jabotí, *Erisma calcaratum.*
Jupatí, *Raphia taedigera.*
Marajá, *Parenoglyphis maraja.*
Mirití, *Mauritia flexuosa.*
Mucajá, *Acrocromia sclerocarpa.*
Murúmurú, *Astrocaryum murumuru.*
Patauá, *Jessenia bataua.*
Piquiá, *Caryocar villosum.*
Pracaxí, *Pentaclethra macroloba.*
Puxurí, *Nectandra puchiri.*[6]
Tucumã, *Astrocaryum tucuma.*
Ucuúba, *Virola surinamensis.*
Umarí, *Poraqueiba paraensis.*
Umirí, *Humiria floribunda.*
Urucurí, *Scheelea martiana.*

Many of these plants are palms, and from all, with the exception of "copaíba" in which the trunk is utilized, the oil is extracted from the fruits. This oil is used for various purposes, one being the prime material in soap and another a culinary substitute for olive oil.

The method of collecting is, in the majority of cases, the most curious imaginable. The palm fruits are fished, so to speak, out of small creeks on the islands where the plants grow. The fruits are carried along by the ebb-tide current, and the nut gatherers station themselves in canoes at strategic points

[5] This statement is no longer true in view of the important research at the Instituto Agronômico do Norte, in Belém, toward the development of perfect smoked sheets and their introduction into the Amazon production.—W.A.A.

[6] See translator's note, p. 33.

to sweep up the harvest by means of a special kind of basket.

In the resin group are: "jutaícica," *Hymenaea courbaril;* and "breu da terra," *Protium heptaphyllum.* Despite the few species figuring in the market the first named is of considerable value in the paint industry.

To describe all the trees furnishing lumber would be the task of a specialist and would fill several volumes. The more notable are:

Acapu, *Vouacapoua americana.*
Coaruba, *Vochysia vismiaefolia.*
Cupiúba, *Goupia glabra.*
Freijó, *Cordia goeldiana.*
Louro preto, *Nectandra mollis.*
Louro vermelho, *Ocotea rubra.*
Macacaúba, *Platymiscium duckei.*
Marupá, *Simaruba amara.*
Massaranduba, *Minilkara huberi.*
Pau amarelo, *Euxylophora paraensis.*
Pau santo, *Zollernia paraensis.*
Piquiá, *Caryocar villosum.*

The hardwoods of this region are characterized by a high specific gravity but for the reason already explained the filling of orders for specified amounts of any one species is very difficult.

Among the fiber plants, diligent studies have encountered types to fulfill any requirement, but only the following three native ones are sufficiently developed to supply a market which is in itself not very important:

Uacima, *Urena lobata.*
Piassava, *Leopoldinia piassaba.*
Sumaúma, *Ceiba pentandra.*

It may be noted that the "sumaúma" could be called the queen of the forest because of its gigantic size. Its product resembles the "kapok" of Malaya.

In the group of essences and perfumes there are:

Baunilha, *Vanilla fragrans.*
Cumarú, *Dipterix odorata.*
Pau rosa, *Aniba rosaeodora.*

The first is well known for the value of its perfumed pods and is the only orchid utilized for its fruits. The production is insignificant. From the "cumarú," or tonka bean, an essential oil is extracted which is used in medicine. From the wood of the "pau rosa" rosewood oil is distilled. It contains an active principle "linalol" and is used as a fixative for perfume.

In the miscellaneous group are:

"Jarina,"[7] *Phytelephas macrocarpa,* a small palm producing seeds which are called "vegetable marble." They are used to make buttons and small carved objects.

"Guaraná," *Paullinia cupana,* the fruits of which yield a substance with tonic properties, used in the preparation of a soft drink. This drink, also called "guaraná," consisting mostly of colored water, sugar, and carbonic acid, is popular in Brazil. It is sold, to some extent, abroad.

Brazil nuts, *Bertholletia excelsa,* which are well known and have great food value due to the high protein content. For this reason the name "vegetable meat" could be applied rightly to the food.

"Timbó," *Lonchocarpus utilis* and *L. urucu,* can also be included here. The plants are woody climbers, whose roots contain a poison highly toxic to cold-blooded animals but not to those of warm blood. The active principle is known as rotenone and all the advantages of a perfect insecticide are attributed to it.

All these plants, without doubt, are natural resources, but they are so widely dispersed that anyone interested in securing a certain product cannot be told where it may be found in the quantities desired. Furthermore, normal methods of harvest, as ordinarily conceived, would not avail.

In the entire Amazon Valley the flora is so intermixed that if a square kilometer (0.38 sq. mi.) of ground could be marked off only a single specimen of any one of the different kinds of trees probably might be found therein. Nevertheless, zones do exist where a single or sometimes a group of species occur in denser stands. For example, the oil palms are numerous in the region of the Islands; Brazil nut in the Middle and Upper Tocantins River; and "jarina" in the Middle Solimões. A further exception must be made for "guaraná," which is found only in the municipality of Maués and vicinity, in the State of Amazonas. For all the others dispersion is the rule; highly interesting from the botanical standpoint but very disillusioning for all other purposes.

When the time comes to initiate intensive cultivation of the various native plants of the Amazon, which in itself will be a vast

[7] See translator's note, p. 33.

program, there will have to be some means of selecting the more suitable locations. The frequency of occurrence of the plant itself will be one of the easier methods to guide the future students in the choice of the best sites. However, the principal criterion in the selection of these future sites should be easy accessibility. The harvest of a native product costs practically nothing to the worker, aside from his own maintenance. It is the transportation, not only of the things he needs but also of those he produces, which is the major factor in the cost of the article. It is obvious then that regions must be chosen which permit efficient and rapid connection with the centers of consumption and distribution, if there is to be proper economic development.

Fauna. The Amazon fauna is as varied as the flora. In addition to numerous noxious insects and reptiles, which are dreaded by the inhabitants scattered through the Valley, many animals of economic value are to be found.

So far there has been no attempt to create wildlife preserves for industrial purposes. Instead there has been a merciless and indiscriminate slaughter, which leads to the danger of exterminating certain species. It is practically impossible to enforce hunting laws in the vast hinterland where the hunters practice their profession.

The active commerce in skins, encountered in nearly all the Amazon region, is based on numerous kinds of wild animals:

The "veado vermelho" (red brocket), *Mazama americana,* is one of the animals which contribute most to the exportation. However, the most valuable is the "ariranha," *Pteronura brasiliensis,* a large otter now little seen because of the intensity with which it has been hunted. In addition to these the "caetetú" (collared peccary), *Pecari tajacu* and the "queixada" (white-lipped peccary), *Tayassu pecari;* capybara, *Hydrochoerus hydrochaeris;* "gato maracajá," *Felis wiedii;* "maracajá-assú," *Felis pardalis;* and "onça pintada," *Felis onca,* form a part of the valuable and specialized commerce with the United States. The most important reptiles are the "jacaré" (alligator), *Paleosuchus palpebrosus, Caiman niger,* and *C. sclcropa,* especially since their hides have been introduced into industry in the form of shoes. The lizards, "jucuruxí," *Dracaena guianensis,* and "jacurarú," *Tupinambis* spp., as well as the "camaleão" (chameleon), *Polychrus* spp., are also employed in this manner but to a lesser degree. Certain of the snakes, such as the "giboia," *Constrictor constrictor,* furnish their quota but now less on account of the change in fashion.

Food fishes of an enormous variety exist in the rivers and lakes. The most important is the "pirarucú," *Arapaima gigas,* very abundant in the Lower Amazon where it is caught principally during the period of July to November, the season of low water, when shoals of the fish swarm into the marginal lakes for spawning. Despite its prodigious fecundity, this largest member of the order may soon become extinct if the fishing continues at the present rate and during the least propitious time.

One of the most typical scenes of Amazon life is fishing for "pirarucú" with harpoons. A special dexterity learned from the primitive Indians, as well as a keen vision, is needed. Some specimens weigh nearly 175 pounds and measure more than 6 feet in length. They are usually salted and dried in the sun before being eaten. This curing, even yet, is done under the most primitive conditions possible. Consequently the product is not the sort to satisfy a fastidious palate, and the markets of the south are unwilling to accept it as a substitute for foreign codfish.

Apparently conditions in the fishing colonies are unsatisfactory. These groups were created after the nationalization of the industry; but nevertheless production has greatly diminished in some places and disappeared entirely in others. Water being the major element and fish so plentiful, the unimportance of the fishing industry is incredible.

Another inhabitant of the rivers, a local curiosity, is the "peixeboi" (water-cow), *Frichechus inunguis,* an aquatic mammal of considerable size. The flesh is appreciated by some and is sold fried in its own grease under the name of "mixira."

Turtles abound in great variety, the best

known being the great turtle of the Amazon, *Podocnemis expansa*. It is one of the traditional native foods. Others, lesser known, such as the "tracajá," *Podocnemis dumeriliana*, and the "mussuan," *Kinosternon scorpioides*, are greatly sought to supply the demands of gourmets.

Only a few birds have value other than purely ornamental. The delicate feathers of the "garça" (egret), *Egretta thula*, once created a large and lucrative business but only the demands of feminine fashion will determine whether the egret will again be commercially profitable. The "mutum," *Crax alector* and *C. globulosa*, large gallinaceous birds, with flesh as delicious as that of a turkey, could be an important food item if bred on a commercial scale.

A clam, fairly abundant in the Tocantins River, furnishes a shell of considerable importance in the mother-of-pearl trade. The pearls found occasionally in these shells are considered merely a curiosity.

Just as the now highly prized alligator has been considered useless, or even noxious, likewise other animals might eventually prove to have commercial value.

Formerly all these animals were easily found along the banks of the more accessible rivers, but they are becoming scarcer as they retire to the deeper recesses of the forest to escape indiscriminate slaughter by hunters who, through necessity or ignorance, give little heed to the laws for the control of such activities. A complete extermination of some valuable species can be expected in the near future unless conditions change.

The majority of skins sold in local markets comes from animals originally killed for food, in places having no other source of meat. Thus the skins figure primarily as supplementary articles of commerce. The lack of more efficient and forceful methods for the restriction of shipments and confiscation of skins of protected animals leaves much to be desired. For this reason it is urgent that the provisions not be confined to prohibiting hunting, if some of the species are to be saved.[8]

Breeding farms, similar to those elsewhere, to raise animals whose hides have commercial value should be promoted. These farms will be of economic advantage in conserving the species and in preventing losses due to poor skinning and curing. Also the meat might contribute to the local food supply.

An economic survey of the opulent Amazon fauna would be a notable work from every point of view, particularly benefiting industry and trade.

Climate. Although the Equator divides the country into two unequal parts, with the smaller lying to the north, the temperature does not have so great an influence on human life as many suppose. In fact, the mean temperature is less than that found in the same latitude in other parts of the world. A temperature curve based on observations over a long period of time indicates that the temperature rarely rises above 93° F. in the shade and that normally the thermometer fluctuates between 68° to 90° F. under the same conditions.

The atmospheric humidity, however, is of great importance, especially during the rainy period, improperly called the "winter season," which extends from December to May, being most extreme in February and March. During this time the degree of saturation is regularly above 90 percent, and even in the driest periods it is seldom less than 70 percent, the absolute minimum observed up to the present being 43 percent.

In a region cooled by the antitrade wind and winter rains, one cannot complain very much of tropical heat except during short intervening periods. The origin of various absurd generalizations about Amazon weather can probably be traced to travelers who stopped overnight in Belém on the air route between the two Americas. Although having spent only a few hours between planes in a city at the extreme end of the Amazon Valley, some of these travelers have been overheard later to classify the weather by such contradictory terms as "very hot," "very dry," "chilly," or "rainy." Impressions such as these, created by exceptional weather conditions, come to be regarded as accurate and finally appear in an article or book with all the seriousness of a profound treatise on climate. It is interesting to note how soon people staying for a while give up such preconceived ideas.

[8] See translator's note, p. 33.

Aside from the rain there is no important meteorological condition which could influence the general economy. The barometric pressure is almost constant throughout the year, and conforming to the altitude the mean readings are between 994.9 to 1,009.5 millibars (29.5 to 29.8 inches).

The scarcity or abundance of rain in a certain year, as well as the premature or delayed beginning or end of a season, seems to have great influence on the volume of commercial shipments. Apparently the only plausible explanation is the irregularity of the summer season preceding the harvesting time. This can be verified by the fact that one year of high production is often followed by another in which the volume is reduced to 20 to 40 percent.

An interesting climatic phenomenon, occurring suddenly and irregularly in the Upper and Middle Amazon, is known commonly to the people as a "friagem" (chill). It consists in a sudden lowering of the temperature, usually at night. The thermometer may fall below 50° F. for a few hours, causing shivering and chattering of teeth among the inhabitants. One explanation is that masses of cold air descend from the Andes, owing to the lowered pressure in the locality concerned. The sudden and rare hail storms reported in some places might be explained in like manner.

Data are insufficient for an accurate climatological study of the Amazon Valley, because of the comparatively few weather stations in relation to the immensity of the territory. The present service certainly will be widened to keep pace with the expansion of airways, so that there can be more complete data to explain the effect of weather on the flora and fauna.

The new science of dendrochronology (the study of weather by means of the growth rings in trees) would aid in obtaining valuable data for a climatological map of the past. Graphs made from such studies would reveal what had occurred in various zones, even where no meteorological observations have been made as yet. In the same way future variations of the weather might be forecast.

A chain of weather recording stations should be situated at strategic points to secure daily information on rainfall. To coordinate such data with records of water flow in the rivers would do much to explain the sudden, but still unexplained floods noted in some rivers. Data of this kind would be highly useful to Amazon navigation.

The climate will be no serious obstacle to the development of the Amazon, once this region is integrated with the rest of the Brazilian community on the road to a glorious future.

People. The origin of the extinct race, once inhabiting the mouth of the Amazon and a few of the tributaries, is still wrapped in deepest mystery, the only remains being the ceramics of Marajó and Santarém.

Scattered tribes of Indians in the headwaters of the higher rivers are now mostly influenced by Christianity, although a few continue to repel any approach whatever. The remnants of the tribes who used to live in the Lower Amazon are now integrated with the general social structure, and intermarriage is contributing to the dissolution and disappearance of the primitive types. It is curious to note the strongly Mongolian appearance of some of the descendants of these original inhabitants.

The Amazon being the part of the country least touched by a large-scale immigration, no great nuclei of foreigners are to be found. The Syrian and Portuguese elements, which have contributed so greatly to the commercial development of the country, are encountered only in the state capitals and larger towns, but even they are being rapidly assimilated.

The immigration trend of several years ago brought Japanese colonies to the municipality of Parintins, in the State of Amazonas, but fortunately these were broken up in time. Similar settlements in the State of Pará, in the municipalities of Monte Alegre and Acará, were somehow unsuccessful. For this reason the "yellow peril" of the Amazon is not so important as might be suspected, when we consider the publicity which has been given to the subject.

The bulk of the colonization in the Amazon was made by the northeastern Brazilians. Plagued by the rigors of droughts in their own land and attracted by the fascinating

possibilities of rubber, these people have braved the jungle and have done much to improve the country where they have made their new homes.

As opposed to the conditions in south and central Brazil where slaves were the main support for the great crops of sugar and coffee, there has never been any influence of servile labor here. Consequently very few Negroes were disseminated in the Valley to dispute the dominance of the "caboclo," descendant of the primitive Indian.

Freedom from worry about food or inclemency of the weather has created in the native a most simple view of life. Without ambitions, and with few immediate wants, he limits himself merely to enough work for maintenance from day to day, certain that existing conditions will remain continuous and uniform. This mode of life, however respectable from a philosophical point of view, is not one which favors progress, but it may after all be an intelligent way of living, free from the uncertainties of the so-called supercivilized people who are obliged periodically to blow each other to bits.

The exploitation of the native is the chief trouble resulting from his apathy. Because of the impossibility of enforcing laws to protect workers dispersed over such an immense territory, they are often subjected to a regime of work very close to slavery. Thus, only the few have enjoyed the rewards secured in this objectionable manner. They lived in the larger cities in sumptuous homes or made frequent trips to foreign countries to spend prodigally their ill-gotten wealth. Their profits were secured by double exploitation, in the worst sense, of both Nature and man. This caste of "colonels"[9] of the interior, who administered justice and who were the main prop of the corrupt and extinct regime of Brazil in 1930, fortunately has now almost disappeared. A few, however, with cunning ability still manage to survive.

Man, the major factor in any question, complicates the subject mostly by his absence from this part of the country. Actually, with a population density of much less than 1.5 per square mile, it will be difficult not only to find means within Brazil to solve this need but also to secure a foreign source of adaptable immigration which will not cause trouble in the future.

[9] The term "colonel" is applied to the river lords of the Amazon who made immense fortunes during the rubber boom of the past.—W.A.A.

All this train of interlaced situations, designated *The Problem of the Amazon*, has been created by the improvidence of the people themselves. In seeking the riches yielded by Nature they have not penetrated more than a few miles into the forest and thus have brought about the multitude of tiny and widely dispersed villages along the banks of the rivers. However, full admiration must be rendered to the courage and pertinacity of those who have been confronting all obstacles to maintain themselves in localities where in reality civilization has not yet touched. They are Brazilians worthy of the name, and they represent the national sovereignty over this immense and coveted territory.

Because of the isolation from the rest of the country, the Amazon region still conserves habits and customs inherited from the primitive inhabitants. The folklore is rich and serves to show the dominance of Nature over man, which is exercised in an almost absolute manner. The introduction of modern, progressive ideas is retarded somewhat by numerous superstitions and taboos, but these can be overcome by the adoption of intelligent and persuasive methods of teaching, owing to the docile and comprehending spirit of the people.

Although small of stature the native of the region is generally rather robust, except of course, those born in the larger towns, who are conditioned to other ways of living. Sobriety and simplicity of diet, as well as close contact with Nature, are some of the factors explaining his good physique. From this can be foreseen what the individual of the Amazon will be once he is educated, properly fed, and freed from the endemic diseases which plague him. The human element of the region should not be underrated or looked upon with pessimism, because in comparison with peoples in the same latitude of other countries there is none who presents better or even equal characteristics, especially when the conditions of the Amazon are taken into consideration. Instead of

criticism, no matter how constructive, it would be better at present to have some sort of decisive action to remedy the situation.

The paralysis of progress in the Amazon has greatly limited the opportunities for any individual with a desire to improve his station in life. It has obliged and is still obliging him to migrate to the south in search of a better and more stable future. Annually great numbers of people drift to the capital of the Republic, attracted by the fascinations of the "Marvelous City," and end by filling the already overflowing pavements of the avenues. Even those called away for military service finally prefer not to return to the Amazon with its ever-diminishing population. The fact that this exodus consists of males can explain the smaller marriage rate and the considerable reduction in inhabitants as revealed by the last census.

Although there are many who extol a march to the west, there are few who practice it, and so the only means of securing personnel for the works contemplated for the Amazon will be to utilize to the best advantage the scanty resources of the existing manpower.[10]

Whatever may be the effort asked of man in the Amazon for the realization of that which he most desires, consciously or not, it is certain that he will respond with the greatest willingness, even to sacrifice, once he is convinced of the elevated intentions and the sincerity of those who are to be charged with the resurrection of the great territory where he was born.

Culture. The status of popular education in relation to the national panorama is well known, and although it is true that the degree of illiteracy diminishes year by year yet the percentage still remaining is serious. The Amazon, being an integral part of the Nation, cannot be considered as different in this respect, especially when specific difficulties are taken into consideration. The state capitals and large towns have done much to advance education. The statistics of matriculation and attendance at public schools, as well as the laws for public education which are being formulated by the various states, are proof of the forces being made to improve the cultural aspect of the local populace. Considering the widely dispersed settlements and the meagre funds available to the municipal administrations, we can readily understand the difficulties of raising the educational level of the Amazon people.

The shortage of teachers to fill positions in the interior is one of the difficulties confronting the officials in this branch of public administration. There is no scarcity of capable personnel, which, on the contrary, exists in an appreciable degree among the yearly graduates from the normal schools. What actually happens is their refusal to accept positions in interior towns. Although this reveals an inadequate spirit of sacrifice for the good of the country, yet it is understandable when the refusal is made by a young woman who would have little or no opportunity for marriage were she assigned to some remote place. She would have no civilized comforts and often would be financially unable to bring along members of her family.

This presents a general picture in so far as primary education is concerned. Technical or professional preparation does not exist in the interior and is very limited even in the State capitals, owing to the few institutions qualified to give instruction of this kind. Another contributing factor is that the great majority of young people are unable to attend such schools because they have had to begin working after completing the secondary schools to help support their families.

Apprenticeship to the professions is made by a long-continued service, during which are acquired all the faults and defects of the trade. For this reason there is no good handicraft, and the workmanship of labor is of the lowest quality. The absence of large industries is one cause of this situation, and furthermore the industries have not developed for lack of skilled workers, thus forming a vicious circle not easy to change by ordinary methods.

Higher education lacks neither schools nor students, but there is a great paucity of positions where the graduates may later practice their professions. Every year a new group

[10] Recent activities to bring in new labor from northeastern states have not been as successful as hoped, but a solution of the problem is still being sought.—W.A.A.

of doctors and bachelors of arts appear who are forced to seek government positions once they discover their limited opportunities in other fields. The same is true of the school teachers, all of them preferring to live in the state capitals where there is already a superabundance of colleges with permanent staffs, so that success is a matter of luck or persistence.

An exception to the rule is found in the engineering school, because its graduates find immediate employment despite the somewhat theoretical teaching received. Aside from this exception it is true that there is a shortage of technically trained people to bring advancement to the country. Even so, if all the students continue to pursue the same specialty—civil engineering—then in a short while saturation will leave them in the same predicament as their colleagues, the doctors and lawyers.

There are courses in agronomy[11] and veterinary, but unfortunately the graduates from these have no opportunities to practice their profession independently. Rare is the landowner who seeks the services of a specialist for advice on crops or cattle improvement. These graduates are forced to seek refuge in public service if they wish to follow their specialty. Since even this opening is limited, discouragement results with consequent abandoning of the career.

In chemistry, where the possibilities are infinite and employment certain for those who actually know the subject, not a single course is being offered at the present time, despite the brilliant record of the past in Belém. The School of Industrial Chemistry, sponsored by the Commercial Association and directed by that great Amazonist Paul Le-Cointe, unfortunately is now closed after having turned out a handful of competent professionals, thus cutting short a fine and useful work for the region. All attempts to reopen the course have been frustrated by bureaucratic complications of the educational boards, which, being situated in the Federal Capital, are absolutely uninformed on the actual conditions of the Amazon. No one who really knows the Amazon could admit the possibility of applying here certain of the laws and regulations which were formulated for the entire Nation.

The dearth of technically trained men to serve in the obligatory orientation of producers leaves the latter free to continue the primitive methods of exploiting the soil. Such trained men can be obtained only from schools specializing in practical instruction without emphasis on general cultural subjects. The granting of degrees should be postponed to some future time in those institutions concerned with the professional preparation of the individual. The main thought, therefore, should be to prepare the student genuinely to fulfill the role for which he is intended.[12]

People who can fill technical positions in the interior will not be recruited in the larger towns; instead it will be necessary to utilize young people of the same locality where the future work is to be carried on. Only this will assure a useful corps of collaborators.

It is necessary to point out, excluding of course the already too well-known unscrupulous politicians and opportunists, that a highly developed national sentiment is to be found in the Amazon, even among the most untutored. It is not always easily recognized but becomes evident in appropriate occasions.

Civic spirit has been manifested throughout our history by such heroic episodes as Placido de Castro, in the Territory of Acre, and Veiga Cabral in Amapá, when the threat to national sovereignty was met with prompt defense without need of stimulation or promise of reward. Much can be expected still of these anonymous makers of the Nation, everything depending upon the method by which they shall be trained, and upon the confidence engendered by those who are to guide their destinies. At present this confidence is at low ebb in reaction to past oppressions.

11 Not now true because the small school of agriculture in Belém has been closed for some time.—W.A.A.

12 The inference is that the present system produces graduates who consider themselves to be "white-collar" workers qualified only for executive or office duties. It would be beneath their dignity to soil their clothes or to engage in arduous manual demonstrations.—W.A.A.

(To be Concluded)

MAPPING SOME EFFECTS OF SCIENCE ON HUMAN RELATIONS[1]

By S. W. BOGGS

CHIEF, DIVISION OF GEOGRAPHY AND CARTOGRAPHY, DEPARTMENT OF STATE

THE earth has changed but little since Man appeared, but the geography of human relationships has been transformed in a few decades. Because science knows no frontiers, scientists perhaps tend to overlook the remarkably uneven geographic distribution of the effects of their work.

The popular picture of a rapidly shrinking globe, based on the reduction of time required in circumnavigating the earth, is inaccurate and unfortunate. The world has not shrunk as if two thousand million microscopic ants had been banished from a pumpkin to live on a cherry. For the individual and for all types of corporate society the range of activity and experience and the resources at the command of the individual and society have expanded astronomically. But the effects are distributed very unequally over the earth's surface; the geographic distribution is shifting rapidly and will apparently continue to undergo great changes. The present picture therefore gives no adequate concept of what the future may be like. It is as if the outlines of continents were picture frames within which appeared ever-changing motion pictures, like montage effects in the cinema news reels.

Little has been done by geographers and others to map these phenomena. Any maps that might be devised to portray them would be as definitely dated as the constantly changing political maps of the world. A chronological series of such maps, however, would constitute a slow-motion study, and perhaps would reveal or clarify important historical trends. Intelligent men instead of struggling vainly against the tide of history—now more like a cataclysmic tidal wave—might adapt themselves to making use of its power.

It would not be necessary to go back much farther than the year 1790 or 1800 for perspective. Tool steel and machine tools, which date from about 1770, began to make possible the utilization of scientific discoveries. The period is likewise significant because of the birth in the Americas of an infant republic and the spread in Europe of the ideas of the French Revolution, while in China that period coincides with about the maximum extent of the Manchu empire.

Maps are advantageous for the presentation of data of this character because they can show graphically the location and the extent of change, and cannot evade areas and subject matter as dexterously as text can. Maps, however, require accompanying text to reveal significant points which might otherwise be noted by very few map-users. For most of these maps colors and atlas-quality reproduction on fine paper, like those for the best physical and political maps, are required. The accompanying cartograms in black and white merely suggest a few of the possibilities discussed below.

In 1700 the distribution of available energy was practically uniform over the land surface of the globe, since man depended chiefly upon his own muscles, slaves, and domestic animals. But the multiplication of physical energy utilized by mankind, which is basic to all technological development, has resulted in an extremely uneven distribution of power utilized today. A lump of coal weighing about one pound now performs as much physical work as a hard-working man in an eight-hour day; and one miner can mine several tons of coal a day. The present diversity in levels of living is due largely to differences in the quantities of energy use per capita by various peoples for productive purposes. The map (Figure 1) reflects the situation in 1937. The changes within the last quarter century have been great, and they may be as great or even greater in the next twenty-five years.

[1] Paper read before the American Association for the Advancement of Science, Section L (History and Philosophy of Science) at Cleveland, Ohio, September 13, 1944.

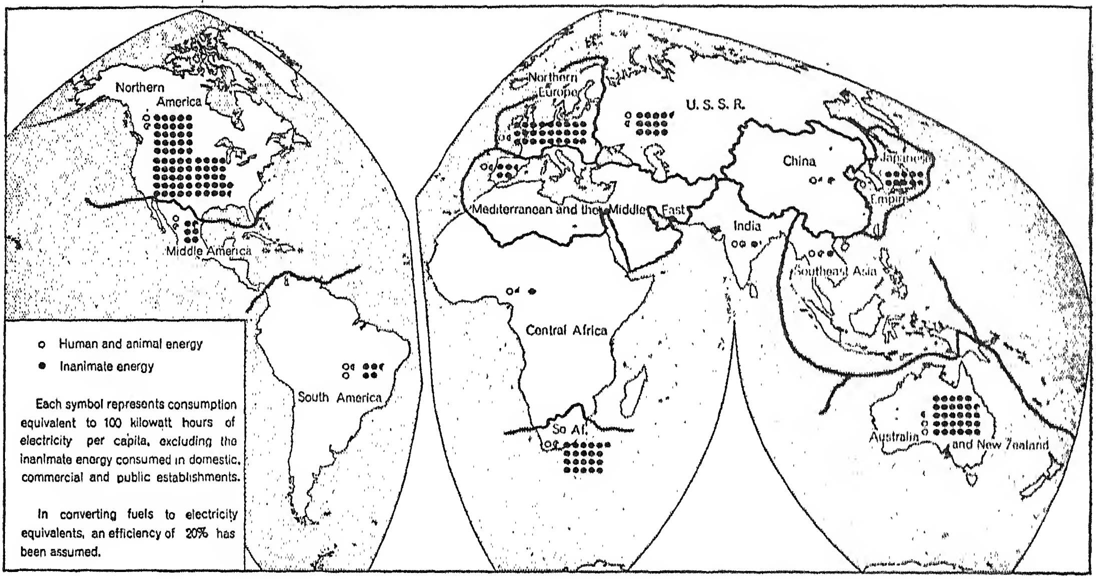

FIG. 1. ENERGY CONSUMED PER CAPITA FOR PRODUCTIVE PURPOSES IN 1937

Technological changes resulting in economical mass production and revolutionary developments in transportation and communication have produced two significant and closely related results: (1) Man's relation to his local environment has been radically altered; and (2) human relationships have been transformed on a global scale. Men can go farther, bring more back home, utilize more raw materials, and do much more with what they get than even the scientifically-minded and far-seeing Benjamin Franklin and Thomas Jefferson could imagine.

Available transportation maps usually show only the principal facilities, with little attempt to present significant differences in cost per ton-mile of freight movement. On a map centered at St. Louis, Missouri (Figure 2), as of the year 1804, equal-rate distances by different means of transport present a very simple pattern, with long fingers following the rivers, six or seven times longer downstream than upstream, and extremely slender because of the high cost of land transport in terms of human and animal effort.

The relative efficiency of land and sea transport prior to 1800 is illustrated by the fact that coal had been mined in Wales since Elizabethan times only where the sea actually cut into the coalfield. Cardiff, only six miles from the nearest coalfields by land, imported coal from Tenby and other ports to the west. An official customs report in 1775 stated that no coal was exported from Cardiff, "nor ever can be, its distance from the water rendering it too expensive for any such sale." Such are the hazards of prophecy in a world of changing technology. Indeed, as a supplement to navigable rivers, canals provided the only cheap inland transport, when they could be dug by the simple means then available.

On the map centered on St. Louis today the contrast in the cost of freight traffic with that of about 1800 reveals great expansion in all directions, notably where railroads and motor roads rival the lower-rate river transport; present river rates, however, are based on a certain percentage of railroad rates,[2] so that the down-river distance for a given price is now less than it was nearly a century and a half ago.

Figure 3 is a cartogram intended to give

[2] The fact that a ton of freight may be moved one mile by railroad, as in the United States, for about 1¢, or the wage of an unskilled laborer for one minute, is the significant fact in land transport. The rail tariff on wheat, Omaha to Buffalo (1,001 miles), is 8.9 mills per ton-mile; on the Great Lakes, Duluth to Buffalo (986 miles) the rate is 2.0 mills per ton-mile, which covers a toll-charge through the Locks.

a visual impression of the comparative efficiency of the principal means of transport. A steamship will usually carry a ton of freight eight or ten times as far as a railroad, for a given sum of money, and from one hundred to several thousand times as far as human porters or pack animals. The bars in the diagram indicate in a general way how far a ton of bulk freight, such as wheat, can be transported for a sum approximately equal to the daily wage of a human porter in regions which lack railroads and motor roads. The maps in different scales are so proportioned in size, very roughly to be sure, that equal distances on all maps represent equal cost in terms of human effort. The map scales are therefore the reciprocals of the mean value of the bars in the diagram.

This cartogram in black and white is incidental to the preparation of a world map in color, not yet published, which constitutes an attempt to show the approximate cost per ton-mile for freight movement in all parts of the world today. Such a map brings out the areas in which surface transport is possible only on men's backs or heads, or on pack animals, or by means of animals pulling carts on rough roads. Here the cost factor of primitive transport is represented graphically in the legend by a very steep slope, and one may imagine porters or pack animals toiling up these symbolic but very real slopes until they become exhausted. People in these regions are walled in by high transport costs. Railroads, with a cost factor per mile like the gradual upward slope of a smooth coastal plain, cut through the areas of high primitive costs like a great river which has incised its channel through a mountain range in past geologic ages.

Such equal-cost-distance maps may be called "isotimal," from the Greek word *isótimos* meaning "equal cost or effort." In

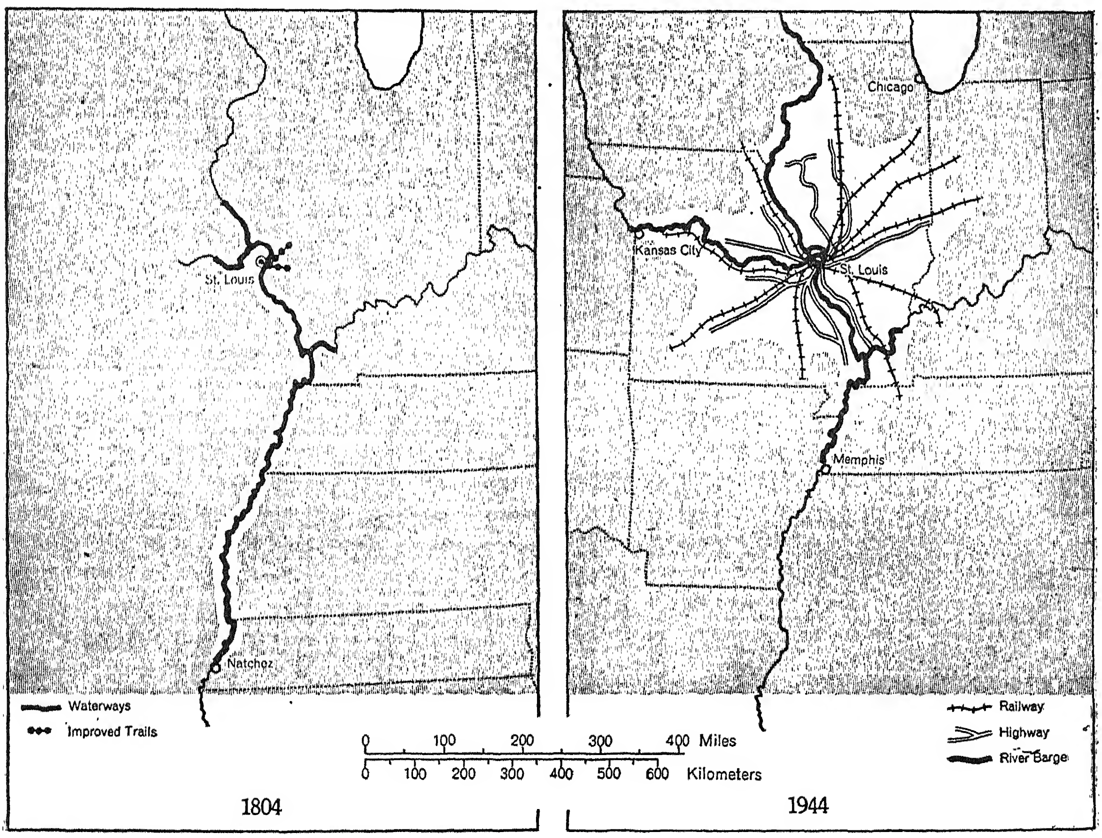

FIG. 2. EQUAL-RATE DISTANCES FROM ST. LOUIS, MO., 1804 AND 1944

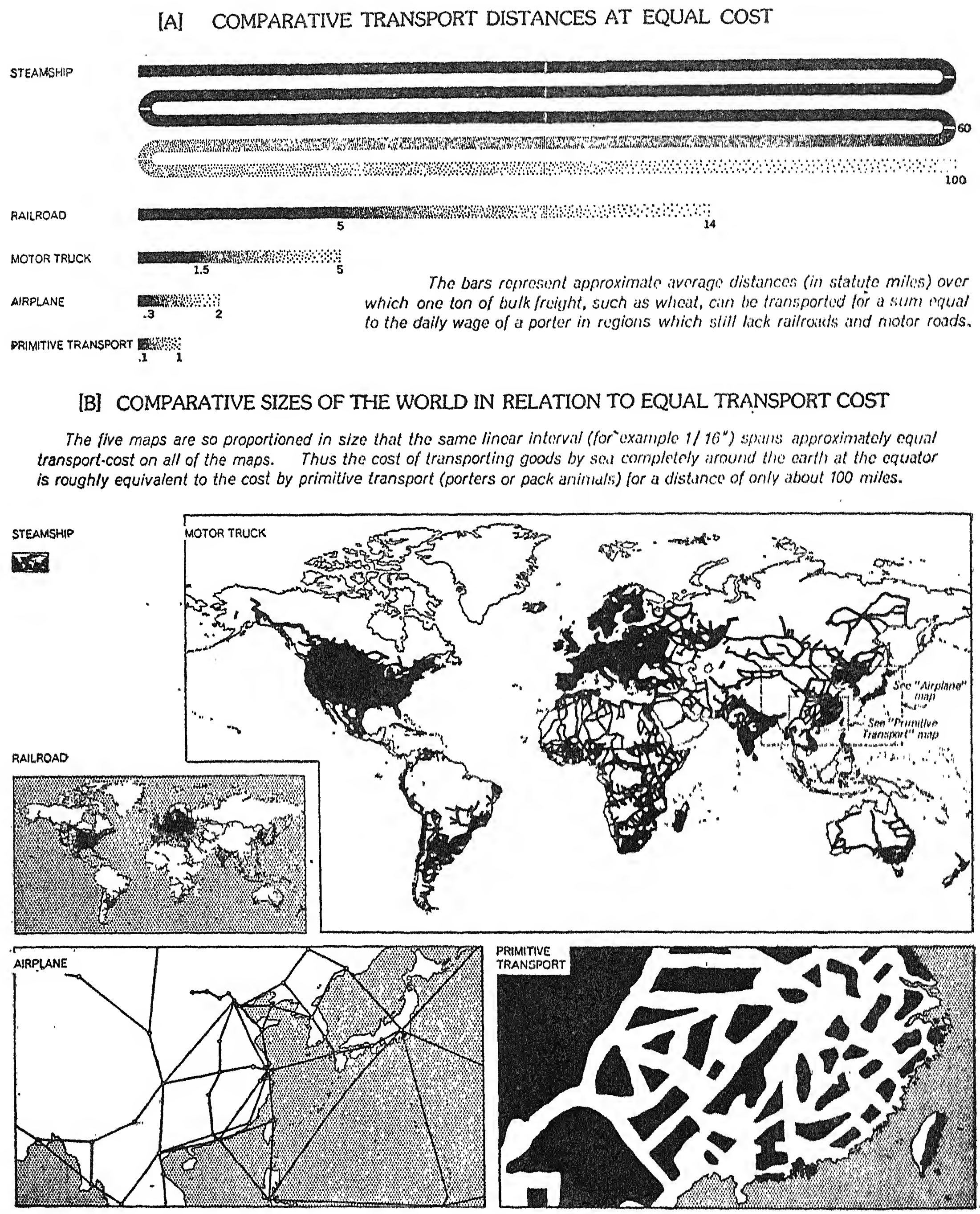

FIG. 3. TRANSPORT BY DIFFERENT MEANS AT EQUAL COST

compiling a map of this type, one would like to get back of the complicated rate structures of railroads, motor truck carriers, river and ocean shipping, and measure cost in units of human effort. The march of physical progress could be recorded largely in a chronological series of such maps.

This access to distance, due to cutting the

cost with mechanized transport, largely accounts for contrasts such as that portrayed by the world maps of wheat production and commerce for 1800 and today. In 1800 the farmer who raised wheat did not dream of selling his product more than a few miles from home, where he could haul it by team and wagon or send it a little farther by river or sea. The human use of the grasslands has been revolutionized by the railroads, the breeding of new wheat strains, the invention of roller milling and other machinery, and the opening of European markets since the industrial revolution. Consequently, wheat grown in four continents today competes in a fifth. Comparison of a map showing both the areas in which wheat was produced and consumed in 1800 with a similar map for today reveals the intimate relations between the railroad net and the areas in which wheat growing has been greatly extended in nature's grasslands.

Maps of many new types may be prepared to depict the geographic distribution of the effects of science and technology upon human relations.[3] Among them might be maps showing the following:

(a) For a given place and several dates, the percentage of goods used in that place or region which came from distances of 10, 100, 1,000, 5,000, or 10,000 miles, thus providing some measure of expanding interrelationships.

(b) For any product for which there is now a world market, the historical geography of production and distribution.

(c) Decreases in cost of production per unit of output, by region and date.

(d) Travel speeds, by regions, for various dates.

(e) Communication costs and volumes of communications, by region and date.

(f) Geography of aviation development—factors conditioning the establishment and operation of air services.

(g) Cultural relationships between different regions.

(h) Levels of living, based on various yardsticks.

(i) Social results of medical science.

(j) The principal bases of prestige in various countries or regions, upon which concepts of success and leadership depend, some of them having been modified in recent decades by the development of certain industries.

One of the principal generalizations of geography is that there is very uneven, one may almost say very lop-sided, distribution of the earth's resources, of land and sea, climatic zones, productive soils, population, and other factors. The influences of these inequalities of geographic distribution are very different from what they were fifteen decades ago—even five decades ago. Some may naïvely imagine that the effects of this uneven distribution have been practically obliterated. On the contrary, they have simply been given new values, and some of them are more significant than ever. Just as the geographical factors have by no means been eliminated in war, in these days of mechanized warfare and of airplanes, so their influence in peace is constantly changing and is as yet inadequately appreciated. In terms of past experience it is as if we were living at the same time on several worlds whose differences in size were of almost astronomic proportions.

However great may be future changes in world maps showing the distribution of population, transportation and communication facilities, exploitation of minerals, and the like, the pattern appears to be already well developed. The abstract pattern of relationship possibilities, moreover, is not likely to change as much as it has already changed within the last century. In at least one direction the ultimate has already been attained. Communication is almost instan-

[3] Maps of the world presenting data very objectively and impartially are most needed. To the people of this or any other country they would afford assistance in understanding the viewpoints of peoples whose historical backgrounds and environments differ greatly. It may be remarked that one of the most notable atlases in recent years is the Great Soviet World Atlas, projected in three volumes, the first of which, published in 1937, was devoted chiefly to world maps of great variety. Presumably an even greater contribution to world understanding could be made if such a map series included more maps specifically designed to show when, where, and how great have been some of the changes in human relationships between regions during the last century or more.

taneous, with the speed of light, and may reach all points of the globe at once; it is being extended through television and the use of many electronic devices.

In the days of both Nebuchadnezzar and Napoleon the fastest travel was at the rate of a fraction of one percent of the velocity of sound, whereas today it rapidly approaches the speed of sound, but presumably can never attain a speed many times that of sound. The efficiency of the railroad might conceivably be doubled or quadrupled, but presumably no method of land transport can be devised which will reduce the cost to a level of that of the most efficient ocean freighter. One factory machine may now perform the labor of 10,000 human beings working by hand, but even if a new machine is invented which will produce as much as one hundred machines do now, the order of change will be less than that which has already occurred. The wizardry of chemistry already unites rare materials from the ends of the earth so that men who produce tungsten in Kiangsi Province, China, are closer as economic neighbors in normal times to Pittsburgh, the Ruhr, and the British Midlands than to communities in China one hundred miles distant.

Scientists will doubtless produce marvels far beyond our present conceptions. Their insatiable curiosity is now penetrating fields of invisible and astonishing forces; they operate without fear and in a spirit of humility before facts which enables them to discard outworn hypotheses and to learn new ways very rapidly. The changes to come in many regions hitherto referred to as "backward" may greatly exceed those already manifest in areas in which changes have been greatest in recent decades. The maps of human activities and relationships will doubtless pass through rapid metamorphoses in the near future.

Flat maps cannot effectively reveal relationships of air travel and transport and of radio. While many types of aviation and telecommunication maps should be made, special globes and accessories are almost essential.

Man has a fondness for circulating which accounts for some of his problems of relationships. Circulation is the rule in nature, of the air itself, of the sea, many birds, and some animals. Man's new facility of movement enables him to circulate with freedom equal to nature in its freest moods.

People everywhere, even in remote places, are thereby being stimulated through contacts by radio, the press, the airplane, the marketplace. Human friction and heat may thus be generated. But to try to build a sort of wall to exclude contact, instead of to become adapted to it, is futile—a crustacean psychosis in an avian age.

The amazing discoveries of scientists and the resourcefulness of engineers and technologists afford assurance that men's needs on the physical level can be met. The most difficult and important problems for the future which have stemmed from scientists' laboratories are the problems of human relationships, which have been multiplied almost beyond conception. Institutions with adequate resources, young men and women whose understanding of the world in the last few years has been broadened and deepened, may, by using geographers' techniques in the cartographic interpretation of spatial relations, provide us with maps that will carry us a long way toward a sound understanding of the world in which we now live.

INSECT RESPONSE TO COLORS

By HARRY B. WEISS

CHIEF, BUREAU OF PLANT INDUSTRY, NEW JERSEY DEPARTMENT OF AGRICULTURE

We know nothing of the sensations that insects experience when they see either colored or white light, and hence it was thought best to entitle the article as above. In this way any intimation that insects have color perception, such as we know it, is avoided. In the economy of insects, light is of extreme importance, as is indicated by the size and development of their compound eyes that have large receptive surfaces in comparison with the volume of the comparatively less-developed central nervous system.

The response of insects to various wave lengths of light, or colors, has been under investigation for many years. The early conclusions drawn from various experiments have been the subject of disagreement, particularly because of the difficulty in determining whether the apparent discrimination of colors by the insects was due to the intensity of the rays or to their wave lengths. The problem has been attacked by entomologists principally in two ways. Some observers noted the natural tropisms of insects, while others trained certain species to go to particular colors for food. Frequently, little or no attention was paid to the external source and mixture of wave lengths used in the experiments, and the results were described in terms of human color vision. However, a few investigators worked with known wave lengths in order to determine whether insects have color sensitivity and not merely sensitivity to light depending on intensity.

Sir John Lubbock established the fact that bees were apparently able to distinguish one color from another and could be trained to associate the finding of food with blue- or orange-colored papers. Auguste Forel accomplished the same thing with colored paper flowers. C. Hess projected a spectrum on a parallel-sided glass container that held imprisoned insects and observed that caterpillars and adults of the butterfly *Vanessa urticae,* and also bees, went to the yellow-green area, and that when disturbed from these areas returned to them. From these observations Hess concluded that, since totally color-blind persons see yellow-green as the brightest part of the spectrum, his insects were also totally color-blind. Hess also selected grays, the brightness of which matched the brightness of given colors, and found that when his insects were given a choice between a gray and a given color, both of equal brightness, they went to either one without discrimination or did not react at all, and from this he concluded that various colors are of the same relative brightness to insects as they are to totally color-blind persons and that insects lack color vision.

K. Frisch trained an Asiatic species of honeybee to come to a given color for food and to pick out that color from among others when no food was present. He used a series of pieces of paper of 15 shades, from white through various grays to black. The bees were then conditioned to some color, as blue, by associating that color with a sugar solution in a watch glass that was placed over the color. After conditioning of the bees was completed the blue paper, without food, was placed in various positions among the grays, and in every trial the bees gathered over the blue in search of food. After conditioning his bees to various colors Frisch concluded that bees could distinguish all colors except red and certain greens, and that these colors appeared to them as darker or lighter grays, and that, therefore, their color vision was identical with that of partially color-blind persons.

Various other workers, studying phototropisms, found that some insects preferred dark blue, others red and yellow, others green, etc., and their conclusions, agreeing with those of Hess and Frisch, were expressed in terms of human color vision, giving rise for a time to a state of confusion. In many cases no tests were made with ultraviolet, to which insects react, but which is

beyond the range of human vision. In addition, some investigators covered their colored papers with glass. Light going through the glass was reflected back again, and the glass also reflected colors from near-by objects. Frank E. Lutz tested the colored papers used by Frisch and found that some of his greens and blues reflected ultraviolet, that his yellows and greens reflected blue and red, all of which invalidated Frisch's color scale for insects.

Little is known of the wave lengths with which the early investigators worked. In fact, many of them paid no attention to this aspect of the problem and interpreted the behavior of the insects in terms of human color vision, a practice which persists to some extent even to the present day. In 1881 Sir John Lubbock found that ants carried their larvae and pupae out of a region of relatively weak ultraviolet into one illuminated by longer wave lengths, and from this he concluded that colors as seen by insects were not the same as colors viewed by man. Later workers found that many other insects were photopositive to ultraviolet. Hess thought that the general sensitivity of arthropods to ultraviolet radiation was due to a fluorescence of the outer parts of the ommatidia, believing that the short waves were thereby changed to longer ones and that the animals did not have true ultraviolet vision. However, F. E. Lutz and E. N. Grisewood, using the cornea of a large flesh fly and of a honeybee, found that they were transparent to 2537 Angstrom wave-length units (A) and that such a wave length reached the inner elements of the eye. When the 2537 A beam was allowed to fall upon a mass of crushed Drosophila eyes there was still no fluorescence, at least nothing visible to humans. As time went on an increasing amount of attention was paid to the fact that color vision was a matter of wave lengths and a decided improvement took place in techniques and testing equipment. And ultraviolet was considered as a color, at least to insects. Most of the investigations with insects to date have been concerned with that portion of the radiant-energy spectrum from about 2500 A to 7200 A.

A. Kühn and R. Pohl trained honeybees to come for food in a narrow trough illuminated by ultraviolet of wave length 3650 A. After training, the food was removed and the entire spectrum was projected upon a sheet of white paper. Then the bees collected for the most part on the place subjected to the wave-length 3650 A. Frank E. Lutz also trained bees to come for food to a white card, reflecting ultraviolet wave lengths, and stingless bees (Trigona) to distinguish between ultraviolet patterns, one of which marked the site of their nest. L. M. Bertholf, in an extensive study of the reactions of the honeybee to the spectrum visible to us, found that for this insect the spectrum extended from 4310 A to at least 6770 A, the point of maximal stimulative effect being at about 5530 A. In terms of color, this means that the sensitivity of bees to light extends from violet to red, with the most stimulating part being the yellow-green. He also worked with different wave lengths in the ultraviolet spectrum and found that the stimulating effect was greatest at 3650 A for the honeybee.

In a study of the extent to which flower-visiting insects see ultraviolet, Lutz carried out tests in the field with numerous flower-visiting species. He put the insects into a rectangular box having a window at each end, which consisted of light filters that were interchangeable. He then put his head through an opening in the middle of the window, surrounded by a light-tight hood, and observed what was going on. Many of his test insects went from dark to clear glass, from dark to ultraviolet, from dark to blue, from blue to ultraviolet, from dark to green, from green to ultraviolet, from dark to red, and from red to ultraviolet, but when at ultraviolet they would not leave to go to clear glass. The conclusion is plain that many insects react positively to red, yellow, green, blue, and ultraviolet, and that they "see" ultraviolet better than other radiation wave lengths. Whether they "see" these radiations as colors or as different degrees of brightness is unknown.

During recent years thousands of insects, including larvae, involving over 50 species in several orders, were tested in lots of 100 or more in the laboratory by Harry B. Weiss,

Frank A. Soraci, and E. E. McCoy, Jr. Their insects were exposed to 10 wave-length bands of light of equal physical intensities, extending from 3650 A to 7200 A. The group behavior of these insects indicated that their peak sensitivity took place under the stimulation of ultraviolet at 3650 A. It then declined sharply toward the longer wave lengths, frequently with a secondary peak occurring from 4920 A to 5150 A (blue-blue-green). The remaining longer wave lengths to 7200 A were relatively unattractive to them. These investigators also found that wave lengths in the red end of the spectrum could be made as attractive as ultraviolet by increasing their intensities. Bertholf obtained similar qualitative results when he investigated the relative efficiency of different regions of the spectrum in stimulating Drosophila. He found that, beginning with the longer wave lengths, the efficiency was very low, but started to rise at about 5750 A (yellow) and increased to a maximum in the visible spectrum at 4870 A (blue-green). From there it declined to 4250 A (violet) and then rose to a peak at 3650 A (ultraviolet), which was five and one-half times higher than the first peak at 4870 A. From 3650 A there was a rapid decline to 2300 A. Various other investigators, including W. Sander and J. W. M. Cameron, found that their test insects were more strongly activated by ultraviolet (3650 A) than by any other wave length of equal energy. And the work of various economic entomologists, although devoted mainly to evaluating the comparative effectiveness of light-traps, has furnished additional evidence of the stimulating power of the shorter wave lengths and of the effectiveness of longer wave lengths under increased intensities.

There has been a great diversity in the type of equipment and in methods of persons investigating color responses of insects. The physical intensities of the wave lengths have been equalized by different methods. Sometimes they were unequal and the effect of equal energy was arrived at by calculation. The test wave-length bands varied from narrow to broad. The transmission characteristics of the color filters were either definitely or indefinitely known and the test insects were used either singly or in numbers. In spite of such variations, the test insects, on the whole, behaved with a remarkable degree of uniformity, and the following conclusions that appear to be true for insects in general may be drawn.

Many insects are sensitive in varying degrees to a spectrum extending from about 3600 A (ultraviolet) to about 7200 A (red). Drosophila is photopositive to 2537 A and the honeybee to at least 2970 A. Other insects may be equally sensitive to such wave lengths in the ultraviolet, but tests have not yet been made. Under the influence of equal intensities but different wave lengths, the most stimulating part of the spectrum is ultraviolet at 3650 A. From here it declines sharply to about 4640 A (violet-blue). Then it rises to a secondary peak near either 4920 A (blue-green) or 5150 A (green). Then it drops to 5750 A and levels off. Regardless of the relative positions of the wave-length bands, the group behavior pattern does not vary much from the pattern just stated. It has also been found that many insects will respond positively and in greater numbers to the shorter of any two wave lengths (between 3650 A and 7000 A) that are offered to them, where the physical intensities of both are equal and provided that the intensity of neither is great enough to repel them. Finally, what are unattractive wave lengths under equalized physical intensities may be made attractive by increasing their intensities. In other words, it is possible to vary the behavior pattern by varying intensities.

Up to the present, only the motor response of insects to equalized wave lengths has been considered, but substantially the same type of behavior is exhibited when the electrical responses of the compound eye during the process of photoreception in insects are studied. Such studies have been made by Louis Theodore Jahn and Frederick Crescitelli. According to these investigators, "Leads were taken with silver-silver chloride electrodes from fluid-filled chambers about each eye. The entire surface of one eye was illuminated and the other eye was kept in darkness. Records were obtained by means of a cathode ray oscillograph. For the ex-

periments on colored light, Corning color filters were placed between the light source and the eye." Six wave-length bands were employed, extending from about 4000 A to 7000 A. "The relative intensity transmitted through each of these six filter combinations was determined by means of a thermopile and galvanometer. The infrared radiations were completely removed from the stimulating light by using 5 centimeters of water and a Corning (AKLO) heat absorbing filter."

Jahn and Crescitelli also studied the change in form of the electrograms of the grasshopper eye under variations in intensity of the stimulating light and the quantitative aspects of the response in relation to the quality of the stimulating light. They found that there was apparently no specific effect of wave length on the electrical response of the whole dark-adapted grasshopper eye. At equalized intensities there were decided differences in wave form with the six different spectral bands, but these disappeared and the color responses were exactly matched when the intensities of the different spectral regions were properly adjusted. Quoting again from their paper: "The form of the electrical response of the dark-adapted grasshopper eye to brief stimulation by white or colored light varies according to the intensity of the light. At very low intensities the response is diphasic, the initial positive phase of which resembles the a-wave of the vertebrate electroretinogram. As the intensity is increased the positive phase decreases and changes its position while the negative phase becomes increasingly prominent. Eventually the positive phase is completely eliminated and the electrogram takes the form of the typical high-intensity response. The order of effectiveness of the different colors in causing this change in wave form is: green, blue, violet, orange-red, red."

The curve relating the magnitude of the potential to the wave length had a peak in the green region of the spectrum, and declined sharply toward the red and less sharply toward the violet. The magnitude of the electrical response was found to be definitely related to the quality of the stimulating light and the form of the response to be influenced by the intensity of the stimulating light, either white or colored.

Jahn and Crescitelli also studied, in the same manner, the electrical responses of the compound eye of the moth *Samia cecropia* in relation to the quality and intensity of the stimulating light. Part of their conclusions are as follows: "The electrical responses of the moth and grasshopper eyes to wave length are surprisingly similar. For both animals the same type of graph is obtained when the relative magnitude of the potential is plotted against wave length. This graph has a general similarity to the absorption curve of visual purple. Another aspect of the electrical response to wave length concerns the fact that no specific effects of wave length on the electrograms are discernible. By properly adjusting the intensity, the responses to one color may be exactly matched with the response to any other color, indicating that the differences in the responses to different colors of equal intensity are caused merely by differences in sensitivity and are not effects of wave length *per se*." In the case of the moth the maximum response was obtained with the green band. The responses dropped sharply toward the red band and less sharply toward the violet.

Although the king crab, *Limulus polyphemus*, is not an insect, the work of H. K. Hartline and C. H. Graham on the nerve impulses and responses of single visual sense cells to light in the eye of this animal is of unusual interest. The lateral faceted eye of the king crab contains about 300 large ommatidia, and the optic nerve fibers come directly from the receptor cells with no intervening neurones. These authors studied the nerve impulses and developed a technique by which they recorded the discharge from a single receptor unit in the form of oscillograms, representing the potential changes between the cut end and an uninjured portion of the nerve upon stimulation of the eye by light. The electrical activity in the optic nerve brought about by this stimulation was amplified by a vacuum tube and recorded by an oscillograph. Among other things, the stimulation of a single ommatidium resulted in a small strand of the

optic nerve showing a regular sequence of nerve impulses. "The discharge in a single fiber begins after a short latent period at a high frequency, which has been found to be as high as 130 per second. The frequency falls rapidly at first, and finally approaches a steady value which is maintained for the duration of illumination."

These authors also studied the responses of single visual sense cells to visible light of different wave lengths by means of single fiber preparations from a *Limulus* eye. It was found that when the energy of the stimulating light of different wave lengths was approximately equal, the response to green was stronger than the responses to either violet or red. When the energy was increased in the red and violet, their level of response was raised, and when the intensities for the different wave lengths were adjusted so that the responses were equal, there was no effect of wave length as such, indicating that single sense cells can gauge brightness but cannot distinguish wave lengths. The relative energies of the various wave lengths required to produce the same response after being adjusted in inverse ratio to the degree to which they are absorbed yielded a visibility curve, for a single visual sense cell, that had its maximum in the green near 5200 A and that declined symmetrically on each side to low values in the violet near 4400 A and in the red near 6400 A. According to the interpretation of visibility curves by Hecht and Williams, the stimulation of a single visual sense cell by light depends upon the absorption spectrum of the primary photosensitive substance. The absorption of light by this substance varies with wave length, and the production of a given response needs a certain amount of photochemical change, which in turn requires the absorption of a definite amount of energy.

Hartline and Graham also found that in the same eye of *Limulus* there was a differential sensitivity among optic nerve fibers and their attached sensory cells for different regions of the visible spectrum, and they believe that such specialization of the visual cells, coupled with integrated action, may give rise to color vision.

Thus the visibility curves of a single visual sense cell of *Limulus,* although not an insect, of the eye of a grasshopper, a diurnal insect, and of the eye of a Cecropia moth, a nocturnal insect, are qualitatively similar to the curve of the relative stimulating efficiency of different wave lengths of light for Drosophila, as reported by Bertholf, and to the behavior curves for the numerous adult and larval forms of diurnal and some nocturnal insects, as reported by Weiss *et al.* These curves are not identical because of the different methods of approach and technique, but they are all strikingly similar for the visible portion of the spectrum. All were obtained under wave lengths of equalized physical intensities. Hartline and Graham, and Crescitelli and Jahn, by properly adjusting the intensity were able to match the response to one color with the response to any other color, and Weiss *et al.,* in their behavior studies, found that insects responded to what were unattractive colors under equalized intensities when the intensities of these colors were increased.

Crescitelli and Jahn report that other authors who worked with pigeon eyes and the eyes of certain vertebrates also found that waveform differences are simply intensity differences and that the electrical response to different wave bands could be duplicated by adjusting the intensity of the different bands.

Thus it appears that both the electrical responses of the insect eye and the motor responses of the insect itself to different colors of equal intensity are due to differences in sensitivity, or to the absorption of light, which varies with wave length, by the primary photosensitive substance of the visual sense cells, and are not the effects of wave length by itself.

It appears from what has been set forth that although insects are sensitive to a spectrum extending from about 3600 A to about 7200 A, their motor responses and the electrical responses of their eyes, under laboratory conditions, indicate that they do not have color vision. On the other hand, in the field many species, especially those that are flower-visiting, behave as if they had color vision, although such behavior, if it is the result of vision alone, may be a response to brightness.

In view of the importance of ultraviolet as a stimulant, the effects of such wave lengths in sunlight and in the light reflected from foliage and flowers are of extreme interest. Observations by F. E. Lutz on about 100 flowers indicate that a majority of the "yellow" and many "red" and "purple," flowers are strongly ultraviolet. Few or almost no white flowers reflect ultraviolet, and few blue flowers are ultraviolet. Lutz photographed flowers through color screens and found that most of them reflect considerable green, that white flowers reflect large amounts of red, and that most yellow flowers are as strongly red as red flowers. Some authorities believe that ultraviolet reflected from flowers plays an important part in the sensation which certain flower-visiting insects receive when they "see" such flowers, especially in view of the fact that the ultraviolet energy of sunlight amounts to about one fifth of the sun's spectrum visible to such insects, and also in view of the fact that the shorter wave lengths of light contain greater amounts of energy than the longer wave lengths, and that photochemical reactions are produced more often by ultraviolet than by blue, and more frequently by blue than by green or red light.

On the other hand, little ultraviolet is reflected by green foliage, the maximum reflection from such being from 5400–5600 A (green-yellow), a region not particularly stimulating to insects when confronted under laboratory conditions by a spectrum of equalized intensities from 3650 A to 7200 A, but stimulating enough if the intensity is increased. As a group, insects apparently have little need to be able to distinguish colors. They are confronted mostly by radiation from the sun and sky and by the reflection from green foliage, and their sensitivity to wave lengths extending from 3650 A to 7200 A may make it possible for them to pursue their normal activities in the field regardless of constant fluctuations in the relative energy distribution of that part of the solar spectrum to which they are responsive. As a result they go about their business and are not unduly influenced by changes in the sunlight to which they are exposed. In view of this fact they would probably thrive in an ultraviolet world, or a blue world, or a green world, or a red world, provided the intensities of the colors were sufficient to initiate photochemical reactions in the primary photosensitive substance of their visual sense cells. As to whether or not they experience color sensations, this remains unanswered, but present evidence indicates that they are sensitive to and readily respond to different degrees of brightness regardless of color. Also, colors differ in the amount of stimulation that they give to insects. On the other hand, the training experiments of Frisch, Bertholf, and others, with bees, indicate that there are other effects produced by wave length differences.

THE SELF-MAINTAINING ORGANISM*

By EDWARD F. ADOLPH

ASSOCIATE PROFESSOR, DEPARTMENT OF PHYSIOLOGY, UNIVERSITY OF ROCHESTER

THE purpose of this paper is to outline in part a certain comprehensive investigation which has engaged me for some years. It concerns physiological regulations; the self-maintenance of organismal processes.

Anyone who looks at a table giving chemical analyses of the animal body, will note that water is the most plentiful constituent. It seemed to me that if one could understand how the water content of the animal body is maintained, he might thereby find a pattern for the maintenance of bodily components and functions in general. To a degree this has proven to be so.

Starting with water as a constituent of a living being, therefore, I plan here to indicate what happens whenever the bodily content of water is disturbed. Later, it can be shown that functions other than the control of water content are similarly organized. Finally, it is possible to suggest what these facts of organization mean for understanding the living being as a going concern.

The water content of a dog may be considered functionally. The water in its body is far from static, for it is being lost continuously by evaporation, although the dog's skin retards vaporization greatly as compared with a free-water surface. Most of the evaporation takes place in the respiratory passages, to saturate the warmed inspired air. Other water is continuously used to form urine and feces. All that water is replaced, by periodic drinking. The bare fact of replacement by drinking has been found to be an exact replacement; a quantitative relation between deficit of water in the body and amount of water taken at one draft (Fig. 1). The water is metered during alimentation and accords with the volume needed to remove the deficit in the body. The relation is simple enough; the construction and operation of the meter are at present beyond human knowledge.

* Presidential address before the Rochester chapter of Sigma Xi, May 2, 1944.

Lest anyone suppose that the alimentary tract is crucial to such a relation between intake and content, attention may be directed to the similar relation in the frog (Fig. 2). The frog sits in water, and takes

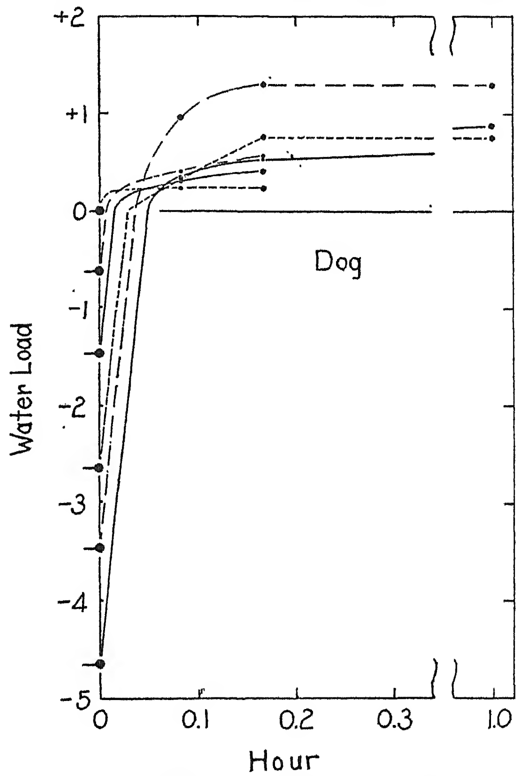

FIG. 1. AMOUNTS OF WATER INGESTED BY DOGS

WATER LOADS ARE IN % OF INITIAL BODY WEIGHT (B_0). 41 TESTS ON ANIMALS PREVIOUSLY DEPRIVED OF WATER TO DIVERSE EXTENTS ARE AVERAGED IN GROUPS OF 8 EACH. VOLUNTARY DRINKING IS AT TOP SPEED UNTIL CONTROL BODY WEIGHT IS REGAINED. ALL FIGURES ARE REPRODUCED FROM REFERENCE 3.

water in through the skin alone, again at a rate nearly proportional to the deficit of water previously established. The same has been established for earthworm and for one species of ciliate protozoan.

Having emphasized the prevalence of a relation, and de-emphasized the special proc-

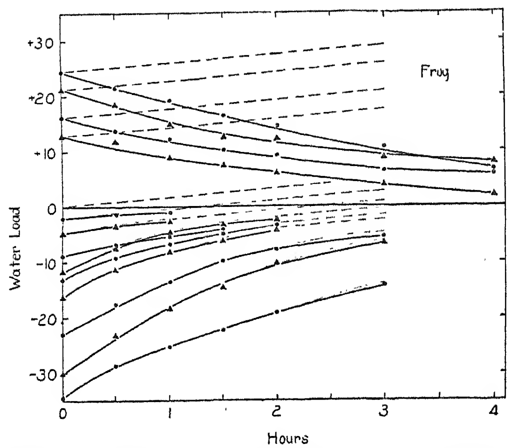

FIG. 2. COURSE OF WATER LOAD (% OF B_0) DURING RECOVERY

Rana pipiens RECOVERING FROM HYDRATION AND DEHYDRATION. *Solid lines,* TOTAL WATER LOAD (10 TESTS EACH); *dash lines,* GROSS WATER LOAD, INCLUDING URINE KEPT IN BODY BY LIGATURE OF CLOACA. DIFFERENCE BETWEEN SOLID LINE AND CORRESPONDING DASH LINE REPRESENTS TOTAL (URINARY) OUTPUT.

esses present in diverse species for executing it, I think it fair to suggest further that no organism would get along without this relation. This suggestion means that every species living has some means in constant readiness for gaining water when it is available, at a rate that is faster than usual whenever water deficit prevails. Such a general statement may seem to be a gratuitous hypothesis; it is demanded, if the belief is justified, that no organism can long survive without maintaining its water content. One thereby assumes that whatever individuals or species were unprovided with means of gaining water as needed ceased to exist. "There is not a single animal which could live or endure for the shortest time if, possessing within itself so many different parts, it did not employ faculties which were attractive of what is appropriate, eliminative of what is foreign, and alterative of what is destined for nutrition," Galen[1] said.

What happens, then, to an organism that has too much water in its body? Experimentally a dog has diverse amounts of water forced upon it by introducing the water in each test through a stomach tube. The response to all excesses of water is qualitatively the same; urinary output increases, and by an amount that eliminates most of the excess within three or four hours. Again a metered response is found, a precise correlation (Fig. 3) between rate of a process and content of water in the body.

The response of water excretion to water excess does not depend upon any special excretory organ. It is found in frog, insect, earthworm, and ameba, each of which has an emunctory different in structure from that of dog.

The relation between intake and deficit, and the relation between output and excess, can now be compared. Both deficits and excesses form a scale of water contents, water balance constituting the point between them. Both intake and output are rates of exchange and have common units. The combined graph (Figs. 4, 5; equilibration diagram) shows at a glance that the organism is protected both from excesses and from deficits. It is protected not only by virtue of faster exchange toward recovery (intake in deficit), but also by slower exchange by that process which would interfere with recovery (output in deficit). The organism seemingly cannot help but regain water balance. So would a reservoir for a city's water supply, or a toilet flush tank. We like to think in terms of

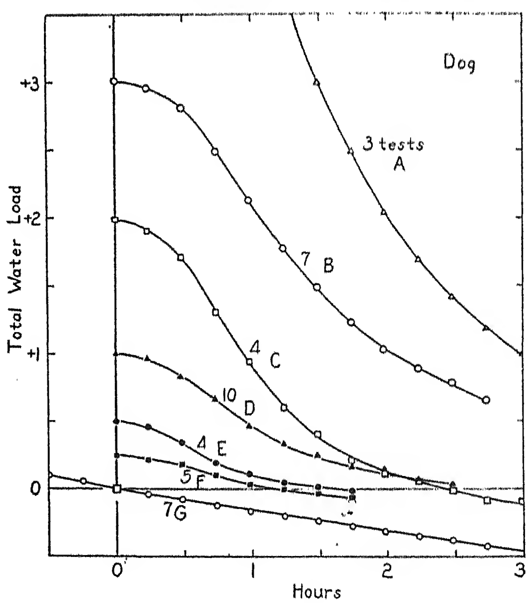

FIG. 3. COURSE OF WATER LOAD (% OF B_0) AFTER HYDRATION

DIVERSE SINGLE QUANTITIES OF WATER ARE GIVEN TO DOGS BY STOMACH TUBE AT ZERO TIME.

mechanical analogies; but they poison our minds when we infer that only a mechanical device could serve to accomplish the organism's trick.

All this arrangement or pattern or organization insures that the animal will recover its usual content of water after every disturbance of it. Actually, the arrangement is so good that the water content of the dog stays within one percent of the usual. That value characterizes the operations of this set of automatic relations. It represents a surprisingly small tolerance by an organism that has 999 other properties to govern at the same time.

Does it matter to the organism whether the water content is as usual or not? A dog or a man is debilitated by a deficit of water equal to six to ten percent of his body weight. He is convulsed by an excess of water equal to eight percent of his weight. He dies when either deficit or excess is double to triple this value. Somewhat larger departures from balance are tolerated only when salt and other constituents of the body accompany the water.

It might seem that the organism pinned its life upon the functioning of a tenuous relation among organs of exchange. And so it does; but the relation cannot fairly be called tenuous, for it operates everyday for scores of years in each of us, and not one in a million of us dies of dehydration or of water intoxication. Though not cast in iron with case-hardened bearings and triple communication systems, the organism survives the hazards of its water supply.

Equally immaterial are the arrangements by which the behavior of the dog or frog or earthworm makes water available when needed. No amount of internal regulation can take the place of mere availability of water to drink. Sensory and motor equipment of considerable extent is tuned to the task of finding water. Only when considerable water deficit is present does this task or drive become the predominant one. Then locomotion increases in rate, and sight, smell, and touch are used as guides to a source of water. If this behavior also seems tenuous, we yet recognize that it is so successful that dogs and men rarely die of dehydration. Some frogs and earthworms do, getting too far from moist ground, but not for lack of

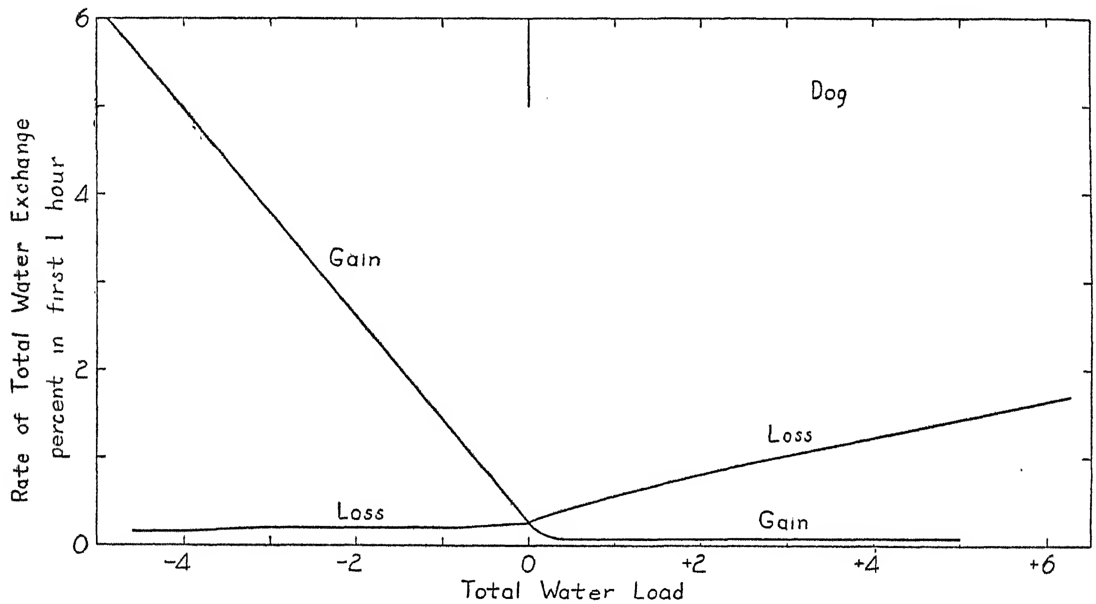

FIG. 4. RATES OF TOTAL WATER EXCHANGES IN DOG

THE RATES ARE % OF B_0 GAINED OR LOST WITHIN THE FIRST HOUR OF THE RECOVERIES SHOWN IN FIGS. 1 AND 3, IN RELATION TO TOTAL WATER LOAD (% OF B_0). THIS GRAPH IS TERMED AN EQUILIBRATION DIAGRAM.

useful behaviors toward water and water vapor.

In the whole attitude of the animal body toward water, no one can separate structure from function or process from behavior. All are knit together into a pattern of activities that works. No one can grasp that pattern who limits himself to the study of one sort of functional activity. For, the organism combines chemical, mechanical, and neuromuscular processes into a whole fabric that means success toward water. But the fabric is only a fragment if one arbitrarily confines his study to partial processes.

This account may suffice at present to describe the regulation of water content. Men have known the several facts of this regulation for centuries, and some of the quantitative details for seventy years. The present research consisted of putting selected facts together. It paid attention to *rates* of exchange instead of to forces concerned. The facts could go together in a dozen different ways; but only one choice of "variables" successfully represented the automatic aspect of the total processes. Mansfield Clark[2] challenges scholarship by asking: "Is knowledge a scrap-heap or an edifice?" "A goodly part of scholarship is the studious maintenance of balance between the advancement of knowledge and its consolidation." Only one grouping of the above facts resulted in an edifice.

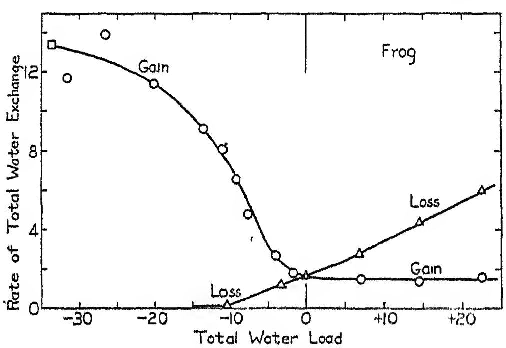

FIG. 5. RATES OF WATER EXCHANGES IN FROG

RATES ARE % OF B_0/HOUR, IN RELATION TO TOTAL WATER LOAD (% OF B_0). EQUILIBRATION DIAGRAM. RATES ARE ALL COMPUTED FROM FIRST ½ HOUR OF RECOVERIES OF WATER CONTENT SHOWN IN FIG. 2.

Granting now that the physiologist can, by proceeding in the above manner, understand how the animal body is hydrostatted, has he described an isolated case, or has he possibly found a key to understanding also the other 999 sorts of bodily maintenance? He is interested not merely in chemical constituents; but as well in metabolisms, postures, excitabilities, movements. Each sort of those is also maintained. Certain of the maintenances can be shown to operate in a way as automatic as that for water content.

Heat exchanges of man or dog are patterned in a manner analogous to water exchanges. The equilibration diagram erected for water needs only to have different units for heat placed in it.[3] Evidently establishment of the pattern for water regulation has furnished a plan on which heat regulation can be quickly and easily understood. The same floundering need not be repeated before this field of physiology can be grapsed.

And so, there are patterns of maintenance for oxygen, carbon dioxide, glucose, and other biochemical constituents. Some show transformations to storage forms; some have multiple paths of disposal. Each has its metabolic peculiarities, yet its content in the animal body is secured by a series of equilibrating reactions and behaviors.

Nonbiochemical quantities fit the pattern. The frequency of man's heart beat, the pressure of blood in his arteries, the concentration of proteins in his plasma, the size of his liver, the frequency of his meals, all show equilibration. When any is disturbed, a compensatory rush of specific activity ensues. When any is put out of reach, increased efforts are put forward to bring it within reach again.

The study of water exchanges of the whole organism had a certain advantage for our understanding of equilibrations in general, by virtue of the fact that both gain and loss could be easily and directly measured. For water of the *blood* alone, gain could not be separately ascertained. For glucose, most of the gain and most of the loss are accomplished by chemical transformation within the body. As regards frequency of heart beat, gain and loss cannot be identified except by mathematical integration; a decrease of frequency means a cumulative deficit, its payoff is seen

only in the change of frequency itself. Obviously the description of equilibration of any one of these functions is less informing than one in which the constituent processes of exchange are separable.

Investigation has revealed, therefore, the widespread generality of the relation set up between rate of exchange and content of component. The specificity of relation is, in each case, that required for successful handling of the component. Elsewhere[3] the evidence for this statement is amplified. What I have described some physiologists would label a mechanism of homeostasis. A danger in having a label is that physiologists may be deterred by it from further investigation of the general relations involved.

It remains to say something about the properties of organisms that may be inferred from the fact that all these many components are handled side by side in the body.

In my school days, I recall, I wondered how an organism could possibly be automatic. A dog or a man was such a pliable thing; how could he keep each of his processes under control? Could he be a thermostat, hydrostat, phonostat, and equanimostat in a thousand dimensions? How could any one system or unit be protected from enough disturbances at one time to continue its independent existence? While looking after one requirement another would escape notice, it seemed. At the time I gave up the youthful vision to the guess that, since I by taking thought could not manage a thousand flying tricks at once, neither could the thoughtless body. Perhaps my difficulty was my supposition that thought was as economical a process as any; now I recognize that without thought more gigantic feats are accomplished, and painlessly, than ever materialized with it. Now, after carefully examining a few processes in some detail, I come back to the view that the dog or ameba is a unit, statted in a thousand dimensions, so fool-proofly that no one of the dimensions gets out of control within a lifetime, nor hereditarily within a geological era.

I see no exaggeration in the belief that a thousand functions are simultaneously equilibrated in a dog, in an earthworm, in an ameba, in a bacterium. Small size is no hindrance to the possession of the requisite number of checks and balances. Protoplasm may look labile to the eye, yet it is the only visible component of these immaterial steadinesses. Alone it is less than the instrument panel which is no substitute for the radar that can incorporate those instruments.

The great miracle of regulation is that all the equilibrations can be carried out simultaneously. Perhaps controlling machinery is not hard to set up, but provision must be made that when set up the various stats do not interfere with one another. The thousand of them are designed to coordinate. To only a small degree is water content disturbed when glucose or heat is adjusted. When, however, mutual disturbance results, a definite order of preferences is pre-established; thereby water may be expended to dissipate heat, but heat may not be appreciably retained to save water. All those inherent coordinations are a secret of the organism's success.

Every biologist has a right to speculate how these arrangements in the organism came about. How many choices did the organism have with respect to each of its thousand components? I would guess at least five qualitative ones—one to relate gain to excess, one to relate loss to excess, one to relate gain to deficit, one to relate loss to deficit, and one to relate gain to loss. That adds up to five thousand choices of procedures or of operations. Then each of the five thousand had to jibe with many others which may have required many times five thousand more choices. Was natural selection among random trials the means of evolution? Did all the million or more inadequate germplasms perish? It may have been so, but I often wonder whether there is not some guiding pattern by which the combinations of processes that click can add another one which clicks without trying at the same time all those that cannot click. Such patterns are recognized in nonliving systems as well as in animate ones.

The truism that organisms are largely self-steering or automatic seems not to depend on whether or not their actions correspond to some particular definition of vitalism. Darwin showed how a process of natural choice,

of inevitable adjustment, produced an apparent purpose or end. This was to some "a great mental relief," says Merz.[4] "It explained how it comes about that nature, even with unloaded dice, so often—yet not always—throws doublets."

All the above is not an utterly new chapter in physiology. Historically, attempts to grasp the nature of regulations are ancient and various. The earliest were among the Hippocratic physicians who recognized the tendency of the body to heal itself and who accordingly relied less on external remedies. The detailed investigation of bodily functions began only in the nineteenth century. Then von Baer and Bernard began to appreciate the intricate machinery that served to steady one after another of the properties of the body. Only with the end of the century was it realized that the background of natural healing was one in reality with the study of morphological adjustment and with physiological regulation (Driesch[5]). Thereafter the opportunity arose to record in quantitative kinetic terms exactly how much exchange was related to how much deficit. Emphasis upon the dimensions and the correlations has, I think, made the story concrete and intelligible.

But only a beginning has been made in the study of the physiological organism. Most of the interrelations among components and processes which I vaguely mentioned still remain to be defined. The allowances for activity, for temporary predominance of one process over another, need to be investigated. And by comparative studies perhaps we shall gain some notion of what occurred in the unrecorded past to sort out the possible relations into that particular assortment which is the automatic physiological organism. We may say with the great naturalist John Ray:[6] "I predict that our descendents will reach such heights in the sciences that our proudest discoveries will seem slight, obvious, almost worthless. They will be tempted to pity our ignorance and to wonder that truths easy and manifest were for so long hidden and were so esteemed by us; unless they are generous enough to remember that we broke the ice for them, and smoothed the first approach to the heights."

What I have tried to present is a set of data regarding the water content of the body in relation to water exchanges for the dog and the frog. Such relation was shown to be general for a variety of organisms. By it the maintenance of bodily water content was understandably concrete. Further, such relation was found to be general for a variety of functions and properties. Thereby a glimpse was obtained into the intricate interlocking controls that pictured approximately, though very incompletely, a pattern of the living organism as a going and self-controlled unit.

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ON THE FABLE OF JOE PYE, INDIAN HERBALIST, AND JOE PYE WEED

By FRANK G. SPECK and ERNEST S. DODGE

UNIVERSITY OF PENNSYLVANIA AND PEABODY MUSEUM, SALEM, MASS.

WHO was Joe Pye, a man whose name has become a legend in American plant nomenclature? Was he a living character in the New England Indian population, or a mere phantom created by the rustic imagination of the colonists? The legend of an Indian medicine-man, named Joe Pye, curing the victims of a fever, typhus is mentioned by some, in early times is repeated by many New Englanders. He used a "weed" (*Eupatorium purpureum* Linn.) which took his name thereafter and found a status in the rural herbology of the Bay State. And garden-club botanists, and naturalists dealing with New England flora and folklore, have woven the legend into American plant history.

To an ethnologist the legend in question has significance. It has a bearing upon the discoveries accredited to native tribes which have benefited mankind at large, upon the broad field of aboriginal "natural science," and lastly upon the background of the American traditions that have built up our national character in the past three centuries. In this mood of inquiry we may now approach the topic.

We may well heed the admonition of Wm. Hamilton Gibson against contenting ourselves with being mere botanists—historians of structural facts. A wave of bold, realistic recognition of plant values in human history was about to invade the cloister precincts of old-school phylogenists and taxonomists.

At the time he wrote this a new branch was already sprouting from the main stem of learning in the broadest domain of the plant sciences; and that was ethnobotany. Most recently and most painstakingly a review of the gradual maturation of ethnobiology has been made the subject of treatment by E. F. Castetter (*The Domain of Ethnobiology,* American Naturalist, Vol. LXXVIII, March–April, 1944, pp. 158–70), to whom we may leave the burden of defining its scope and character.

The attention of the senior author was called to the question of identity of the Indian Joe Pye by a letter from Doris Appel, the talented sculptress of Lynn, Mass., who was fulfilling a commission for a series of heroic figures in stone to be placed in the Hall of Medicine of the National Museum in Washington. The series was to show in combined realistic and symbolic guise ten of the great contributors to the development of medical science of the world from earliest times to the present. She felt the urge to devote the first niche in the array of medical pioneers to the statue of a "primitive" healer typifying the very beginning of curative knowledge in human history. And her choice of a character to symbolize the dawn of knowledge in medical herbalism fell upon some American Indian medicine-man or doctor, if such a one could be found, to represent his race as discoverers of drugs and curative agencies derived from New World flora and as aboriginal American experimenters in medical progress. Mrs. Appel appealed to the senior author, in 1942, to suggest, through search in historical documents a personage whose history and fame might meet these requirements. Notwithstanding the failure on his part to find a suitable subject to serve as model for the likeness in stone, he did gain something from the attempt by learning more about Joe Pye, who at first seemed to be a possible candidate for the contemplated honor of immortalization in sculpture.

The bouquet of references culled from floral literature to grace the memory of Indian Joe Pye is not intended to form a final contribution to his unmarked grave. Here and there the naturalist ethnographer has repeated a version of the American backwoods classic. The following examples have

been encountered in the course of a summer's browsing amid the books on flowers and "weeds." Their wording is much the same—evidence of plagiarism. It would be pleasant to record the source of their origin in colonial literature. But such a pleasure is denied the writers for the present.

"'Joe Pye' is said to have been the name of an Indian who cured typhus fever in New England by means of this plant" (Mrs. Wm. Starr Dana, *How to Know the Wild Flowers,* N. Y., 1893, p. 210).

The same essayist a year later in a popular and appreciative approach to the wonders of plant life in natural history repeated her notice of the origin of the name without, however, indicating the source from which she took her information. Commenting upon the attractions of Joe Pye weed as a seasonal product in the wild bouquet of nature, she wrote, "It is said to have taken its name from an Indian medicine-man who found it a cure for typhus fever" (Mrs. Wm. Starr Dana, *According to Season,* N. Y., 1894, p. 107).

Thumbing casually through the literature of popular botany, we found another reference to the name of *Eupatorium purpureum,* Joe Pye weed, this time taxonomically classical and legendary for America as well. It comes from the pen of F. Schuyler Mathews (*Field Book of American Wild Flowers,* N. Y., 1902, p. 468); "Named for Eupator Mithridates, and for a New England Indian who used the plant in some concoction for the cure of fevers."

"Joe-Pye, an Indian medicine-man of New England earned fame and fortune by curing typhus fever and other horrors with decoctions made from this plant" (Neltje Blanchan [Mrs. N. B. Doubleday] *Nature's Garden,* Nature Library, Vol. 9, N. Y., 1907, p. 149).

The very latest reference encountered is that made by W. P. Eaton in the Boston *Herald,* Aug. 5, 1944, who writes on "Joe-Pye-Weed," saying, "Of course if you call it Joe-Pye-Weed you are perpetuating a legend only, of some Indian who employed this plant, so it is said, to cure fever."

The irrepressible Rafinesque now comes into the picture. In 1828 (C. S. Rafinesque, *Medical Flora, or Manual of the Medical Botany of the United States of North America,* etc., Phila., 1828), he gives the earliest reference to the Joe Pye legend to be found in print. Writing of Boneset (*E. perfoliatum*), he lists a vulgar synonym, "Joepye" (op. cit., Vol. 1, p. 174), and then adds (p. 179), "The name of Joepye is given to it, and to *E. purpureum,* in New England from an Indian of that name, who cured typhus with it, by a copious perspiration."

Seventeen years later, an exact repetition of Rafinesque's worded statement appeared in Peter P. Good's *Family Flora,* etc. (Elizabethtown, N. J., 1845, Vol. II, no p., article 51), without citing Rafinesque as his source. He also confused *E. purpureum* with *E. perfoliatum* under the English name of Boneset.

In the hope of finding some reference either to *Eupatorium* or to Joe Pye in the works of John Josselyn, the 17th century pioneer essayist in the field of American botany, a search through his *New England Rarities* (1672) and *An Account of Two Voyages to New England* (1675) yielded no data, even under the careful scrutiny of Prof. Tuckerman's detailed annotations. To what source earlier than Rafinesque the references to the fable repeated by later writers may be traced, we are still in the dark.

The original source of mention of Joe Pye as an existent person, and other references to the legend of the "weed" no doubt occur. Perhaps someone acquainted with early American botanical literature may supply the reference to its first appearance in print.

Many modern writers whose treatment of botany often combines popular features of sentiment and appreciation, folklore and fancy, with technical descriptions do not refer to the legend or to the source of the name. This is the case with Britton and Brown, *The Illustrated Flora of the Northern States and Canada* (1936), W. H. House (1935), E. R. Spencer *Just Weeds* (1940), all of whom pass by the story of Joe Pye and his antifebrile "weed." And the works of Gray are silent in the same respect. That T. F. T. Dyer in his *Folk-Lore of Plants,* N. Y., 1889, does not mention it is not strange for he was an Englishman. The medical botanists of the last century, W. P. C. Bar-

ton, *Vegetable Materia Medica of the United States,* Phila., 1818, J. Bigelow, *American Medical Botany,* Boston, 1817, and R. E. Griffiths, *Medical Botany,* Phila., 1847, also failed to note the legend.

Why the Joe Pye weed should have acquired the names it carries in different parts of its range, Skunk weed, Marsh Milkweed, Trumpet weed, Purple Thoroughwort, Indian Gravel or Kidney-root, Nigger weed, Quillwort, Motherwort, Tall or Purple Boneset, and King- or Queen-of-the-Meadow, as its synonymy reveals, raises other questions of European associations in folk botany. Incidently, the name "Joepye Weed" is apparently used only in a rather narrow area, stretching from eastern Massachusetts to western New York. North, south, and west of this area it is known by various of its other names, but it is, however, only the connection of the distinctly American plant with the mysterious, distinctly American personality of Indian Joe Pye that we choose to examine now.

Was Joe Pye, or the family bearing his surname, ever noticed in the genealogical literature of the New England tribes? Have the traditions surrounding the name and the plant been collected and discussed as historical facts or fancies?

An answer to the first query may be found in at least one dependable reference which we desire to call to the attention of botanical folklorists and historians.

The key to the solution of the mystery of Joe Pye's family authenticity among the Indians of New England of over a century ago is to be found in W. DeLoss Love, *Samson Occum and the Christian Indians of New England,* Boston, 1899. In the Connecticut Historical Society archives is preserved a diary written by Samson Occum, the Mohegan Indian convert of Connecticut who served the missions to the Brothertown and Stockbridge Indian bands in the Oneida country, in the state of New York (1786) and who was instrumental in the foundation of Dartmouth College. Occum made an entry in his personal diary July 14, 1787, saying "Some(time) in the morning went to see Joseph Pye, alias Shauqueathquat, and had a very agreeable conversation with him, his wife, sister and another old woman . . ." (Love, op. cit., p. 266). In 1789 Joseph Pye is again referred to in the chronicles of the Mahican mission at New Stockbridge as accompanying Joseph Quinney, another officer conspicuous in the history of the Stockbridge tribe (Love, op. cit., p. 281). The particular circumstances that led to mention of a Joseph Pye in this period of Indian migration which witnessed the removal of so many of them from Massachusetts, Rhode Island, Connecticut, and Long Island to new mission locations in central New York among the Oneida, are not pertinent to the purpose of this account. It is sufficient to note that in the last quarter of the 18th century an Indian named Joseph Pye was so relatively prominent in the Stockbridge tribe as to have merited historical notice. Be it recalled that the said tribe was composed of families drawn from the region adjacent to and west of the Connecticut River in its course through Massachusetts. Hence it follows that Joseph Pye of the chronicles was a descendant of the Indian family Pye stemming from the State of Massachusetts. It requires no stretch of imagination to ignore the assumption that his name and that of the mysterious Indian Joe Pye of "New England" who brought *Eupatorium purpureum* to assuage the fever of the colonists, is an accidental coincidence. The Indian natives of Massachusetts had adopted the English system of family surnames from the time of John Elliot, their apostle, more than a hundred years before the J. Pye of the 1787 reference. It follows that the latter bore the name of a direct paternal ancestor who dwelt in some part of the same state. That the J. Pye of 1787 could, however, be the same individual whose name and legendary beneficence are perpetuated in the herbal tradition so early and so widespread is quite unlikely. They could more reasonably have been grandfather and grandson, members of an Indian family residing anywhere between Massachusetts Bay and the Connecticut Valley.

Lest this brief communication be burdened with a mass of irrelevant data, we may conclude the historical résumé by affirming the tradition of existence of an Indian named

Joe Pye in the colonial period of the Massachusetts Bay colony.

Current sources of folklore concerning plant medicines are not silent in eastern New England. Traditions die hard among the people of the back country, especially if they smack of picturesque romance in the days of early settlement. An example of how the colonial saga of Joe Pye weed has been handed down among rural herbalists comes recently from William Luscomb, a native of Essex County, on the northeastern coast of Massachusetts, of Indian extraction. "Catnip Bill," as he is called in Gloucester and Essex, near where his wigwam of old sails, mats, and linoleum is located, is an herb-gatherer and itinerant Indian "doctor" over 80 years of age. We know him well. When approached and questioned about the story of Joe Pye weed, he became eloquent. He proclaimed in effect that Joe Pye was an Indian medicine-man who lived near Salem (Massachusetts) in colonial times, that he owned a large tract of land, that he taught the settlers to use "Joe Pye weed" to cure fever, that eventually he was crowded out of his land without pity by the whites whom he had befriended. So he had heard it from his father who was also an herb-doctor. Needless to add he, like others of his class, had more to comment upon in reference to the injustice of the whites. As to the fate of poor Joe Pye, Catnip Bill only knew that he had finally given up and moved away, which meant westward to Indian towns in the less settled districts of the state's interior.

Since the indications pointed to old Salem as a center from which the fable may have spread, we hopefully searched the local histories, but Joe Pye remains an unmentioned character in published town history, and in Essex County botanical records. One search covered the works of C. H. Osgood and H. M. Batchelder (Salem, 1879), J. D. Phillips (N. Y., 1933), S. Perley (Salem, 1928), and C. H. King, (1822–66, Brattleboro, 1937), all historians and folklorists of the county, and that of the botanist J. Robinson (*The Flora of Essex County*, Salem, 1880).

Not to overlook a possible clue to the meaning of the family name Pye in the native idioms of Massachusetts and New England as a whole, we may say that it seems at present etymologically insoluble. Without knowing what degree of corruption the original pronunciation of the term suffered through being sounded by English tongues and written out by English scribes of the period, it is baffling to relate the simple term to any plausible equivalent in New England Algonkian languages and dialects. It looks like a shortened form of a longer name-title such as was frequent in the Indian personal and family surnames of the region. That it could have been a nickname derived from English "pie" does not appeal to an ethnologist's view of the case. To our knowledge there is no family among the Indian survivors in New England that bears the name Pye. And finally, since the last speakers of the Mahican language spoken at Stockbridge have died, there is no living source among the Stockbridge Indians of today who could explain the personal name *Shauqucathquat,* noted by Samson Occum in 1787, as an alias for the Joseph Pye that he knew then.

SOVIET GENETICS AND THE "AUTONOMY OF SCIENCE"

By LEO KARTMAN

SECOND LIEUTENANT, SANITARY CORPS, ARMY OF THE UNITED STATES

DR. MICHAEL POLANYI'S article in the February SCIENTIFIC MONTHLY clearly indicates the historical forces which have been operative in the development and dissemination of science. Under the title, "The Autonomy of Science," the author poses a problem which has now become of prime importance not only to science but to every ramification of human social existence. Basically, this question is concerned with the contradiction between the theory and practice of individual control as against the practical ideology of state or collective control. This struggle between the two most fundamental forces of contemporary industrial society gained its greatest impetus, perhaps, from the impact of the October revolution which transformed Czarist Russia into its own antithesis. Since that cataclysmic event this controversy has entered and penetrated and colored every shade of our thinking so that it emerges even in many of the most trivial aspects of daily life. Dr. Polanyi has indicated its present importance to the future of science not only through a historical resumé, but also by means of a discussion of state control in the Soviet Union. These observations, coming from a scientist of his calibre, must be seriously analyzed since they surely carry considerable influence.

If I interpret his view correctly, Dr. Polanyi has attempted to show that the health of science depends primarily upon the degree of independence it enjoys in the task of choosing and conducting its research and the degree of free public discussion concerned with scientific concepts and conclusions. He suggests that "any attempt to direct these actions from outside" will bring about a state of disorganization which can only create a condition in which "the main body of science itself would disintegrate and fall into oblivion." By outside influence, I understand him to mean the direction of scientific work by non-scientists who are technically ill equipped to evaluate scientific problems, the trends of scientific thought, and the types of necessary research at any given moment. A state policy which allows such persons to direct scientific programs is thus negating all the conditions which have been responsible for the consistent growth of science during the past 100 years.

That the affairs of science should be managed by scientists is axiomatic and needs no further discussion here. That scientific progress has been "swift and steady" during the past century of its "independent" growth is, generally speaking, a valid observation and would be impossible of negation. That the future development of science is contingent upon a permanent state of "autonomy" is a moot point which I shall not argue at this time. (The position of science in industry, under civil service, and under war economy suggests, perhaps, that the basis for scientific progress may be changing quite radically even in those countries "where science is still free.") I am concerned here specifically with examples of state control and, for the sake of argument, I am assuming that state control is *à priori* productive of evils the presence of which leads to the death of science. The argument, then, is not whether state control is desirable as against individual control; the argument here relates to the problem of the *examples chosen* by Dr. Polanyi to prove his point that science is subverted by "attempts made to break the autonomy of scientific life and to subordinate it to State direction."

It would be difficult, if not impossible, to show that the state domination of science in fascist countries, especially Nazi Germany, has resulted in anything but a frustration of scientific research, a distortion of the legitimate aims of science, and a negation of scientific spirit. Dr. Polanyi's quotation from Himmler and his observations concerning recent German "scholarship" are well

taken. However, the main example of corruption brought about through state interference, cited by Dr. Polanyi, is concerned with certain developments in the growth of Soviet genetic science. This circumstance, I wish to urge, is an unfortunate one which detracts greatly from the value and validity of his case. Had he chosen to discuss the serious ramifications of principles laid down by Nazi "official science" his argument would have emerged forceful and convincing. The choice of the U.S.S.R., it would appear, vitiates fundamentally the cardinal *raison d'être* of his thesis. I shall attempt to show the reasons for this assertion.

In the first place, it must be made clear that the choice of the Soviet Union as against Nazi Germany is dependent upon the idea that "the attempts of the Soviet Government to start a new kind of science are on an altogether different level" from that of the Germans. He then reasons that since the Soviets are genuinely interested in creating a science for the public good the status of science in that country should provide the most adequate example of the principles of state control in operation. This is certainly a valid argument and it correctly, though superficially, implies a distinction between types of state control on the basis of motivation and subjective good faith, at least. (That there are more fundamental differences between a fascist dictatorship and a socialist dictatorship cannot be discussed here; it is unfortunate that this distinction was not emphasized since it is basic to the whole argument.)

The sole example cited by Dr. Polanyi concerns the genetics controversy in the Soviet Union which culminated in a national conference in which the two principals were T. D. Lysenko and the well known geneticist N. I. Vavilov. I unfortunately do not have any material on these discussions at hand; at any rate, I remember that, at the time, the reports in the American press were highly distorted and colored in attempting to promulgate the idea that Stalin was destroying formal genetics by clamping down on Vavilov and other so-called "orthodox Mendelians." Actual accounts of this conference showed that it was a nationwide discussion between the proponents of "vegetative hybrids" and the defenders of cytogenetics. It is Dr. Polanyi's contention that Lysenko's views won out and that the stage had been set for this victory of stateism and incompetence as far back as 1932 when it was decided that "genetics and plant breeding should henceforth be conducted with a view to obtaining immediate practical results and on lines conforming to the official doctrine of dialectical materialism, research being directed by the State." Furthermore, he goes on to show that the success of I. V. Michourin in producing new varieties through grafting laid a basis for the idea that new strains may be inherited independent of the germ plasm through a process of "vegetative hybridization." It is also shown that, previous to the conference, Lysenko had gained a large reputation and following and that he was appointed to important scientific posts in which capacity he was in a position to direct research. The influence of Lysenko, it is pointed out, caused many farmers, young students, and others without scientific training, to attempt experiments of one sort and another which they reported in journals and which were sanctioned by "gullible practitioners and politically minded officials."

It cannot be denied that such excesses of unscientific activity actually took place in the Soviet Union. The facts which Dr. Polanyi cites are substantially correct. That Soviet genetic science has been shattered by these events is, I submit, a complete distortion and misrepresentation of those facts. The advent of Lysenko and his school was nothing more than an episode, a passing phase in the development of Soviet genetics. I shall try to explain the reason for this below. At this point let us first cite responsible evidence that Soviet genetics is not only alive, but has continued along a course parallel to our own development in this field.

I have before me a volume of addresses made by eminent American scientists before the Congress of American-Soviet Friendship which was held in New York on November 7, 1943. Among a host of presentations was one by Dr. L. C. Dunn, of Columbia University, on Soviet work in biological science with special emphasis on genetics. Dr. Dunn, in an early portion of his paper, says:

The great vitality of Soviet biology is nowhere better evidenced than in my own field of genetics and its close relative, cytology. Here there is no doubt that the most important contributions have been coming from the U.S.A. and the U.S.S.R., and in the number of workers, of institutes and in quality of work these two countries are comparable. Genetics has been recognized in Russia as one of the disciplines underlying agriculture and medicine and has received a large measure of support.

It is quite clear that this famous geneticist has reference to classical cytogenetics and not to the theories of Lysenkoism. The development of genetic research has continued unabated to the present time—let us quote another passage from Dr. Dunn's address:

In 1921 the American geneticist, H. J. Muller, took to Moscow strains of the vinegar fly *Drosophila* and there grew up the greatest center of theoretical research in this field outside of the United States. . . . Out of Soviet genetics have come also new ideas of chromosome structure, of the origin of mutations and new ideas on the arrangement and relations of the hereditary particles, the genes, by very many workers. *By 1940 Moscow had in fact become one of the most important centers of work of this kind.* (*Science in Soviet Russia*, pp. 28–34.)

The impression gained from these passages is certainly not a picture of the disintegration of Soviet genetics under the aegis of state planning and state control. The writings of H. J. Muller, J. B. S. Haldane, and other well-known geneticists substantially support Dr. Dunn's contentions. A country which has produced such brilliant and world-renowned geneticists as Vavilov, Dubinin, Timofeef-Ressovsky, Dobzhansky, Gershenson and others can hardly be said to be favorable to ideas which are destructive of the accepted notions in genetics or any other branch of science, for that matter. Dr. Dunn tells of a letter he recently received from Professor Gershenson, director of the Genetics Institute of the Ukranian Academy of Sciences at Kiev, in which is cited the terrible destruction to the institute by the war. Gershenson asked for American publications and stocks of *Drosophila*. This clear indication of the vitality of Soviet cytological genetics (if it needs any defense) is further strengthened by the fact that:

. . . in this third year of Russia's participation in the war, she is still the largest foreign subscriber to the chief American scientific journal in this field. More copies go to the U.S.S.R. than to all other foreign countries. Moreover, a standard American text-book which appears in the United States in editions of 2,000 copies is printed in the U.S.S.R. in editions of 15,000.

It is sufficiently evident, I think, that no substantial case can be made out against state intervention on the basis of circumstances surrounding Soviet genetics. Dr. Polanyi has fallen into the not uncommon error of allowing Soviet excesses to color his view of what is really basic. It must be understood that the October revolution released a fund of energy which found its chief source of creativeness in the engineering of change. Every phase of human life, from the most intimate and personal factors to the largest state problems, has been dominated by the desire to liquidate the old ways, the fetters of decadent Czarist and reformist ideas. In such a spirit it is not surprising to find extremes in every field—and that is exactly what happened. Public education went to extremes suggestive of anarchy under the Dalton plan; marriage and divorce laws became exceedingly lax; the various leagues of "the Godless" carried on their work with an unparalleled sectarianism and myopia; there were excesses in criticism of all kinds, from self-evaluation to the public criticism of actors, composers, scientists, and factory workers; millions of little people, sincere and enthusiastic, were fired by the march of new ideas, and change itself became an obsession which they misinterpreted as the practical expression of dialectical materialism.

Such is the context in which we must view the Lysenko affair and the temporary entertainment of ideas which did not square with the established principles of cytogenetics. Every geneticist is well aware of these facts, but there are few, if any, competent geneticists who would argue that Soviet genetics has been rendered impotent by state control through the sanction of Lysenkoism or Michourinism. The new varieties developed by Michourin were on a par with the work of our own Burbank. If the Soviet Union sees fit to honor Michourin and rename a town Michourinsk, we have our town of Burbank in California and have heaped honors upon the man whose name it holds. Many popular science writers and many people in the United States have confused Burbank's

work with that of fundamental genetic research. That fact has not made our genetic science any the poorer and Burbank's influence has certainly not undermined the standing of the work of Morgan and his school.

Lysenko's inordinate enthusiasm to become divorced from formal genetics and to break away from Mendelian ideas stems directly from that national preoccupation with change and revolution in all things. Soviet materialistic philosophy had emphasized the importance of the environment as a factor in determining social and political ideas and conceptions of art, literature, etc. This tenet found expression in biological work as well, especially since it was reinforced by Pavlov's work on conditioned reflexes and by the practical application of this theory to human character development in the work of John B. Watson and his behaviorist school in America. Lysenko and his followers became enamoured of the principle of environmental influence and carried it to an extreme. A combination of political philosophy and the valid biological principle that changes in the environment of the embryo may produce marked aberrations—that "a character is always the joint product of a particular genetic composition and a particular set of environmental circumstances"—caused Lysenko to apply poorly digested political and scientific knowledge in a mechanical way. Some of the things he did were productive of practical results, just as work in other countries, on the selection of phenotypic varieties, has produced useful economic plants. It may also be possible that during the course of selection experiments genuine mutants were selected and not recognized as such. Lysenko was blind to the significant fact that the principle of dialectics finds its own validity in the prevalent concepts underlying cytogenetics rather than antiscientific extremes which created a principle out of an obsession for change. In the words of the great Soviet biochemist Bach:

> It would be a mistake to think that, in setting itself practical aims, Science in the U.S.S.R. neglects the solution of theoretical problems; quite the contrary is true, Soviet scientists strike for a happy combination of theory and practice and for their interaction.

This has not been the place for a discussion of the more basic aspect of Dr. Polanyi's thesis that science must remain autonomous to survive. Such men as J. D. Bernal, H. Levy, J. B. S. Haldane, J. G. Crowther, Henry Sigerist and others have written books and articles upon the subject which seem to indicate that Dr. Polanyi's argument is basically unsound. That is to say, his thesis is unsound in application to *all forms* of state control while it is undoubtedly valid in reference to specific types of which fascism is the best example in the contemporary world. In attempting to show the evils of state planning on the basis of the analysis of a mere historic incident he has not only weakened his case but has shown a degree of uncertainty regarding the question of what constitutes state planning, not in a vacuum, but in any given type of existing economic system. His article has correctly emphasized the esoteric nature of science and he can lay claim to having defended the thesis that science must be planned by scientists. He has not proven the claim, however, that Soviet genetics is an instance of a moribund science rendered asunder by state control. Close study of the matter will show precisely the opposite condition to obtain. We can do no better than close this discussion with a quotation from Harlow Shapley of the Harvard College Observatory. In a brilliant article which makes a strong plea to American scientists for collective scientific planning, he says:

> You will need to ask yourselves whether we in America are still young-minded enough, and socially minded enough, to work in this way for a common national and human good. Or should we leave dreams such as this to those national groups where there is no hesitation in making five-year plans, social, economic, scientific, and where the plans are carried out and progress made premeditatively toward the transformation of a national culture?

Genuine scientific progress, like the struggle for international peace, is indivisible. It cannot long tolerate conditions which tend to subvert its very foundations. Countries have had their pseudo-science, their mistaken theories, their excesses and extremes, their Lysenkos and Michourins. The test of the permanence of spurious ideas and the domination of imposture is the test, not of state control *per se*, but of the type of political and economic system that holds the reins of control.

SOME NUTRITIONAL ASPECTS OF GREEN LEAFY VEGETABLES

THE inclusion of numerous "protective" foods in our daily diet has been the nutritionist's common recommendation in recent years. Leafy green vegetables are among these particular foods. They are recommended, not for their contributions of protein and calories, but, mainly, for their minerals and vitamins. Consequently, we should take cognizance of those factors modifying these particular nutritive values in these fresh garden vegetables.

We have been prone to gauge our daily diets in terms of one or two servings of greens. We have, as yet, given little or no consideration to the inherent nutritional differences in these because they are of different plant groups, or to the induced variances in the same crop when it is grown on soils differing in fertility. Marked variations in vitamin and mineral contents have been observed in these crops as a result of their diverse genetical groupings, and of growing them under controlled, but variable, levels of soil fertility, determining the resultant plant composition.

With the exception of lettuce and a few minor crops, the green leafy vegetables are classified botanically as belonging to either the goosefoot (Chenopodiaceae) or mustard (Crucifereae) family. The former includes spinach, Swiss chard, and beet greens, while kale, turnip greens, collards, and mustard greens comprise the latter group. Nutritionally, with respect to their ultimate mineral contributions, the two families differ sufficiently to warrant a more careful discrimination in their use than is now commonly made.

Perhaps no vegetable has been maligned more than spinach. This is true in spite of "Popeye, the Sailorman," who has been clothing its consumption with strong appeal. Without undue disparagement of the nutritive qualities of spinach, it is unfortunate that such publicity could not rather have suggested the more popular use of kale, turnip greens, or some other member of the mustard family.

Of late the relative availability or digestibility of calcium and other minerals in leafy vegetables has been an item of concern. In all members of the goosefoot family and with one other related crop, New Zealand spinach, sizable quantities of oxalic acid make doubtful the dietary utilization of the plant's most important mineral components. Such vegetables may be worthless as contributors of calcium and magnesium and, in addition, may render unavailable a large percentage of the same minerals normally furnished by other foods in the diet such as milk.

Whereas the mineral contribution may be negative in spinach and its relatives, the vegetables of the mustard family are practically free from oxalic acid. They can, consequently, be recommended for their large amounts of readily assimilated basic minerals as well as their vitamin content, which generally exceeds that of the high oxalate-containing plants. These nutritionally superior greens are easily grown under a variety of climatic and soil differences. In addition, they have a long growing season. With most people there can be cultivated for them a liking and preference, certainly equal to that for spinach.

Not only do nutrient differences occur in leafy vegetables as a result of botanical relationships, but of increasing importance, as a determining factor in the dietary value of a given crop, is the fertility level of the soil growing it. Marked variations in chemical composition have been noted when spinach, Swiss chard, and New Zealand spinach were grown in sand-clay mixtures supplied with variable amounts of plant nutrients.

The outstanding effects so far have resulted from nitrogen. The excessive or even moderate use of nitrogen fertilizer salts, under our conditions, has given substantial reductions of as much as 60 percent in vitamin C, calcium, and phosphorus. Statistically these differences have proven significant far beyond the one percent level.

With respect to oxalic acid, it has been repeatedly demonstrated that the concentration of this organic compound in the crop increased as more nitrogen was provided in

the soil. In Swiss chard, oxalic acid production at all fertility levels was in excess of the combined chemically equivalent quantities of calcium and magnesium. The surplus of oxalate was progressively of greater magnitude as the soil nitrogen supply was increased and calcium decreased.

The influence of soil fertility on mineral and vitamin contents and thus its more indirect effect on the production of plants resistant to or unsuitable as food for insect pests was recently noted in a series of striking observations of selective injury on New Zealand spinach by the common greenhouse thrips (Heliothrips haemorrhoidalis). With 320 plants in the experiments, the insects, though free to choose whatever plants they wished, invariably selected as food those low in nitrogen. It was also of significance that when the soil's calcium supply was increased the insect attacks on the low nitrogen groups were less serious.

Many insects have, without question, definite attributes for food selection. This differentiation has been observed by us among plants of a given variety grown on soils differing in fertility. Similar discriminations are equally obvious between botanical groups. The goosefoot family and New Zealand spinach, objectional to man as food sources for calcium, are relatively free from insect attacks, while, in contrast, members of the mustard family are commonly infested with numerous parasites. These lower forms of animal life, unbiased by modern dietary propaganda, seemingly demonstrate more judgment in their choice of food than does man in his adherence to the directive of the two green leafy vegetables daily, regardless of kind or nutritional quality.—S. H. Wittwer, Missouri Agricultural Experiment Station, Columbia, Missouri.

THE DEVELOPMENT OF SOCIAL NESTING HABITS IN CUCKOOS

Cuckoos have long been famous for announcing the hours of the day, but they have another accomplishment not so commonly known—that of laying eggs in the nests of other birds. Of approximately 200 species in the family about 80 are known to parasitize the nests of other birds. Some species appear to be just starting on the road to such parasitism, and lay eggs only rarely in nests of other birds. Other species have developed the habit further to the extent of never building their own nests but always laying their eggs in the nests of other birds. A few species have advanced to the point of laying their eggs in the nests of only one other kind of bird.

One small group of cuckoos, instead of developing the habit of social parasitism, has become communistic. The birds associate in flocks of about a dozen individuals, spending the days together and sleeping in the same tree. At the breeding season the members of the flock cooperate in the building of a single nest in which several females may lay and incubate eggs. The males assist in the construction of the nest and in the care of the young. After the breeding season the young stay with the flock for a variable length of time, may help in the care of the next brood, and may even breed with the colony the next season.

These communistic breeding habits have developed in a small group of four related kinds of cuckoos called anis living in Central and South America. The most primitive of these communistic birds, *Guira guira*, live in loosely-knit flocks. The birds form pairs which may separate from the flock to nest or may join with other pairs. The development of communistic habits continues in the bird *Crotophaga major*. These birds associate in small groups and several pairs join in building one nest. The climax is reached in the two species, *Crotophaga ani* and *C. sulcirostris*. These two birds have somewhat different ranges but very similar nesting habits. The marital relations are very flexible. Frequently one bird has several mates. In some cases it is certain that a female consorts with several males and in other cases one male mates with several females. All birds assist in building one nest and in caring for the young. All the parents bring in food and give it to the young. Some females which had not laid eggs carried caterpillars to the nestlings. The development of this social nesting is probably the result of several influences in the history of the group.

One important factor in the evolution of

communistic nesting is the modification of territorialism. The concept of territorialism has developed from attempts to interpret various types of fighting behavior in animals. Briefly stated, the idea of territorialism implies the defense of a chosen area, usually the nest site and surrounding land, against members of the same species. For example, many birds defend a plot of land by fighting and singing to threaten possible invaders and drive out interlopers at once. The nest is built in the area and there the young are reared. In all the species of the communistic cuckoos the colony owns a territory which consists of two distinct sections. One part is a clump of trees for sleeping and the other is an area of fairly open land for feeding. From the communal territory, in general, other members of the species are expelled.

Intruders are threatened and driven about the territory in furious "dog-fights." The birds use a special call to indicate that an aggressor has entered the territory and then the whole flock attacks and chases the interloper, sometimes for as long as 2 days. In some cases the stranger eventually is accepted into the flock. If a stuffed bird is fastened in a tree the owners of the land attack and soon demolish the dummy. An amusing experiment is to put a mirror in a conspicuous place and watch the birds attack their own image and attempt to drive the "stranger" away.

Among the kinds of communistic cuckoos, however, certain differences in the maintenance of territorialism exist. *Guira guira* defends its territory only slightly. Numerous intrusions are permitted. The important point is that within this colonial territory one or two pairs may have their own small territories. However, the defense of this small territory is not vigorous and in many cases other individuals of the colony use the nest, thus producing a communal nest. At the next stage of development, as exemplified by *Crotophaga major,* the birds remain in pairs and all cooperate to build one nest in which several females lay eggs. The flock defends the territory. In the last stage of evolution, shown by *Crotophaga ani,* each colony defends its territory most aggressively and without exception attacks strangers. The greatest innovation at this climax of development is the disappearance of the habit of pairing. Copulation is quite promiscuous; a female has been observed on several occasions to consort with various males and a male may accompany any female. In these three stages the phylogenetic development of social nesting coincides with the disappearance of territorial defense by the pair and the appearance of territorial defense by the colony.

Several factors probably have contributed to the breakdown of territorialism for the pair. The first is that sexual fighting (fighting in reference to the sex-partner) is absent or extremely weak in the whole group. The situation may have permitted freer relations between the various members of the colony and a loosening of the sexual bond. A second factor which contributed to the breakdown of territorialism is the lack of a song. For most territorial birds song is one of the most effective methods of maintaining a territory and serves to indicate the boundaries of the plot of land and to threaten intruders. None of the communistic cuckoos has any note with the characteristics of song.

The weakening and eventual disappearance of the habit of active defense of a territory by a pair probably contributed to the development of social nesting in these cuckoos. Another influence of considerable importance may be the method of stimulation to lay eggs. Birds, like mammals, may be divided according to spontaneous or non-spontaneous manner of ovulation. In mammals the term spontaneous refers to species such as the rat which ovulate at regular intervals without external stimulation. Other species (non-spontaneous) such as the rabbit under natural conditions ovulate only after the stimulus of copulation. In birds the simple stimulation of coition has been superceded by the more intricate courtship performance. In many cases ovulation can be produced by a very slight external stimulus, such for example, as stroking the neck of female pigeons. There is considerable evidence that the communistic cuckoos belong to spontaneous type which requires no external stimulus to lay an egg. The birds

frequently drop eggs on the ground even far away from the nests and captive birds have dropped eggs promiscuously. The kinds of birds which ovulate spontaneously can dispense with courtship performance to stimulate egg-laying and since the function of courtship behavior is primarily, at least, the stimulation of ovulation, spontaneous ovulation and the lack of courtship are compatible. As a further correlative of spontaneous ovulation and the lack of courtship may be mentioned the lack of pair formation. Courtship, especially the mutual performances carried on during and after laying eggs, serves to bind the mates together and to prevent the dissolution of the pair. Thus in the communistic cuckoos the spontaneous method of ovulation made possible the disappearance of bonds between the members of the pair and the lack of bonds seems to have permitted communal nesting.

The development of social nesting in these cuckoos has depended largely upon influences closely connected with behavior and with the physiology of reproduction. The type of habitat is a factor of an entirely different kind which perhaps encouraged the evolution of communistic nesting habits but the importance of the habitat on the development of social nesting habits is difficult to assess. *Guira* inhabits areas of open parkland savanna and sleeps and nests in clumps of dense trees. Since there are few groups of trees in such vegetation, the birds tend to come together in flocks. The original habitat of *Guira* was probably the Chaco and the Campos of Brazil. These areas are characterized by open stretches of grass or marsh with scattered clumps of trees. The birds are able to feed out in the open but are forced to come together to sleep and nest. Although these islands of trees are sometimes very large, nevertheless there is the tendency for the birds to gather together. The members of the genus *Crotophaga* have retained the habit of feeding in open areas and sleeping and nesting in clumps of trees. These considerations suggest that the type of habitat in which the species originated forced the birds to come together in groups and to divide the territory into a nesting and a feeding habitat. The formation of flocks was conductive to the development of communistic nesting.

This group of birds has developed a unique type of nesting as shown by three developmental stages. The evolution of any anatomical or behavioristic character procedes in response to a multitude of conditions, none of which can be cited as the entire cause of the development. In the case of the phylogeny of the habit of nesting communally several factors may be considered as permitting the peculiar development. Perhaps the most important aspects are the breakdown of maintenance of territory, the habit of spontaneous ovulation, and the type of vegetation. The evolution appears to be guided by psychologic, physiologic, and ecologic influences.

A certain parallelism between the communistic cuckoos and humans may be noted in passing. One of the noteworthy differences between man and other mammals is the fact that human offspring remain with the parents for a very long time. Many of our social customs depend upon this intimate relationship. The communistic cuckoos also remain with the parents for a long time, in many cases for another breeding season. Indeed, the parents, the grandparents, and the young of another brood may join in feeding young birds in the nest. These birds have certainly developed social habits similar to ours and perhaps have even gone beyond. —DAVID E. DAVIS.

BOOK REVIEWS

A PACIFIC BAEDEKER

The Pacific Islands Handbook, 1944. R. W. Robson, F. R. G. S. 371 pp. Illus. 1945. $4.00. The Macmillan Co., New York.

From 1932 to 1942 the Pacific Islands Yearbook was published in Australia. War conditions prevented its publication in 1943 and the present edition, prepared by Mr. Robson of Sydney, Australia, has been issued in the United States. Much of the new material is American in origin and will prove to be of special interest to the people of this nation. The author presents a very careful chronology of the Pacific war from December, 1941 to March 30, 1944 and points out that each of the five groups of islands, Polynesian, Micronesian, Melanesian, Indonesian, and Non-Tropical, has been affected in one way or another by the activities pertaining to the great conflict. Near the opening of the book there appears a listing of the various island groups and a special listing of the islands governed by different nations.

Preceding the presentation of gazetteer data regarding each of the island groups Mr. Robson has given a brief but excellent summary of the history of the Pacific islands. There are thousands of these land areas in the Pacific Ocean and they range in size from New Guinea and Borneo, the largest islands in the world, to land areas that appear as tiny dots on our maps. When visited some of these very small islands are found to contain but a few acres of land. Many of the Pacific islands are the tops of lofty mountain ranges that lift their heads a few hundred up to a few thousand feet above the sea. These ranges rest on the sea floor three to four miles below the surface waves.

Most islands in the zone between the tropics of Cancer and Capricorn have coral reefs as festoons about their margins, or are composed entirely of coral limestone. Within this belt live unnumbered billions of tiny animals that secrete lime from the sea waters and build their skeletons of this material. They flourish in clear sea water all around the earth, where the depth is not more than 120 feet and where the temperatures do not drop below 68° Fahrenheit. Millions of coral skeletons broken into fragments or worn into sand grains by the action of waves are in each fringing or barrier reef; other millions are required to produce a single circular island enclosing a lagoon. Within the lagoons of these circular coral islands or atolls, the waters are comparatively shallow but with sufficient depth and extent to serve as safe harbors for ocean vessels and ships of the air. On page 41 you will find a carefully prepared description of a coral atoll.

The peoples who inhabit the Pacific Ocean islands vary greatly but they all presumably descended from early migrants from the Asiatic mainlands. They appear to be a mixture of Mongol, Caucasian, and Negro. There are hundreds of different languages used and since insular positions favored isolation the different groups of people have developed many customs and habits which are peculiar to themselves.

Until the middle of the 19th century these islands were not coveted by European or American nations. They were in a sense "no man's land." Today not a single one of them has not been taken over by some one of the powerful nations of the world. Each one is jealously held because of its strategic importance and the world awaits with considerable interest what may happen relative to the ownership and government of some of these islands following the close of the present World War.

The author is an able and a skillful journalist with an intimate knowledge of Far Eastern affairs. He has stressed many of the important events in the background of this war and laid a foundation for understanding some of the problems that must be faced after the fighting is over. He has known somewhat intimately of the activities of the Japanese people in the years preceding the war and has reviewed that history with skill and presented his ideas in a most readable form.

In the main body of the book the material is such as would be anticipated in a gazetteer or yearbook. There are articles about each of the important islands, and in each article the population, industries, shipping services and numerous other items of interest are treated briefly.

Mr. Robson has also included a section on political events in the Pacific. That is very welcome material for many who are becoming interested in the affairs of the Pacific Ocean peoples.

One of the outstanding features of this yearbook is an abundance of maps. Many of these are so large that they must be folded; others are small detailed maps of individual islands. The maps have been prepared chiefly for the purpose of bringing out the facts of location. Most of them do not reflect relief and they give no data regarding the climate, resources or other geographic factors of interest to many. Unfortunately on some of the maps, where an attempt has been made to show relief, as in the map of Polynesia, opposite Page 93, the cartographic work is very poor. The desire of the author to include so many maps is most commendable. It is unfortunate that he did not have more skillful draftsmen to help him in the preparation of that portion of his report.

Hundreds of thousands of American people have vivid recollections of experiences on and near these islands and in the homes of many of our countrymen, in the homes of Australians and New Zealanders there will be sad memories of the battles fought on or near these tiny land areas in the western Pacific. As a whole the volume is most welcome and especially at this time when all intelligent people of the world have become intensely interested in the Pacific Ocean area.

WALLACE W. ATTWOOD

CLARK UNIVERSITY
WORCESTER, MASS.

A NEW APPROACH TO EVOLUTION

Tempo and Mode in Evolution. George Gaylord Simpson. 237 pp. Illus. 1944. $3.50. Columbia University Press, New York.

THIS volume is a notable contribution of the Columbia Biological Series to the newer knowledge of evolution, maintaining the same high level of originality, wealth of material integrated, and lucidity of presentation which characterized the two other books on the subject preceding it in the series. Dr. Simpson who is a paleontologist attempts to bridge the chasm which was thought to exist between the data and theories of paleontology and genetics. Students in paleontology were believed to deal in a descriptive manner with the course of evolution as deciphered from residues of the historic stream scattered about at random, while geneticists liked to depict themselves as peering into the inner mechanism of biologic change. There can be no question that the appearance of this book will help greatly eliminate the mutual distrust which insulated the two vital aspects of the same basic phenomenon.

This point is made clear in a brief introduction. The text of the book consists only of seven chapters each of which is loaded with valuable data and theoretical speculation so that hardly a paragraph fails to convey a new idea or present in a novel relationship previously known facts. In discussing rates of evolution the author correlates specific and measurable morphological features, such as dimensions of teeth, which are plotted against lapse of time or thickness of geologic strata for several genera of Equidae. From the resulting curve are deduced some very stimulating generalizations. Dr. Simpson also discusses such aspects as the comparative duration of life spans of genera, changes in their rates of evolution at different geologic states and presents a statistical treatment of survivorship of extinct genera. All are originally and penetratingly treated in a manner to expose new possibilities and relationships. The treatment has almost the flavor of magic in that the author does so much with relatively little paleontological material though it is spiced with a considerable mixture of genetic concepts.

In the discussion of the determinants of evolution such as variability, mutation rate, character, population size, length of generations and selection, all the available genetic data and speculations on the subject are marshalled and integrated. The author examines and expands the meaning of the terms micro-, macro- and mega-evolution as distinct phases of the evolutionary process. Comparative evolutionary rates of different lines are divided into types called horotelic, or possessing a characteristic frequency curve "with a strongly prominent mode and with frequencies of rates falling off steeply on each side, more steeply on the side of faster rates," bradytelic which display slower rates and finally tachytelic or faster ones. The

various features and fates of each type are discussed against the background of the recent contributions in mathematical genetics by Fisher, Wright, Haldane and others.

Particularly stimulating are the last three chapters entitled Inertia, Trends and Momentum, Organism and Environment and Modes of Evolution. The first two discuss such familiar problems as teleology and orthogenesis, pre- and post-adaptation, and the complexity of the relationship between environment and adaptive zones which leads to the somewhat intricate but suggestive concept of the adaptive grid. The last chapter presents brilliant and fascinating schemes of evolutionary courses and potentialities as exemplified in speciation, phyletic evolution and quantum evolution, the three main modes of the evolutionary flux. It is impossible to give any more detail of this powerful scientific stimulant. One thing is certain. Evolution is now a scientific discipline amenable to challenging analysis and sound speculation. With the help of the experimental labors which Dobzhansky, Wright and others have launched, explorable tracts will undoubtedly be brought to view in a field of investigation which many believed to be doomed to obscurity for a long period.

MARK GRAUBARD
DEPARTMENT OF AGRICULTURE
WASHINGTON, D. C.

PERSONAL NOTES OF T. JEFFERSON

Thomas Jefferson's Garden Book. Annotated by Edwin M. Betts. 704 pp. Illus. 1944. $5.00. The American Philosophical Society.

EDWIN MORRIS BETTS and the American Philosophical Society have brought to garden lovers many comfortable and satisfying moments in "Thomas Jefferson's Garden Book 1776–1824 With Relevant Extracts from his other Writings."

Just as Darwin's "Voyage of H.M.S. Beagle" has held both an honored and a useful place close by the hands of naturalists the world over, so this garden book may find a similar position with garden lovers. And just as the "Diary" of the voyage published in 1933 is in many ways more revealing and more interesting than the re-written "Voyage" published a century earlier, so "Thomas Jefferson's Garden Book" direct from the pen of the master himself is in many ways more revealing and more interesting than any re-telling of his garden activities by others, excellent though they may be.

The title "Thomas Jefferson's Garden Book" may appear at first a bit misleading, for the original Garden Book was a memorandum book 20.3 cm. by 16.2 cm. with 158 leaves, of which only 33 are filled with Jefferson's notes. These notes occupy only a few of the 704 pages of the printed book. In fact it is the "Relevant Extracts from his (Jefferson's) Other Writings" which comprise the bulk of the book and which are the best of the reading. They are taken from Jefferson's "Farm Book," "Weather Memorandum Book 1776–1820," account books, stray memoranda, and letters.

Here will be found the record of the years 1776 to 1824—begun when Jefferson was 23 years old and continuing into his 72nd year, the year that Lafayette visited him at Monticello. The entries show the broadness of his interests—the date the first purple hyacinth bloomed in the spring, when peas were up, when the blossoms and pods were formed, when the peas were ready for the table. He noted that it took 100 strawberries to fill a half pint, to which Professor Betts adds the valuable comment that today "15 strawberries fill one half-pint." He found tongue-grafting a very effective method of top-working fruit trees and claimed to experience losses of only five grafts in a thousand.

On March 31, 1791, he wrote to his daughter, Maria Jefferson, in Philadelphia, "I wrote you in my last that the frogs had begun their songs on the 7th; since that, the blue-birds saluted us on the 17th; the weeping-willow began to leaf on the 18th; the lilac and gooseberry on the 25th, and golden willow on the 26th. I enclose for your sister three kinds of flowering beans, very beautiful and very rare. She must plant and nourish them with her own hand this year in order to save enough seeds for herself and me." And so the story runs from page to page of most delightful reading.

Professor Betts' annotations are helpful. Many more could undoubtedly have been made had space permitted. The "plumb-peach" mentioned by Jefferson several times and which Dr. Betts has not found listed in pomological works is very likely the nec-

tarine. The same expression is even today used colloquially for the nectarine. In 1807, correspondence from Timothy Matlock of Lancaster mentions the "Moor Park Apricot," which is still grown today as "Moorpark," and the same letter describes "Sechell's Pear." If this is the "Seckel" variety, which originated near Philadelphia and which, too, is still grown and esteemed today, it is as early a reference to this variety as can be found in pomological literature. In 1811, Jefferson wrote to Madame de Tessé in France, thanking her for the seeds of Koelreuteria, "one of which has germinated and is now growing. I cherish it with particular attentions, as it daily reminds me of the friendship with which you have honored me." The Koelreuteria is today an interesting and too-little known ornamental plant.

Everyone who peruses this book, for it is not a book to be read at a sitting, will find passages that appeal to him especially. For example, "The greatest service which can be rendered any country is, to add a useful plant to its culture; expecially, a bread grain; next in value is oil." Again, in a letter to Dr. John Manners, written February 22, 1814, is a remarkable statement regarding classification of plants and animals —"Nature has, in truth, produced units only through all her works. Classes, orders, genera, species, are not of her work. Her work is of individuals. No two animals are exactly alike; no two plants, nor even two leaves or blades of grass; no two crystallizations. . . . This infinitude of units or individuals being far beyond the capacity of our memory, we are obliged, in aid of that, to distribute them into masses, throwing into each of these all the individuals which have a certain degree of resemblance; to subdivide these again into smaller groups, according to certain points of dissimilitude observable in them, and so on until we have formed what we call a system of classes, orders, genera and species. In doing this, we fix arbitrarily on such characteristic resemblances and differences as seem to us most prominent and invariable in the several subjects, and most likely to take a strong hold in our memories. Thus Ray formed one classification on such lines of division as struck him most favorably; Klein adopted another; Brisson a third; and other naturalists other designations, till Linnaeus appeared. Fortunately for science, he conceived in the three kingdoms of nature, modes of classification which obtained the approbation of the learned of all nations. His system was accordingly adopted by all, and united all in a general language."

"I adhere to the Linnaean because it is sufficient as a groundwork, admits of supplementary insertions as new productions are discovered, and mainly because it has got into so general use that it will not be easy to displace it, and still less to find another which shall have the same singular fortune of obtaining the general consent. During the attempt we shall become unintelligible to one another, and science will be really retarded by efforts to advance it made by its most favorite sons. I am not myself apt to be alarmed at innovations recommended by reason. . . . My reluctance is to give up an universal language of which we are in possession, without an assurance of general consent to receive another. And the higher the character of the authors recommending it, and the more excellent that they offer, the greater the danger of producing schism."

Through the pages walk the great of his generation—George Washington, Thaddeus Kosciusko, Patrick Henry, Andrew Jackson, the Marquis de Lafayette, and James Madison. The correspondence brings to life Andre Michaux, Bernard McMahon, Rafinesque, John Bartram, David Hosack, Baron von Humboldt, and Lamarck. He ordered fruit trees from William Prince, of America's first family of nurserymen; and he corresponded with John Adlum, the father of grape culture in eastern America.

Professor Betts has done especially well in presenting the political background year by year as the chronicle of the "Garden Book" is revealed. So much of history treats solely of wars and battles and political crises. It is refreshing to find a place where the leading personalities of Jefferson's day are entwined with gardens, plants, and intensely human qualities.

The book is well printed and fully indexed. In addition to the 685 pages dealing with the Garden Book and relevant extracts from Jefferson's other writings, it contains 45

pages of appendices of interest, including a description of Jefferson's "Mouldboard of Least Resistance" and a list of books and pamphlets on agriculture, gardening, and botany in his library. The binding is adequate, but one wishes that circumstances might have permitted a more elaborate handling, to which the contents are well entitled.

H. B. Tukey
Agricultural Experiment Station
Cornell University

A PHYSICIAN'S CREDO

The Doctor's Job. Carl Binger, M.D. 243 pp. 1945. $3.00. W. W. Norton & Co., New York.

This extraordinary book defies the reviewer. The excellent diction is intriguing, interspersed with wit, some satire, compelling laughter when one is so stirred up he does not feel like laughing.

Chapters on the changes in medicine, specialists, choice of a physician, doctors and patients, are familiar to all doctors and are replete with valuable information for the layman. The chapter on psychoanalysis is confusing more or less to doctor and layman alike.

It is the chapter on Psychiatry and Medicine that is of particular interest today. The whole world now is the problem for these specialists. No Bacon today can write it all. From the Inquisition to the Thirty Years War to Hitler, is one and of the same pattern. How the world became as it is history tells. "In medicine's struggle with superstition, magic gives way to religion and religion to the method of science" says the author. That the truth will make us free has been prated throughout the centuries ignoring the fact that truth has been ignored while babbling superstition and religion. Why it remains so despite the development of great minds in science and mechanics is the major problem today.

The chapter on Some Common Psychiatric Problems as the author states "bears the problem of personal guidance" but also the guidance of nations in this world of turmoil. Perhaps he comes to the kernel of things when he states "Few of the clergy can manage human maladjustments which tax the most experienced psychiatrist." That I would say is the real kernel of the book. What we need is knowledge and not a play of words on things that never existed. Thinking without facts makes possible the proof of things one wants to see. Like hope of peace by prayer, when it is obvious that as an intermediary stage we must have cannon and airplanes. Experience has forced this upon the attention of everyone, even those it would seem who prefer to bask in the penumbra of a promised salvation.

It is only by tackling the psychiatric problems as the author does can we hope for a diagnosis and prevention of states such as now engulf us. He presents an interesting discussion of all the little wars that beset society varying all the way from child tantrums, puppy love to domestic turmoil and divorce which is the prototype of war. In both, mercy and truth must both go behind the ropes.

The new freedom has received a warning that the old restrictions are struggling with the new freedom. This he expresses "Man has not only taken a bite out of the apple but has swallowed the whole business with a resulting colic."

These revealing chapters while applied largely to individual problems are applicable to the solution of the diseases of the world. Herein lies the great stimulus to be found in the book now that we all are fearfully speculating on whether or not our so-called civilization has finally succeeded in committing suicide. It is a message of hope in the midst of despair.

Other chapters have more to do with the simpler problems of disease. Socialized medicine now excites the apprehension of many in the profession because they believe it threatens the science and practice of medicine which has developed within itself. The author presents this so comprehensively that it removes this fear. We must realize that we are facing new factors. All realize that the field of medicine has become so broad that some sort of group effort is mandatory. By showing that a large part of medicine's efforts are already socialized the author makes the prospect of the next step more palatable. This fact comes as a sort of revelation to those of the profession who grew up with the changes quite unaware that they were taking place. The confusion caused when one group does the planning while

another compensates, the violinist is now apparent. The small boy who hits the hornets nest plans something but it goes beyond the planning. Thus it comes about that active peristalsis is mistaken for mental activity. Sensations which should call for the bed pan are interpreted as an urge to sing.

It is a pity that this book cannot be used as the basis of a course of study, not only for premedical students but for the average students of college. It would give a broad view of life and living important to the individual and through individuals to a better comprehension of world problems. It would make the student understand the basis of a nobler life, and what is more, to want it. Yea, verily the truth will make us free but the juggling of it as we see now may raise something, the terribleness of it surpassing the headaches of Dante and Milton.

The truth will make us free but when do we start courting it? Here is a starting point not only for the layman but for the medical profession.

ARTHUR E. HERTZLER, M.D.
THE HERTZLER CLINIC
HALSTEAD, KANSAS

PENICILLIN IN PERSPECTIVE

The Story of Penicillin. Boris Sokoloff, M.D. 167 pp. 1945. $2.00. Ziff-Davis Publishing Company, Chicago.

IN this small volume the author ably combines an entertaining style with extensive information to bring the reader the high points of one of the most interesting stories in all science. The book is highly recommended to the intelligent lay reader who will appreciate the use of nontechnical terms wherever possible. The physician, the biologist, and other scientists will also enjoy this well-documented portrayal of science at its best. Extensive footnotes combined at the end of the book supply details for those desiring more complete information without interrupting the telling of the main story.

The first chapter provides an interesting setting based largely on personalities and experiments at the Pasteur Institute in Paris to orient the reader and acquaint him with the problem at hand. The monumental discovery of penicillin by Dr. Fleming is obscured somewhat in the second chapter by the author's enthusiasm in giving all due credit to Dr. Florey for bringing the great discovery to the attention of the medical profession. One might wish for a better look at the human side of Fleming, the man, as well as a fuller coverage of the rebuffs he encountered in attempting to interest the world in what he had done. No mention is made of his having been knighted for his contribution. Chapter three surveys the problems encountered in converting Dr. Fleming's laboratory experiments into the massive manufacturing enterprise necessary to make the new drug generally available. Why the English scientists entrusted this venture to their American allies and the contributions of the latter to the practical aspects of the problem is well covered. The triumphs of penicillin in each of the important diseases amenable to it are illustrated in the fourth chapter. One might wish the limitations of penicillin had been stressed more to avoid leaving the non-medical reader with the impression that the drug is a panacea. Chapter five mingles medicaments from molds in folklore with the present feverish activity to obtain useful drugs other than penicillin from molds. In the last chapter Dr. Sokoloff very ably depicts the problems, aspirations and rewards of workers in the fields of medical research. At many points in the book one encounters undue emphasis on nationalism. Science, like music, transcends geographic and racial boundaries.

WM. G. MYERS, M.D.
THE OHIO STATE UNIVERSITY
RESEARCH FOUNDATION

COMMENTS AND CRITICISMS

Dear 'Fessor:

A note to let you know that I am still alive and well after last week's excitement—and to thank you for the reading material which you have sent me. The Scientific Monthly I enjoy especially. I note especially one article in last December's issue, describing an imaginary visit of A. A. Michelson to Immanuel Kant, which you read and commented upon, and which I cannot forbear to write you about.

As you probably remember, what the whole article resolved itself into was a critique of the basic premises of the scientific method, in which this method came out somewhat second best at the hands of Philosophy. Judging from the points you marked, I would say that you seemed rather taken by the philosophic viewpoint yourself, and that is what leads me to bring you to task. I take a dim view of Philosophy in general, as I have probably told you. Nearly all of its systems, from the days of Plato to those of Spencer, when viewed objectively appear as nothing more than rationalizations of the prevailing moral values and metaphysical cosmology of the time in the light of the knowledge then available. You can see St. Augustine in the dark ages perfectly content with the theological views of the times, and everyone from Descartes to Hegel trying to "resolve the opposites" brought on by the new found knowledge in its impact upon beliefs which had been believed to have a divine, or at least an intrinsically valuable, origin. Your article is still carrying on the old game of trying to rope in science and break it to the philosophic halter.

Now it seems to me that the only philosophic school which ever brought to light anything of lasting value was the English "school" which began with Locke and ended with Hume, with old Bishop Berkeley sitting very uncomfortably in the middle. As your author pointed out himself, these men put a tack in the chair of philosophy which has caused its occupants to rest very uneasily ever since. Locke made the initial observation that we can know the external world only by our senses; the Bishop shrewdly pointed out that, since these senses are fallible, we cannot really be sure that the external world exists at all; and then Hume proceeded to hang the Bishop by his own cravat by failing to find any ground for the existence of the Almighty. It seems to me that no philosopher since has ever been able to find his way out of this impasse. With all due respect to Kant and his kindly views, he sounds very unconvincing.

Since that time, various other men, not philosophers at all, have even further strengthened the position of Hume. Not just the senses, but the human mind itself has been shown to be very, very fallible. The accumulated data of zoology reveal the brain as an adaptive mechanism, which adjusts the human to his environment. Psychology and physiology show the mind to be nothing but the manifestation of the working of the brain, much as the moving picture on the screen is manifestation of the working of the picture machine, and in no way independent of it; and they show too that the picture varies according to whether the machine has a high or low blood sugar, an excess of adrenal secretion, or a hyperactive thyroid. On top of all this, Freud, one of the truly great contributors of all time, showed that this mind can be shown by scientific observation *not* to be under the voluntary control of its possessor at all times; its thoughts can only be developed in those patterns determined for it by its hereditary capacity to think as developed and altered by past experience and present bodily well being. The synthesis of all of this is this: the human mind is not a reasoning mechanism at all, but the manifestation of a brain which is geared to produce THAT ANSWER WHICH IS MOST COMFORTABLE TO ITS OWNER, AND TO MAKE HIM BELIEVE THAT THIS ANSWER IS THE TRUTH. In the light of this knowledge we can see how it is that men—even the most learned and wise—cling so dearly to so many versions of the truth, and mankind advances by slow fits and starts; and we can see that the probability of man's reaching the truth by a process of "pure reason," without checking their results at every step by experiment, is quite remote, even in the case of a Kant. (Who, by the way, was a very neurotic individual.)

Now what of your philosophic method? I admit to you that we cannot know of the existence of the external world, or of the Almighty. Let me go farther, and point out that those men who try to do so by introspection give the impression of so many cows, contemplating their own bovinity. I agree that I can find no justification for my method within my own mind—but pragmatically (John Dewey is very useful) I must say that it makes no difference whether the external world exists or is just an idea. If we *assume* that it exists, and start from there, we seem to be able to alter the conditions of our existence vastly for the better by using the scientific method. And since this assumption has seemed to work, and been so fruitful, those of us who stand upon it cannot help but be amused at those philosophers who have started with another ASSUMPTION—that "I can reason"—and have lead themselves thence into a maze of sterile contradictions.

And so, 'Fessor, I don't find myself fazed by philosophers as so many men do. The typical old

philosophical argument that "we cannot conceive of such a situation," and therefore it cannot exist, or must be untrue, produces nothing from me but a horselaugh. Imagine Plato trying to conceive of radio, or Kant working out the present concept of the quantum, which is now a wave, and now a particle. Such an argument has no bearing upon the facts of the case; it is merely a commentary upon the limitations of the human mind.

The successful philosophers are the schizophrenics, who build their own mental world and retreat happily into it, while the rest of us hard-working individuals feed and shelter them.—(Lt. LAWRENCE E. HINKLE, JR., M.D., USNR)

Letter dated March 22, 1945 from a young naval officer to his father.—ED.

Proselyte or Temporize?

It is time that scientists, individually and as a group, should assert themselves. Not that they should organize as a pressure group for their own aggrandizement as some suggest, but that they should get at the job of performing the service to their country which they alone can do.

Scientists are now busy in laboratories throughout the country at the task of winning the war. They are hard working men, modest for the most part, and content with the satisfaction of knowing they have done their work well. Unfortunately, they are so modest that they are quite inarticulate outside their own profession. While officials in government, labor, and management virtually monopolize the public stage, the men who even made that stage possible remain in the background. Fortunately, public recognition of the role of scientists in winning the war is slowly emerging. This is as it should be, but it is not enough.

Equally as great responsibilities and opportunities lie ahead. Summed up, they can be stated in one sentence: Science is the major hope for creating the jobs which our people will need after the war.

Scientists are aware of this. They write papers on the subject for each other to read; they make speeches about it for each other to hear, and there is no essential argument. Naturally, they know that the problem is not so simple. Science alone cannot do it. Scientists need the support of private industry and of government for the facilities and the opportunity to work. They must have the support of these same groups to produce the results of their research in quantity. They must have the support of the distributing and servicing organizations which take these products to the public and enable the public to use them. They must have the support of the public itself. All are members of the team, but the scientists must create the products upon which all the others depend. Scientists must provide the ball for the rest of the team to carry.

All this seems clear and straightforward to the scientists. But is this enough? What do the people think who will benefit from the manufacture, sale, and consumption of these products? What do the managers of business think? What do the officials of government think?

To most laymen, science is still a mysterious and even awful thing. As for scientists, the cartoon caricatures of them as bearded, desiccated savants who ply their profession among nightmares of glass-blowing and behind locked doors is surprisingly close to being correct. True, the public reads accounts in the press of great discoveries and inventions, but too often these are "souped up" so that they bring a blush to the scientists involved. There is much to be read about tomorrow's dream world full of gadgets and leisure. But in these accounts there are many overstatements which, while sincerely meant, are damaging to the outlook of both the scientists and the public. How much better if the public could look upon scientists as individuals who practice no magic, and whose "miracles" are born and reared only by long and arduous hours of work? Scientists must tear away this veil of mystery and let the public see them as fellow humans.

In recent years, the managers of business have begun to learn the power of science when it is applied to their own problems, but their conversion is far from complete. Even in the chemical industry, there are islands of management holding out against the scientists with the defense that scientists are luxuries or necessary evils which are to be discarded when business declines, i.e., just when they are needed most. Among industries where the relation between science and business is not so obvious, there are still many converts to be made among the members of management.

In fairness it must be admitted that this is not altogether the fault of the managers. At least half lies with the scientists who for one reason or another are inept at meeting on common ground with businessmen. It is high time, therefore, that scientists meet them more than half way and demonstrate to them that in the diversified fruits of the laboratory lie the industries and the jobs of the future.

It is among certain officials of government, particularly the economists, that scientists have fared the worst with respect to the postwar period. Last year a contest on the subject of providing postwar employment was sponsored by a nationally known company whose very existence depends on certain fundamental natural processes. Most of the winners of this contest were government officials whose duties, directly or indirectly, were concerned with planning for the postwar period. In only two of all the winning plans was there more than passing reference to

the use of science to create employment. In many of the plans it was implied that no more applications of science which would result in major economic growth could be expected. Rather, most of these planners would provide employment after the war by changes in and controls of our economic system.

Now scientists will welcome corrections of the system which will permit it to function more efficiently and thereby better serve the needs of the people. They also will be, or should be, content to leave such corrections to those who are experts in those matters. But the creation of an optimum environment will only allow the economic system the freedom to function. The environment itself cannot create. The scientists must create the technological food on which the economic system survives and grows in the optimum environment.

Fortunately, among other officials of government there are indications that plans are under consideration to help harness science to industry in a manner similar to that which harnessed science to agriculture. This is a step forward, and if properly conceived and administered, it should be beneficial. Government has recognized the importance and power of science in winning the war and in preserving national security after the war. Scientists must see to it that government recognizes the importance and power of science in our economic life after the war.

To proselyte is not in the nature of most scientists, but the fact remains that science is the great hope for the future. This is the golden opportunity for scientists to convince our people that the application of science can create jobs and prosperity and happiness. Is there any higher justification for knowledge than the benefaction of mankind.—S. D. KOONCE.

Malthus Rides Again

In his review of my book, *Enough And To Spare,* under the heading "Population Problems" (SCIENTIFIC MONTHLY, vol. 60, pp. 316–317, April, 1945) Professor Karl Sax disagrees sharply with my conclusion that the available resources of the earth are adequate to meet all the future needs of mankind. Two items seem especially to have aroused his indignation to such an extent that his comments display more of partisan bias than of unprejudiced appraisal.

The fifth paragraph of his review opens with this sentence: "The declining birth rates of the Western nations are attributed by Mather to the natural consequences of man's evolution and 'are due as much to physical factors of the human body as to the mental attitudes toward the bearing and rearing of children.'" What I actually wrote is as follows (*Enough And To Spare,* p. 47): "Artificial stimulation of population growth by governmental decrees, economic measures or political propaganda has had such meager results wherever tried that it is unlikely ever to be significantly effective in any country. Indeed there is some evidence, though inconclusive as yet, that declining birth rates are due as much to physical factors . . . etc." By quoting only a part of a sentence, Professor Sax makes it appear that I was guilty of making a dogmatic assertion about matters concerning which present knowledge is obviously inadequate to justify any positive conclusion. As Enid Charles states in her book, *The Twilight of Parenthood,* page 185, "we know far too little of the physiology and psychology of reproduction to dogmatize about their relative importance." Thus, Professor Sax prepares the way for his caustic comment: "One wonders where Mather obtained his biological information regarding the causes of the decline in human birth rates."

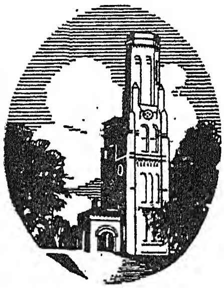
As a matter of fact, there is no excuse for that sort of wonderment. The bibliographic references pertaining to this portion of my book appear in full on page 176 and begin with one of the standard treatises on the subject, Raymond Pearl's *The Natural History of Population,* London, 1939. Pearl's analysis of population data bears out his assertion, page 256, that "the decline of fertility . . . appears not to be exclusively confined to highly 'civilized' countries, where the populations are most sophisticated and eager and adept at birth-controlling. It seems rather to be a world-embracing phenomenon—something affecting man as a species."

The other item to which Professor Sax objects is my survey of the potential agricultural resources of the world. Here he conceals the fact that I specifically attributed the estimates that I used to such authorities in this field as H. L. Shantz of the U. S. Department of Agriculture and F. E. Bear of the New Jersey Agricultural Experiment Station. He would have been fairer to all concerned had he modified the first sentence of his fourth paragraph to read: "Mather used estimates of potential agricultural production made by experts whose conclusions I consider to be unrealistic if not fantastic." It is not that I want to hide behind the skirts (or trousers) of other people. The point is that Professor Sax gives the impression that I, a geologist, have barged into an area in which I am quite incompetent to pose as an authority and have announced conclusions without adequate investigation of the facts. To protect the data against any such personal handicap, I took especial care to indicate the sources from which they were derived, but the review is so worded as to conceal that aspect of the case. As a matter of fact, if Professor Sax really meant what he wrote in his review—"We can have a permanent and *increasingly productive* agriculture" (italics mine)—he is at least as optimistic as I am.

The leading editorial in *Mining and Metallurgy* (the official journal of the American Institute of Mining and Metallurgical Engineers) for April 1945

closes with this sentence: "Those who worry about the exhaustion of minerals are either uninformed or they underrate the ingenuity and resourcefulness of the researcher and the engineer." May it not be equally true that those who worry about the inadequacy of food resources to meet the needs of mankind are either uninformed or they underrate the ingenuity and resourcefulness of the soil conservationist and the bio-agronomist?—KIRTLEY F. MATHER.

THE BROWNSTONE TOWER



At last the manuscript is finished. Written during the heat of inspiration, it was laid aside to cool; then it was taken up again and polished with loving care. Now it seems ready to go to the Brownstone Tower.

With parental solicitude the author mails the offspring of his mind. He worries until he is notified of its safe arrival and remains uneasy until word comes that it has been accepted for publication in the SM. After a brief glow of satisfaction his attention turns to other matters, and the manuscript is *forgotten—temporarily.* Then one day it flashes back into his mind: Why has not galley proof been received? Has the manuscript been overlooked or mislaid or lost?

The alarmed author should be reassured by the following explanation: Unlike many technical periodicals, the SM cannot publish manuscripts strictly in the order of their receipt. The waiting period of any manuscript is determined not only by the total number on hand but by the number in each field of science; usual practice is to publish only one article per issue in a given field. Most manuscripts are published within four or five months after receipt. Occasionally, a timely manuscript is rushed into the next issue. Thus Miller's article on the port of Cleveland was expedited to bring it out before the Cleveland meetings, and Harris' article on the Ruhr was pushed ahead so that it would appear before our troops occupied that center of heavy industry. At the other extreme, a manuscript may have to await publication for almost a year if it is, let us say, one of nine accepted articles in zoology and requires considerable editing. The significance of the last sentence should not be overlooked. Except for timeliness, nothing is more likely to expedite the publication of a manuscript than its submission in good form. The easier it is to edit a manuscript, the sooner may follow all other operations.

The number of manuscripts on hand at any time must be greater than the number of articles (eight to ten) published each month. In order to assure continuous operation of the SM and to facilitate its make-up, it has been found desirable to maintain a stock of about thirty-six manuscripts.

It is not difficult to make up an issue if many articles are in galley proof. An issue is built up around certain articles that are chosen because they are timely or because their turn has come. The *other articles* are selected from the stockpile to complete a diversified table of contents.

The time that elapses from the receipt of an acceptable manuscript until its galley proof goes to the printer as part of the make-up of an issue is variable and unpredictable, but the period from make-up to distribution of the issue by the printer is fixed, if all goes well, at three weeks.

Without reference to time, a manuscript goes through the mill as follows: The editor receives, records, accepts, and files the manuscript. When he edits it, he may, if necessary, correspond with the author. When satisfied with the manuscript he sends it to the printer, who returns four copies of corrected galley proof and proof of illustrations, if any. One copy of galley proof is read, corrected, and approved by the author; another by the editor. Corrected and approved copy is filed for use in make-up. The chosen galley proofs are paged, and dummy is made for illustrated articles. From this material the printer prepares and corrects page proof and sends two copies to the editor, who reads and corrects them and returns one copy to the printer. The printer makes final corrections in the type and prints about 14,000 copies of the issue. When it is mailed on the twenty-fourth of the month, a small supply of copies is sent to the office of the A.A.A.S., from which one copy is mailed to each of the authors of principal articles.

After publication of an issue its manuscripts and all material pertaining to them are held two months for reference lest any questions should arise. At the end of that period manuscripts and proofs are destroyed, the correspondence is filed, and illustrations are destroyed unless the author has requested their return.—F. L. CAMPBELL.

THE SCIENTIFIC MONTHLY

AUGUST, 1945

THE FALLACY OF THE "LOST YEAR"

By A. G. KELLER

EMERITUS PROFESSOR OF THE SCIENCE OF SOCIETY, YALE UNIVERSITY

PROMINENT among legislative agenda is "a national, compulsory, peacetime service bill for all American youth." In not being designated as "military," it avoids a challenge to one American allergy; but it runs head-on into another by reason of the adjective "compulsory" which, to not a few, envisages the policeman, just as "military" has, by tradition, connoted the top-sergeant. Perhaps a more formal term, such as "statutory," might arouse less instant antagonism.

The essence of the matter is simple enough: the civic duty of national defense legalized for all American youth, instead of being left to individual conscience; and training for the discharge of that duty provided under the direction of those responsible for national defense. That such training is to be "military" in its prime objective is inevitable.

There was a time, as witnessed by the top-sergeant tradition, when military training was both narrow and harsh. It still is, in a number of countries. It is not so here. Anyone who has followed the adjustments at West Point and the careers of West Pointers, in civil as well as in military life, knows full well that the Academy has been no "breaker of spirit" but a builder and strengthener of body, mind, and character. There are many of us who, though no longer young twenty-eight years ago, entered the Army as civilian officers in World War I, to obey orders where we were accustomed to give them, and to sustain occasional snubbings; and have regarded even our few months' awkward service as a life-asset.

It is a fact that prolonged service in wartime has upset some young men so that they have chafed at the comparatively flat and unadventurous quality of civil life; but that objection is irrelevant to the question of a short, not narrowly military, peacetime training. I, for one, can discover no valid arguments against this requirement of national service. If two terrible object lessons within a few years have not taught democracies the need of national preparedness against predacious peoples who openly despise us as soft, effete, providentially designated victims, then we deserve what is coming to us—eventually, if not now. As for the individual, privileged or unprivileged, that he will be better off physically is not debatable. And there is no idea of letting his mind or his character go to seed.

It is not only that his physical defects are going to be detected and rectified, his diet balanced, his intoxicants limited, and his exercise adapted so that if he has been living too high, fast, and soft, or too low, slow, and hard, he will be stressed toward some mean that is better for him; it is also that his mental and moral hygiene will be improved. He is going to get some genuine and highly practical education.

He is going to be "toughened" in mental and moral fiber. But toughening is by no means synonymous with "brutalizing." Nobody in this country wants to emulate the spirit of the Hitler Youth curriculum. Quite the reverse, for one of our main objectives is the banishment of that spirit into limbo and its imprisonment there. There will be as little instruction in "hatred" as there is in a fencing academy or on a baseball field. The governing idea is to impart techniques in the use of head, hand, and heart that are applicable in social life as a whole, not on the battlefield alone.

The inclusive mental and moral benefit from the kind of training proposed is the intelligent acceptance of discipline—of which more presently. Consider the effect on morality exerted by assembling youthful delinquents and making them behave themselves for a year or so, in the meantime opening to their vision careers alternative to their antisocial drift—alternative courses, of which they have not caught sight at all.

Evidently this moral-angle slant heads toward education. It would conduct many of the underprivileged and misled, whether or not they like it, into the reach of the learning—and the unlearning—process. In fact, all the approaches to this national service issue lead to education as do spokes to a hub; so that the hub offers a strategic point for viewing the whole wheel.

It is no accident that "discipline" derives from the Latin *disciplina,* meaning "learning." Education is nothing if not disciplinary. The School-Teacher of all time has been Experience, generally Sad Experience; and if experience is not disciplinary, nothing is. So, for the rest of this survey, we take a position on the aforesaid hub.

Education, in the abstract, has the reputation in America of a miracle-working fetish, to which all bow down "in wonder, love, and praise." But when one gets near enough to the idol to see it in the concrete, not only its feet but other items of its structure strike him as anything but godlike. Any veteran teacher can hark back to hours of shrill, censorious buzzing at education, in solo or in chorus, in youthful falsetto or in parental treble and bass, that seem in retrospect to have been as unremitting as cricket-stridulations of a summer evening. Practically every item in any and every educational setup has been damned, at one time or another, and not alone with faint praise. Though the main tune has been: "Education, our Safety in all Generations," there have not been lacking, when it came to the concrete program, dissonant variations or qualifications.

The first practical question that confronts one who ponders over the alleged "lost year" is this: "Is essential education going to be sacrificed by instituting national service?" If the education that is to be interrupted for a year or so is something altogether perfect, and as such irreplaceable, then it would be imbecile to sacrifice even five minutes of it. But not even the professional educationalist believes in that perfection except for his own version; he is, on the contrary, the most impatient and fanatical of reformers, inveighing against what is, sneering at it as old and shopworn, and proposing radical alterations and replacements. He calls loudly for the fetish-worship of Education; but inference from the rites which he prescribes in the endeavor to attain concreteness makes the fetish look as multiform as Homer's Proteus, and as shifty—Lo, here! Lo, there!

The contention here—to be brief and blunt—is that a year or so of such education as we purvey in schools of various sorts could be spared without shattering loss. College men whom I have known, with few exceptions and from a number of colleges, have repeatedly stated that such and such parts of their four years have amounted to a "total loss." They have singled out a few courses as "the only ones that taught us anything worth while." More frequently they have cited their "dead losses" and "hopeless boredoms." Over and over, they have referred to their athletic coaches, who "stood for no nonsense," as their best teachers. Such expressions are not confined to undergraduates. Many an elderly alumnus has reaffirmed them, years after, together with the hope "that things are better now."

Parenthetically, there is the apparently contradictory case of, say, the premedical collegian who realizes the indispensability of all his preparatory courses, but is uneasy because he feels that he has not been really educated. One engineer, with a similarly specialized course, put it this way: "Why don't we get something human? We all smell of gasoline." And he went on to say that there had been enough waste stretches in his earlier education that might have been planted with what he now missed—in fact, that he would be glad to have had his present course extended a year to supply that lack. Though such preprofessional cases are comparatively few among the millions to be called to service, they serve to fill out the picture.

As for the ordinary college course, how

many graduates could be found who could not, in retrospect, spare fifteen out of those sixty or so hours without a sense of irreparable loss? Many a graduate has expressed the opinion that whatever he had acquired in college—except for the imponderable advantage of spending an extra year in its characteristic "atmosphere," an advantage rather irrelevant to the topic before us—could readily have been accumulated in three years or less.

College men constitute but a small and privileged portion of those eligible for national service. Consider now the high-school contingent. Some of us found, in actual experience, that it was no great matter, fifty years ago, to skip a year in a first-class institution. It took considerable anxious effort and vetoed vacations; but we have felt no sense of educational loss. It is true that we could not have spared the extra year that we were skipping; but even for the less hurried student, those four years could have been condensed into three without any substantial sacrifice, by omissions of the valueless or less valuable, and by better teaching.

It is not impossible, either, that a year might be similarly saved in the grade-schools; and I do not mean through the adoption of any touted foreign system. It has been proclaimed, over and over, that English, German, or French pupils are better educated than American students two or three years older. I have never believed that. I once heard an eminent Russian scholar, on being congratulated because he had learned so much at so tender an age, reply: "But I had no youth! I had no youth!" And I have pitied every John Stuart Mill. The ability to compose Latin verse, or to read Gaius in the original, remaining the while pathetically lopsided and self-complacent about it, has never struck me as a superiority. I have heard intelligent foreigners bewail the narrowness of their traditional education and remark that it would take a number of first class funerals to get rid of it. As one eminent college dean used to put it, real education is not a drill-hole, very deep, very narrow, and very dark down below; it is an excavation, into which the light of day can pour.

There is a lot of fluff in lower-school curricula, from the kindergarden up, that has been interpolated by exponents of the bright idea and "taught" by hypnotized and muddled teachers. A full year's work, or more, on infant philosophy and opinion-swapping of one kind or another—of "teaching to think" where the wish is father to the thought—could be cancelled with no loss whatever except that of intellectual ragtag and bobtail.

In short, a year or so could be saved almost anywhere along the line, without losing anything vital to education; and at the same time a wholesome pressure could be exerted to make the remaining years amount to more. General education would profit. So long as any study has a kind of protective-tariff wall around it, in the shape of a don't-touch-me tradition, to shield it from competition in the open, it will squat where it is. The teaching of the classics has gone to seed behind such wind-breaks, and so has that of modern language, in adhering to the same ineffective methods. What is the use of "taking" Latin or Greek for years on end, if you never learn to read it? It is lucky for the modern languages that practical utility, demanded in a crisis-time, has forced, largely at the instance of the Army and Navy, common sense toward the front. For common sense, long in abeyance, refuses to credit the ancient contention that one can "appreciate" what is said or written in a foreign language when he doesn't know what the words mean. I well recall the ecstasies of a certain aesthete over a passage in *Faust*. Being asked to translate, he couldn't. What he had got out of the passage was a seductive noise.

There would be no object in rescuing a year, even out of chaos, if there were nothing to do with it, or only something worse than nothing. Teaching "brutality" would be worse than nothing—even worse than the most fatuous offerings of any current curriculum. But brutality is out, as remarked heretofore. What is "in"? What will service-education teach?

Assuming that theorists, pedagogical or bureaucratic, do not get hold of it, the proposed service education, whatever its specific content, will be practical, and along lines indicated toward the outset of this survey.

Moreover, though one must await any publication of schedules, he can be confident that two vitally essential desirables will be inculcated, directly or indirectly, in all curricula: Discipline and "Democracy."

Education in this country, by life-experience or in schools, has been weakening in the element of discipline. All the shouting has been for liberty and rights, with little heard about discipline and duty. The covering case of liberty *versus* discipline cannot be presented here; but it can be stated flatly that discipline is quite as indispensable to individual and civic well-being as is liberty.

Discipline is not popular, because it means restraint and compulsion: to refrain from doing what one wants to do, and to do what one does not want to do. And real education involves both of these unattractive performances. One may not choose to go as far as the Seer of Archey Road, who remarks that it makes small difference what a boy studies, so long as he doesn't like it; nevertheless there is sense behind that apothegm. As it is not by sauntering nonchalantly along the easy way, or floating with the current, that muscular fiber is strengthened, no more is character-fiber won by dodging the unpleasant or even the repulsive duty. Obedience to duty is a primary lesson in discipline —a lesson set by some form of authority. To learn it is not demeaning at all, but a display of intelligent discretion.

The proper order in education, especially at the outset of schooling in general or in particular, is: obedience first, reasoning second. That has been the order which the race has had to follow if it was to survive. It is the order of learning anything. Until we have been through the mill we are incompetent to judge of it, favorably or otherwise. Men have obeyed "Natural Law" for ages before they have understood it in the least. We have become "reconciled with the Universe" because "we'd better be." With all our "progress," we understand very little, even yet. Furthermore, the more we do understand, the more there is to try to understand, so that we have very small prospect of full understanding, ever. But obedience must go on, irrespective. The Way of Things cannot be challenged.

How Natural Law works has been discovered by long experience and the necessity of obedience to it. Men have always been concerned about how best to identify and obey it, that it to say, to apprehend, and then to adjust to, life-conditions. That is why they have been eager to take orders from those who have possessed, or have been thought to possess, the authority of greater experience: the old hunter, the smith, the chief, the medicine-man. Such authorities were obeyed until, despite all their excuses or alibis, they were shown up by sad experience. Obedience first; then refusal of obedience to what did not work—reasoning long after. Back always to experience as the final test.

In the epigraph of his *Second Jungle Book*, Kipling wrote:

> Now these are the Laws of the Jungle
> and many and mighty are they;
> But the head and the hoof of the Law
> and the haunch and the hump is—Obey!

Some say that we are still in a jungle, and they are not referring to those of us who are fighting in the tropics. If they are right, we had better be heeding whatever experience the race has had in getting out of the wilderness. In the matter of emergence from the education-muddle, we had better stick to "first things first." The order that has been followed seems often to have prescribed the roofing and gargoyle-elaboration of the educational structure before the foundation has been firmly laid: the last and most frilly features first.

Discipline does not mean slavish obedience. It means the acceptance of guidance from those who are in a position to know. The military establishment has taught discipline as no other. It has over-taught a narrow type of it in the past. It is not at all likely to do that now; and if it should run in a lot of duds or dubs, in courses or teachers, they would be speedily shown up. For our whole tradition has made us individualistic and averse to militarism. That puts the burden of proof squarely upon this new enterprise. Nevertheless, we have been suffering for some time, in education as elsewhere, from liberty degenerating into license; and a short course in discipline, under such broad-

gauge military authorities as we are privileged to possess, promises to be to us a national asset of the first order.

There remains what is perhaps the supreme educational service to be expected from the project before us: its laboratory training in "democracy." If there is any one stiffener that is needed in a society that is being warped into a manufactured "class-consciousness," it is a mutual understanding between those who are being guilefully persuaded that they belong apart and not together.

There are really no "classes," in the Old World sense of groups of fixed status, in this country. Says Susan Ertz in *Anger in the Sky* (p. 286): "Instead of talking about 'People in different classes' we ought to say 'People of different educations', which is all this class nonsense amounts to."

This service-training is but another exhibition of our national policy of opening equal chances to all, so far as humanly possible. It is at any rate a short course with nothing in it that is capable of being interpreted as formative of "classes." It is a kind of wholesale public school, transcending the local type where the young have long mingled with their fellows and have arrived at a degree of mutual understanding and friendship on a smaller scale. We make children go to school, whether they or their parents like it or not; and we think we are doing a good thing, profitable to us all.

There is nothing like personal acquaintance to remove class-misunderstandings and suspicion in general. Conferences around a table have cleared many an atmosphere; and the homelier and more informal the association, even the mere eating together (the traditional symbol of brotherhood), the more likely are friendly relations to ensue.

This generalization needs no proof. Is it applicable to the association of youths in living and working together at the same tasks for a year or so during their formative period? The only answer is an emphatic *yes*. If there is any course of education that can inculcate and confirm the democratic spirit and do away with all this clacking about "classes" in the United States, here it is. The "buddy" relation between veterans may be a little sicklied o'er with sentimentality, but there is something very real in the sense of having been imbued, by service together, with the same spirit. A year's association of all kinds of young people, side by side under an intelligent discipline, is sure to be an education in itself, and unique, no matter what kind of a curriculum is imposed. It would fit a youth for American citizenship as no amount of isolated book-study could; for it would be a genuine laboratory course in mutuality.

Van Doren writes of army life, in *Tilda,* 1943, p. 119:

> It is no way to live forever, but there is something wonderful about being churned without warning into a crowd, bundled up like a stick with countless others. The point is that no one of them turns out to be a stick. A few of them I detest, and plenty of them are tiresome; but I wouldn't change anything if I had the power. I wouldn't authorize any man of them to be different. After we stop having wars, if we do, we'll still have to see to it that every so often the population gets stirred around—forcibly, I mean, and arbitrarily, so that Citizen 86, 424, 973 finds himself overnight with a pack of strangers from places he never heard of, pitched into an ocean of fresh faces. It gives you faith in the species, in case you have lost it, or in case you have identified it with the few persons chance has decided you were going to live with forever. This is merely chance on a bigger scale, throwing you into the lap of the race.

Van Doren may be sentimentalizing a little; but we are going to be plumped into "the lap of the race," whether we like it or not, more often as time passes; and we had better be finding out what it looks like.

Many an elderly American is convinced that he would now be a better citizen, and a better man all around, if he had had the experience, decades ago, that is to be offered, he hopes, to present-day American youth. And it is not a very long inference that the reflex action of this national service cannot but be a blessing to our educational system in general, if only by impelling it to take up slack and to do a more genuine job. The year or so could be spared without much, or any, sacrifice of anything but gratuitous interpolations that crowd out essentials—especially discipline, upon which the proposed national service would characteristically insist.

THE PROBLEM OF THE AMAZON—II*

By F. FERREIRA NETTO

ASSISTANT GENERAL MANAGER, SERVICE OF AMAZON NAVIGATION AND ADMINISTRATION, PORT OF PARÁ, BRAZIL

[Translated by W. Andrew Archer]

THE QUESTIONS

Economy. A representation of the commercial movement of the Amazon in the years from 1889 to 1942 by means of a graph would show that the curve of volume rose until 1912 but afterward fell rather precipitately and that it has remained stationary with the exception of minor variations.

This curve tells the complete story of Amazon rubber, its birth, full glory, and final decadence. The complete decline of this main commodity is now postponed indefinitely, thanks to the plans for its re-establishment and to various interceding factors such as the gradual increase in national manufacture of rubber goods. It is unnecessary to rehearse here the well-understood causes of the failure, but the conditions are still the same, and urgent remedies are needed to prevent a new collapse. Amazon economy rests entirely upon this extractive industry, and its successful re-establishment would be impossiblė in the face of the dangerous consequences of unstable prices for rubber products.

Nowhere in Brazil, or perhaps even in the world, is found a region with an economic aspect so singular as this, where 80 percent of the production is exported and 90 percent of the consumption imported. Furthermore only 10 percent of the exports is destined for the Nation itself, while close to 80 percent of the imports originates in other Brazilian states. Whether due to the relative isolation from the industrial and consuming centers of the Nation or to the greater proximity to the important foreign markets, the fact remains that the great consumers of Amazon products are found outside of Brazil. Thus, not having an important internal market for its products, the Amazon economy is subjected not only to fluctuations arising from international crises or stock-market speculations but also to price fixing by foreign cartels.

* Continued from page 44 of preceding issue.

Shortly before the outbreak of the present world conflict an animated commercial movement was beginning to take shape, undoubtedly motivated by the necessity of certain nations to create stockpiles of materials against future blockades. It is difficult at present to foresee how the Amazon economy will orient itself, in view of the events which are unfolding so rapidly. Any measures that might be taken now are certain to be affected by the outcome of the war; but even so it is urgent that some means be found to increase the national consumption of Amazon products and also to improve the methods of production. Nothing is gained by having the largest forest reserve of the world if no real advantage comes out of it. Of what good are thousands of native rubber tappers if their methods of extraction are uneconomical? Of what use are these immense, potential resources if nothing is done to convert them into actual wealth?

The present exploitation of Nature is absolutely irrational, and for this reason the results have been and will continue to be false and disillusioning. Whoever visits the commercial districts of the larger towns and observes the furnishings of the various business houses usually gains the impression that everything is only provisional, installed as cheaply as possible so that all could be abandoned at any moment without great loss. This disinterest for the appearance and comfort in the place of work, where it is necessary to remain during the greater part of the day, is most depressing when one thinks of the important financial resources of the heads of the concerns. This is one of a series of details neglected by the businessmen whose ideas have not yet turned to modern methods of efficiency and progress. The addiction to accepted custom is one of the things most responsible for the situation in which the Amazon is involved, and it will not change as long as purely commercial interests remain predominant.

Amazon economy will benefit most by measures to abolish the primitive barter of products extracted from Nature. The majority of the local capitalists will not be easily convinced of the benefits to be had by investing their money in new industries to exploit native raw materials or in modern methods of agricultural production. In any product originating in the Amazon can be seen immense possibilities for a lucrative exploitation whether by agricultural or industrial development. Only when this stage of advancement has been reached can the Amazon economy be considered secure from the tremendous crises which affected it in the past.

Parallel with the agricultural development of native crops, certain foodstuffs should be made available in greater abundance by increasing the buying capacity of the people. This should be so regulated as to prevent the complications which follow any price increase for local products and which bring unsolvable problems for employers and public officials.

The business methods of the region, outside the larger towns, are so primitive as to be direct barter of merchandise for produce. Small traders or hucksters traveling in their own boats through the interior do the trading either directly or through a third person. This sort of exchange is unfavorable for organized business because the producer is almost always cheated both in the value of what he gives and receives, but existing conditions make it practically impossible to defend his interests.

In some localities the natives already have a conception of the advantages of working together, which they have learned from the traditional *putirum,* a term applied to community fishing or planting activities. However, cooperative organizations would be handicapped by the lack of means for dividing the profits and paying for personal services.[13] Another obstacle is the people's latent distrust of government regulations. They are afraid to supply any kind of financial data, being accustomed to regard the public administration merely as a system for extracting taxes. Production will evolve into a cooperative plan only through special, semi-compulsive methods backed by adequate organization and legislation.

The taxes imposed by the states and towns fall almost entirely upon native production, because real estate, outside of the state capitals and industry, contributes very little. Nearly always a double tax is levied on an article shipped from one locality to another: a tax upon leaving and another upon entering; and in addition a state tax when the origin, or destination, is a state capital. This taxation increases costs but is necessary to finance municipal governments. Unfortunately the present regime allows the burden of taxation to fall entirely on the producer, that is, on the one who feels it most.

Land valuation and establishment of a sound industry, with a consequent improvement in business transactions, will give the local governments sufficient means for their support, leaving untouched the initial stages of production. Reversion of taxes now assessed on producers would result in a trade revival brought about by increased exports.

Those abuses of inexperienced laborers by exploiters, people who unfortunately receive the lion's share, must be terminated once and for all. An end to the sort of business in which labor figures merely as a balancing of the books would realize a better income for the laborer and new investment of capital. The new system would not cause financial loss in any prior agreement because that could be guaranteed until fulfilled. Money in itself has no value, and the necessity of making it produce will cause a search for new investments, perhaps in fields of harder work but yielding results that are more sound and real for both the country and the individual.

Finance. Credit is the basis of economic prosperity of the people. Its absence causes stagnation of all industrial and commercial activities. The almost nonexistence of credit in the region under discussion contributes to the series of factors hindering progress.

Bank deposits are not large, yet they are considerable in comparison to the relatively

[13] This may be explained by the fact that the people of the forest have little or no money. Their commercial transactions are largely conducted by barter, as has been indicated above.—W.A.A.

small importance of the commercial exchanges. However, money is not lacking for bank transactions as indicated by the small rate of interest paid. Investors have little confidence in promoting products liable to fluctuate in value or subject to other unpredictable influences.

Landholdings cease to be a sufficient guarantee when they reach sizes larger than some countries. Nearly all commercial transactions are based more on personal credit than on land ownership. All operations are made on short terms and at a rate of interest that does not invite investment of large sums of money in an industry with delayed returns.

Only the state can assume the responsibility of financing the development of this region, following a plan whereby the present system of exploitation would be slowly replaced by a beneficial one of agriculture.

The Bank of Brazil administers a specialized agreement supported by law to aid agriculture and industry, but apparently the scope is not broad enough to apply in this region where special legislation should be in effect. Fundamental differences of the Amazon make inapplicable the laws suitable for the rest of the Nation.

The red tape and formalities are so discouraging that few people succeed in getting what they need. The limited number of banks, all located in the state capitals, makes borrowing difficult for the small farmer who lives in remote places and has little time or resources to go to them. Thus, a small loan must be made through various brokers, who absorb most of it in commissions. Consequently, evidence is completely lacking in the interior that the Amazon has had a period of great prosperity. Even the state capitals benefited very little from this former affluence. There was some extravagance, though not to the extent that has been claimed, and under present-day conditions a recurrence would be unlikely.

Even today trading in the more important products—rubber and Brazil nuts—is through commission merchants, known locally as *aviadores,* who sell consignments from the proprietors in the interior and dispatch goods ordered by the latter. Proceeds from sales are used to liquidate indebtedness and commissions previously agreed upon, while any balance remaining is credited to the proprietor. On a smaller scale the proprietor uses the same system in accounting with the men who work for him. Naturally such primitive business methods run counter to the general welfare. Progress can come only by raising the buying capacity of the inhabitants to permit a higher standard of living, instead of a mere subsistence level. The absolute dependence of the producer upon the capitalist is displayed in a number of ways, all of them detrimental for the worker, the producer, the government, and the general economy.

As yet the official institution of credit has done very little for the extractive industries and absolutely nothing to introduce agricultural methods.[14] In view of the peculiar needs of the region, the problem would be solved more logically by the distribution of essential equipment and consumer goods than by loans of money. However, such matters are more difficult than the mere handling of money and books and, consequently, have no place in a banking organization.

Progress could be facilitated by cooperative associations, but unfortunately the working people are not amenable to the idea. Probably they could be taught the advantages of this economic system through school groups, something that has not even been attempted.

There should be no sentimentalism or ulterior interests in conserving outmoded business methods. In this age the individual rights, no matter how strong or legitimate, should yield to the urgent needs of collective rights. A singleness of purpose should be maintained to establish a great collective organization and a true era of economic prosperity in the Amazon Valley.

The solution of this matter of financing is intimately linked with all the other problems. Consequently, a joint plan of action must be guided carefully by the principles pointed

[14] An exception is the recent agreement between the Banco de Credito da Borracha and the Instituto Agronômico do Norte to finance and establish model plantations of high-yielding and disease-resistant strains of rubber trees.—W.A.A.

out in this chapter. The results will be ineffectual unless the various defects are eliminated.

Health. Many have exaggerated the unhealthfulness of the Amazon Valley, but actually the time of plagues and epidemics has long since passed. The prophylaxis for malaria is so well known and easily administered that the disease affects only those who are heedless of elementary precautions. These measures are no different from those which must be observed in wet lowlands not so distant from the Capital of the Republic.

Naturally, in a region where water is the predominant element, represented by thousands of lakes and marshes of all sizes, mosquitoes reproduce on an unlimited scale. For this reason malaria is the great scourge, especially of the poor people. With a lowered vitality they are disinclined to work, and, consequently, careless observers unjustly accuse them of laziness.

The problem cannot be solved by destroying the innumerable breeding places of the anophele mosquito, but it can be approached by the radical treatment of the sick and the carriers to eliminate the sources of infection. Unless science discovers that the disease can be transmitted from other animals, this will be an effective means of control. It does not preclude the employment of chemical or physical methods already in use in other parts of the world. Also, biological methods should be sought through research on the natural enemies of the insect.

In addition to malaria, various other ailments common to the whole of Brazil contribute to the debility of the rural populations here. All are easily treated and can be avoided by simple sanitation and by a slight improvement in the standard of living for the individual.

To talk of leprosy as a problem is to ignore the great accomplishment of the Federal Government and the private organization directed by Miss Eunice Weaver in eradicating this disease from the country. The original program is being vigorously carried out and should soon attain its end.

Tuberculosis, another great reaper of lives, is more a social than a sanitary problem, being limited almost exclusively to the larger towns and cities crowded by poorly nourished people. The only hope for improvement will be better living conditions and education in proper food habits. Naturally the extent of this education will be limited by the local availability of milk, fruit, meat, and green vegetables, but the increased production of these foods will be a task for those concerned in solving the problem.

Only a highly successful campaign against undernourishment will reduce the present high infant mortality, which, according to official records, is largely the result of malnutrition. It must be admitted that living conditions are more than half responsible for the health of the population, and in a region where pauperism exists it is surprising to find neither a high lethal index for certain diseases nor recurring epidemics.

The inadequate hospital service is a serious matter. There exist for this vast territory only thirty-five hospitals and *casas de saúde* (nursing homes), with a total capacity of 3,549 beds.

One of the major factors hindering the achievement of any comprehensive sanitation program is the sparse population, widely dispersed in tiny settlements throughout the region. The tremendous amount of money needed for an adequate project would far exceed the financial ability of the country. An example of this can be shown in the costs for sanitation operations near Belém; in an area of about 2.5 square miles, where the airport and other installations of the SNAPP organization[15] are located, more than $12,500 has been spent without complete eradication of mosquito larvae.

The only expedient plan will be the selection of special areas for sanitation, but to attract settlers these areas must also offer some sort of remunerative employment. This again illustrates the complete interrelation of all aspects of the Amazon problem.

The magnitude of the problem should not weaken our determination even though a lifetime be needed for an ultimate solution. The errors of the past must be avoided, especially

[15] SNAPP is an abbreviation of the title—Serviços de Navegação da Amazônia e de Administração do Porto do Pará.

the selfish attitude of accomplishing no more than one's official duties. There should exist a deep conception of the eternity of Brazil.

Transportation. At first glance it might seem to a casual observer that transportation in the Amazon did not constitute a problem. The impression that all parts of the region are readily accessible because of the magnificent network of rivers is only partially justified. Various facts indicate the complexity of the situation.

To begin with, the rivers constitute, so to speak, merely the main roads, but there are no lateral branches to transport products from the zones spreading out from both margins. Such transportation cannot be accomplished economically because a capricious Nature has given an arbitrary orientation to all navigable tributaries such as creeks and draws.[16] Only man will be able to bring about the necessary changes, but the construction of land highways will prove very difficult.

The question of fuel is most important. Coal is too costly because there are no nearby mines, and so wood must be used instead. But this fuel presents a number of disadvantages. The supply is undependable because 60 years of woodcutting along the margins of the rivers has resulted in a scarcity of trees to fell. The lack of replanting has left only a skimpy second growth to feed the furnaces of the steamers for a greater part of their routes. Transportation costs are increased by the frequent stops to take on fuel and by the reduced cargo space. Wood has inferior combustion qualities and furthermore is usually water-soaked from the lack of shelters in the supply ports. Woodcutters are prone to abandon their hard work and take up more lucrative occupations whenever there is a price increase for native products. Because of this the crews and passengers of boats have at times been obliged to cut enough wood to proceed on their journey. The recent increase in rubber prices will make the situation even worse.

Another factor contributing to the irregularity of the transportation schedule is the dispersion of small settlements along the rivers, some even in the headwaters where the depth of water is slight during a good part of the year. The Territory of Acre, one of the three administrative divisions of the Amazon, suffers most from inadequate transportation during the dry season. Thus, cargo and passengers bound for this region are subjected to tedious delays and reshipping, which are reflected in greatly raised costs of all goods consumed there and in ruinous economic effects.

Whoever takes the trouble to read the manifest of a ship in transit will be surprised at the great number of ports of visit, for many of which only a single parcel is delivered. To know that a ship may lose as much as three hours in delivering this package better explains what navigation on the Amazon is really like.

All this indicates the importance of more accessible populated centers and the adequate development of agricultural wealth. The unstable production of the past has discouraged measures that might have been taken to improve the situation. Conditions will not change until a uniform and dependable volume of cargo can be assured.

The number of ships in traffic has been decreased in the past 20 years by 140 vessels of varied tonnage. Nearly 80 were lost in accidents, and the remainder were sold to other parts of Brazil or to foreign countries. The need for these ships is most acute at the present time.

However, the situation is being improved somewhat by the activities of the SNAPP organization, recently created by the Federal Government to take over the duties of the English companies which operated in the region during the last rubber boom. The law was designed to protect the Amazon Valley by conserving the profits accruing from transportation of its products. The policy is well expressed by giving the new organization wide authority in applying all revenues for the benefit of the Amazon proper. This arrangement has produced some notable results in spite of the mounting difficulties in securing essential foreign materials now controlled by international agreements. An

[16] In the original text there appears the word "igarapé," an Indian word meaning literally "canoe road"—W.A.A.

example has been the construction of one ship of a badly needed type, using native materials in so far as possible and only Brazilian workmen.

All the ships in actual use are old, and their constant repair is a perennial expense which has to be deducted from the small profits. No satisfactory navigation service can be maintained under these conditions without an appreciable deficit. The distances to be traversed are enormous, and practically all the provisions to be consumed during the voyage have to be stocked in the base port. A sudden accident can lay up a ship for many days until repair materials can be brought from some city along the route.

The ship is the only link between civilization and the Amazon jungle, and the cities and towns are as isolated as islands in the middle of the ocean.

Only three short railways are found in the Amazon region. All are situated at the margins of the Valley as follows:

Madeira–Mamoré (235 miles), connecting Porto Velho on the Madeira River with the Guajará Mirim on the Mamoré. This serves Bolivia better than it does our country. The construction work was a veritable epic, and hundreds of crosses along the way mark the struggle of man against Nature.

Bragança (186 miles), which connects Belém with the town of the same name almost at the edge of the Atlantic. The elevated, dry lands along the line are suitable for cereals, but production has been insufficient to bring any improvement of the railroad, which needs to renew its now quite dilapidated rolling stock.

Tocantins (56 miles), which was planned to avoid the dangerous navigation of the Tocantins River. In spite of an immense quantity of abandoned material, the construction of this railway has been paralyzed for more than 20 years. The reasons are not yet well understood, but they would seem to be the same as those which prevent the opening of a highway in the same region.

Attempts already initiated for a road across the Tocantins Valley have been systematically defeated, apparently by ulterior interests of proprietors of large landholdings. They fear the loss of profits derived from their monopoly, which binds the people to the present barter system. To bring progress to the Amazon will require grim determination because man's obstructiveness is often greater than Nature's.

The development of highways is almost nothing, there being less than a thousand miles. The roads are constructed of earth with no top dressing, and during the rainy season they are practically impassable except through constant repairing, which is usually neglected.

River transportation is inseparably united with the future of the Amazon; it cannot be otherwise. In fact, some viewpoints are so deepseated as to consider this to be the only problem of the Valley. A careful study will show this to be not entirely true; navigation constitutes a problem only because it has been made so by other phases of the Amazon question.

The need for a better fuel causes most concern at present. Perhaps the petroleum in the Amazon Basin, unfortunately not yet found in Brazilian territory, will eventually solve this difficulty. The Amazon is the only outlet for the immense deposit discovered along the banks of the Pachitea, a tributary of the Ucayali River in Peru, but the oil will be used primarly to benefit the region where it is located. To bring this oil to Brazil will present great difficulties, not only those of Nature but also those brought about by the world oil trust.

Establishing permanent trading posts to reduce the infinite number of stops for the ships would save time and fuel wasted in the present erratic routes.

The renovation of the fleet, impossible at present, must be done in the not very distant future, because the great majority of the ships have reached the limit of usefulness. Certainly the future iron industry of the Nation will produce the material necessary for the complete reconstruction of the fleet, thus permitting the return of the golden age when of the 303 ships registered in Brazil, 220 were plying the waters of the Amazon. This past magnificence must return to the Amazon, but only an economic stability can make it permanent.

Communications. Establishing a chain of rapid communications connecting all the scattered towns is most vital for the economic and social structure of the Nation and should merit serious consideration on the part of administrators of public affairs. Intercommunication is fairly well developed in the southern and central parts of Brazil but is still quite deficient in the Amazon.

This is especially true of the telegraphic systems. A cable line is limited to the main towns along the river proper, while radiotelegraphy serves only a few cities. A land line is maintained with great difficulty due to frequent breaks in the wires caused by falling trees.

The situation can be improved only by a wider use of radiotelegraphy, and a good step in this direction has been the installation of stations on all the SNAPP ships. However, this service is somewhat inadequate because the ships are constantly on the move. A much better plan would be the gradual installations of stations in all sizable towns.

The use of home radios, of course, presents great advantages, but this must wait until a later period when the people will be prosperous enough to buy the receiving sets.

Postal communications increased immensely with the introduction of air routes, but only the more important cities have been helped, the smaller places still remaining isolated, many of them having no contact with the outer world for long periods. Boat mail can no longer be considered adequate in this hurried world where minutes are worth money.

In the event that the commercial air lines were found to be incapable of supporting themselves, then the military air service could be called upon for aid. Landing fields are still too few to permit any great extension of lines, but the larger towns could be lawfully obliged to clear land and prepare suitable runways.

Any program of development for the Valley must depend essentially upon rapid communications to coordinate the various aspects of labor, sanitation, and settlement of people in the new towns to be created.

The Amazon is extremely isolated from the rest of Brazil; aside from the telegraph it can be reached only by boat or by airplane. Whatever enterprise is undertaken to end this situation will render great benefits, not to mention military advantages.

In this hurried age when the world can be encircled in a few days it becomes imperative that swifter and more efficient systems of communication be brought to the Amazon; to the people who have had so little and who, until recently, have been so scorned.

Social relief. Among the functions of the modern state social relief stands out most prominently because it is dedicated directly to the individual or family. This new term, now widely known, signifies one of the most noble tasks to which a public or private organization can devote itself. Social service is expressed by various means and forms in the larger cities of Brazil but practically does not exist in the Amazon. Aside from the small religious organizations, limited in scope because of their scanty resources, nothing else is found which can be called social relief. The absence of great fortunes, the disinterest of man for his less favored brother, and the insignificant revenues of civic governments are the prime causes of this situation.

A great impetus in the general work of sanitation and education would come in the creation of a mobile corps of social workers, preferably constituted of women who could be recruited easily and rapidly. This organization, perhaps having a semimilitary form, would have as its work the dissemination of general instructions on hygiene, prenatal and postnatal care, home planning, and the assembling of family records for a better understanding of living conditions.

In addition, a multitude of small but indispensable services could be performed in helping the individual to feel less crushed by the immensity of the jungle and to have a desire to improve his way of living. The inhabitant of the Valley, for reasons already known, is indolent and listless. He leads an almost vegetative existence, when instead he might have, if not for himself at least for his children, comfortable surroundings and prosperity from the vast riches which Nature has placed at his disposal.

For this and other reasons, it is imperative that the rights of the working man be protected so that he will not be subject to the exploitation so frequently practiced by those who are not ashamed to take advantage of a faulty legislation to accumulate riches at the cost of misery among their fellowmen. Economic wealth and continuous welfare service can be expected only from well-organized labor, truly oriented along technical and social phases. It is precisely in the Amazon that man, because of his isolation, receives least benefit from the existing social laws. The New State designed these laws to protect the worker, to keep him from being used as a mere tool, which is thrown aside once it becomes old or useless. Provisions which are beneficial in other parts of Brazil are not necessarily applicable here because different conditions need different legislation. A statute should be established immediately to regulate the relations between capital and labor in the Amazon and to bring better enforcement of the law for the protection of the working class.

Lowering infant mortality, sending children to school, keeping workmen out of the tavern, stimulating the organization of the family in legal molds, and teaching patriotism are other tasks which can be performed by the social service to bring real benefit to the abandoned people in the greater part of the Amazon. For the success of the program there are necessary the special gifts of abnegation and devotion, found only in women. Doubtless many of them are merely waiting for an opportunity to serve their country.

A Protestant missionary organization, fully understanding what is most practical for propagation of its ideas in this region, has adopted these methods in part. Using appropriate boats, the missionaries go in search of future converts, traveling the swamps and streams to carry spiritual comfort, medicines, and instruction. The results have been magnificent, and only scanty finances limit these activities so worthy of applause and imitation.

In this critical moment in the history of humanity, when a universal catastrophe threatens to subvert the foundations of society, there is great uncertainty for the future. Living conditions become more acute day by day. Now would be the time to encourage and befriend the unfortunate and destitute to help them resist the avalanche and to construct a smiling future within a great Brazil.

The Solution

Unity. Since, properly speaking, there are not several Amazon problems but rather a single one, likewise one general solution is possible and not individual ones. All the phases of the question are so closely interrelated that only one plan of action, directed by a single organization, will be likely to succeed.

Any specialist, facing the problem independently, is liable to make narrow interpretations which disregard other aspects of the subject; consequently, his conclusions will be unsound and of no value in solving the problem. Of course, each facet of the great general problem should be studied by specialists, but only a peculiar supervising organization of wide vision and autonomy could determine the proportion of work which fits each of these elements. The different operations must be so articulated as to transform the present insecurity into a completely stable condition.

Apparently, then, the best organization would be so integrated as to allow solution of individual questions by subordinate though independent offices; but with all conclusive data channelling through the supervisory board for final interpretation and application. In this way the programs of the separate units could be welded into a plan of unified action to attack the peculiar problem of the Amazon with all its interdependent and correlated phases.

Nothing is to be gained by a sanitation program in a region which can have no towns, and much less by directing a migratory current to a place that is not healthful. To arrange transportation for a zone which produces nothing is equal to increasing production in an area that has no transportation.

These remarks should not be interpreted as destructive criticism against the efforts now being made. The authorities in charge ap-

parently have a widely constructive viewpoint and are fired with determination to bring forth impressive and permanent results, but without a strict and essential coordination the efforts will be fated to complete failure in so far as the rebirth of the Amazon is concerned. There can be seen a considerable dispersion of forces in the existing multiplicity of administrations, organizations, services, and plans, when better advantage would come by directing all into one united action.

In this period of enormous sacrifices we are in no position to continue and much less increase unprofitable expenditures; every cent invested by the public treasury must be transformed into something useful to the general welfare.

An efficient administrative service for the Amazon absolutely cannot be located outside of the region. Furthermore, it is something which only the Federal Government can undertake. Uniformity of legislation can be imposed by means of a special statute to regulate fiscal or administrative activities which might interfere with the development of the organized plan.

A decisive moment has arrived in our economic history, and never before has so much been spoken or written, true or false, about the Amazon in relation to this subject. The entire region, faced toward the future, with its eyes on the Chief of the Nation, awaits anxiously and confidently for the fulfillment of the promises which have been made.

Technique. In choosing plans of attack a knowledge of the subject is essential, but in final analysis the method of applying the plan will count most. Nothing is gained by arguing that this or another plan is the better way of solving a particular case, because there yet remains the task of translating the plan into action to achieve the desired result. Since the Amazon differs in all aspects from the rest of Brazil, it is logical that the courses of action to be adopted shall have to be different also.

These methods would constitute what might be called a special Amazon technique and are not to be understood by making a rapid trip through the region or by reading a few books written by people who likewise paid brief calls. All the factors outlined in the first part of this work must be considered, and all the causes of the complex questions must be understood perfectly. Prompt action must be taken to correct mistakes and failures apt to interfere with the program, no matter how perfectly conceived. The powerful interests involved within the intricate economic mechanism will cause conflicts, but the frequency of such clashes depends upon the steps taken to anticipate them.

Immediate results should not be expected, because they will not materialize. The man chosen to direct the program should be inherently patient. The seed is planted only after the soil has been prepared, and the final harvest depends upon the care given to the different stages of growth.

The foregoing explanations should indicate the futility of any composite plan that attempted to cope with this vast territory as a whole. Knowing the diverse questions, and the agency specially qualified to solve these questions being established, the next step would be to select a limited number of zones for the initial trials. The results from these experimental areas would furnish sufficient experience and data to extend the operations progressively to other parts of the Valley.

The selection of the test areas should not be influenced by politics. The best criteria would be guided by the following:

1. To be easily accessible for year-round navigation.
2. To have a reasonable density of population.
3. To have abundant natural resources.
4. Not to be subjected to periodical flooding.
5. Not to be inhabited by savage Indians.
6. To be centrally located in the geographical divisions of the Valley.

Once the zones are established, attention should be given to the primary objectives with due thought for immediate consequences and alternatives in order to deal promptly with any emergency that might arise.

The experimental plantings in the Ford concession on the Tapajós River, even though confined to the single specialized crop of rubber, can serve for observations of methods

already tried. The mistakes made there can be avoided in the future; they caused the loss of millions of dollars and much precious time, neither of which can be afforded by us.

The experience and observations of the native are not to be scorned but should be carefully examined to eliminate useless or harmful details.

After the first objectives, the subsequent ones should have time limits set for their completion, which will be realized in proportion to the degree of efficiency attained.

One of the principal conditions for success calls for an organization with perfectly harmonized functions, where every member comprehends thoroughly the desired ends in order to proceed intelligently and not as a mere machine.

To advance some services ahead of others might result in a useless loss of time and money. Thus, laborers should not be sent to places where the supply of provisions and tools is uncertain because of transportation. It is important that a regular supply of materials be previously assured because local resources are practically useless without some means of providing their movement from place to place. Suitable boats are not easily constructed at present, and consequently, until this deficiency is remedied, all transportation will have to be adjusted to the capacity of available craft.

Few, or perhaps none, of the enterprises already achieved in the world can serve as a model for the one now being envisaged and discussed; consequently, the lines of action should not be based on hard-and-fast rules. Progress should be made from simple, easily executed measures to more complex ones, avoiding the failures so frequently caused by unnecessary complications arising from an inadequate understanding of a situation. Evidently, then, one detail of the technique to be used indicates a guidance more by common sense than by dogmatic procedures, which are not applicable for a region still in the infancy of civilization. All the rest can be condensed into two simple words: *work* and *honesty*.

Determination. In overcoming a difficulty nothing is more important than a firm determination to master it. This determination is innate in the individual or else he acquires it from the interest taken in the problem to be solved. Thus, an important point for the success of any proposal for the renascence of the Amazon will be found in the choice of the men to direct the program.

When yellow fever threatened to invade the Amazon, in a form that would have wiped out all human life, the one man who knew what to do was Oswaldo Cruz. He was confronted by the task of extraordinary magnitude, but he accomplished it perfectly.

But for the indomitable energy of Pereira Passos perhaps the Brazilian capital would not now have its monumental appearance of which all Brazilians are so proud. The country's history contains names of many illustrious patriots who were able to complete the missions given them, despite all the difficulties encountered. Men of this calibre should be assigned to carry out the plans for the revival of the Amazon, because it will be the work of a giant and a task sufficient to require a lifetime.

It would be a shame to lose more precious time in the present situation by having an incompetent leader, whether by indecision, indolence, dispersion of activity, or above all by disinterest in the subject, either from ineptitude or ulterior motives.

Many individuals still hope to continue the *status quo,* some for fear of losing a source of easy riches, others believing that any change might prejudice organizations already established. It is here that the mettle of the leader will be demonstrated in his ability to deal with the criticism and resistance certain to break loose when the first measure in favor of the general welfare runs counter to private interests.

Certainly no one could exercise such great powers without complete support from the Leader of our country. Likewise it is true that the one chosen should be deserving of the trust placed in him, directing into a single channel all his creative and constructive ability, without hindrance from matters of secondary importance, keeping ever in view the supreme goal to be reached—*The Good of Country and the Aggrandizement of Brazil.*

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TO A REALM UNREAL

I wonder as I gaze at lowering sky,
Elusive mask to curvature of space,
Wherein dimensions lie, three straight and time,
What spirit guides the cosmic realm and why.

I linger on and let my dream thoughts ride
The ether waves that penetrate their course
Unto dimensions' end, then forced to bend,
Righted by the guiding hand of Heaven,
There come whisperings that tell of unknown worlds.

In dreams I build a scaffold to the sky,
I climb its outer dome and listen to
The music of the spheres as they go by;
I tread the golden cupola and try
To put to test my childhood's sense of Heaven.

Reverberating clash then spills my vision,
Realities my reveries dispel,
Space-time, unreal, is but a fashioned frame,
Unmeasured rounds of Heaven, bounds of Hell.

—W. B. PIETENPOL

DEPARTMENT OF PHYSICS,
UNIVERSITY OF COLORADO.

UNITED STATES RESTRICTIONS ON ARGENTINE BEEF

By EARL B. SHAW
STATE TEACHERS COLLEGE, WORCESTER, MASSACHUSETTS

ONE of the greatest causes for friction between the United States and Argentina arises from our refusal to lift restrictions on the import of Argentine beef. The main reason given by American cattlemen is that Argentine cattle are affected by foot-and-mouth disease, a highly contagious disease whose effects are greatly feared in the United States. Argentine cattlemen say the real reason for American restrictions is the fact that American beef producers fear competition from Argentine beef on the American market, and to avoid this competition they hide behind *aftosa* (Spanish for foot-and-mouth disease) merely as an excuse for excluding Argentine meat. The writer will examine the merits of the two view points on the following pages.

Foot-and-Mouth Disease. Foot-and-mouth disease, endemic in Argentina, is one of the most widely spread diseases attacking cloven-footed animals. The United States Department of Agriculture lists the following countries in which the disease exists:

Albania, Arabia, Argentina, Belgium, Bolivia, Brazil, Bulgaria, Burma, Ceylon, Chile, China, Chosen, Czechoslovakia, Denmark, Ecuador, Federated Malay States, Finland, France, Germany, Great Britain, Greece, Hungary, India, Indochina, Iran (Persia), Iraq, Ireland, Italy, Luxembourg, Netherlands, Northern Ireland, Norway, Palestine, Paraguay, Peru, Philippine Islands, Poland, Portugal, Rumania, Spain, Straits Settlements, Sweden, Switzerland, Syria, Thailand (Siam), Turkey, Union of Soviet Socialist Republics (Russia), Uruguay, Yugoslavia, all countries on the continent of Africa other than the Union of South Africa, . . . (Bureau of Animal Industry Order 373, October 26, 1940.)

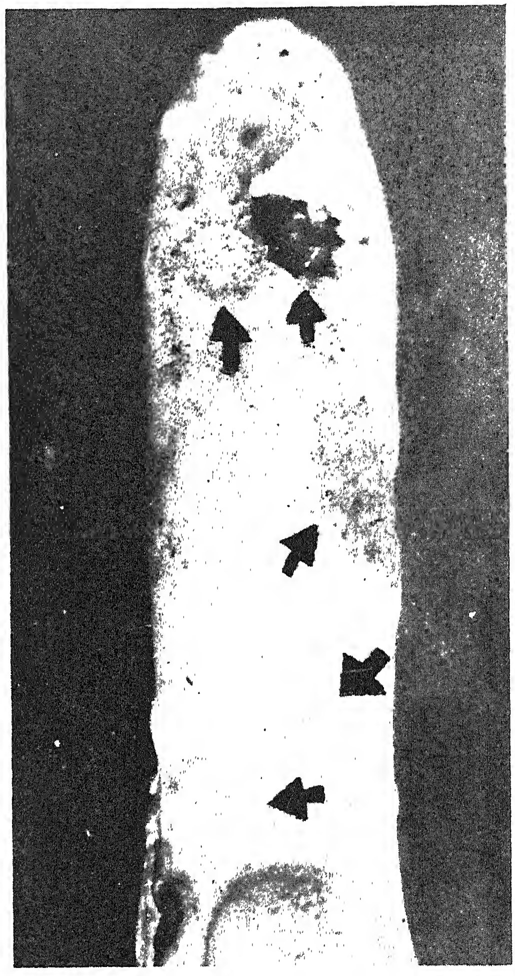


FIG. 1. INFECTED TONGUE[1]
FROM A COW WITH BAD CASE OF FOOT-AND-MOUTH DISEASE. THE DARK AREAS ARE VESICLES. THE ONE ON THE LEFT WITH FLAP IS FRESHLY BROKEN.

Foot-and-mouth disease is characterized by an eruption of blisters or vesicles (Fig. 1) on the mucous membrane of the tongue or tissues of the mouth, on the skin between and above the toes of the feet (Fig. 2), and on the teats and udder (Fig. 3). As a rule vesicles rupture within a day leaving a raw, red surface. Excessive salivation occurs in cattle. Lesions heal rather rapidly, but in some instances those of the feet may give rise to serious secondary bacterial infections. Be-

[1] Figures 1–3 are published by courtesy of the Bureau of Animal Industry, U. S. Department of Agriculture; figures 4–8, by courtesy of Ministerio de Agricultura de Nación, Dirección de Propaganda y Publicaciones, Argentina.

FIG. 2. FOOT LESIONS
THE ULCERATION OF THE TISSUE BETWEEN THE TOES MAY EXTEND TO THE LIGAMENTS OF THE FETLOCK OR PRODUCE DISEASE OF THE JOINT OR OF THE BONE.

fore and for a short time after appearance of lesions a striking rise of temperature occurs, and during the attack animals lose considerable flesh. When cows contract the disease, a severe reduction of milk flow results, and cases of abortion are numerous among pregnant animals.

Mortality is usually light, often not more than 3 percent of the herd. Once in a while, however, losses may reach 30 to 50 percent as they did in certain European herds during severe attacks between 1918 and 1921. If the animal recovers, a time lapse of from ten to twenty days is the rule in mild cases, but the disease may linger from three months to a year in malignant form.

Financial losses from the disease may run into millions of dollars. In Germany during the attacks of 1920 and 1921 estimated damage was approximately $119,000,000. This does not include expense resulting from disruption of business caused by quarantine restrictions. In Switzerland losses during the same years reached $70,000,000. Swiss figures may carry fuller significance if it is noted that the number of susceptible animals in the republic at that time was only one-fiftieth the number in the United States; and that the area of Switzerland is less than one-half that of the state of Maine. The most severe outbreak in the United States was that of 1914. It cost about $9,000,000 to eradicate the disease by slaughtering 172,000 animals: 77,000 cattle, 85,000 swine, 10,000 sheep, 114 goats, and 9 deer. Other less costly outbreaks in the United States have occurred in 1870, 1880, 1884, 1902, 1908, 1924 (twice), and 1929.

The first epidemics in the United States were introduced by imported animals affected by the disease, but later infections were carried by some such means as hay, straw, halters, ropes, hides, hair, wool, or garbage. The 1929 occurrence in California was traced to garbage from a trading steamship docked at San Pedro. The ship had taken on fresh meat in a South American port.

The infective agent is a filterable virus present in the fluid and coverings of the vesicles, in saliva, milk, urine, or other secretions and in bone, meat, or blood of the affected animal. The virus does not always die rapidly. Definite evidence is available to show that in one instance the infective agent persisted in a field for 345 days; yet a

FIG. 3. BLISTERS ON THE TEATS.
IN CASES OF SERIOUS AFFECTION OF THE UDDER, TEAT PASSAGES MAY BE CLOSED, RESULTING IN A CAKED UDDER; AND TOXIC POISONING MAY ARISE FROM IT.

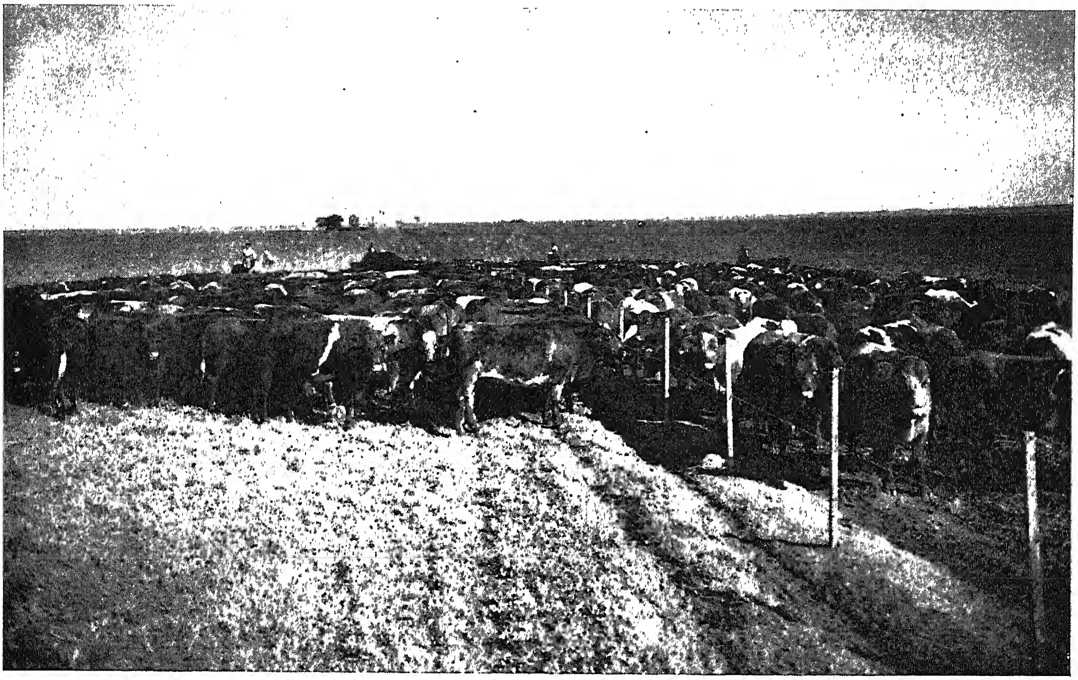


FIG. 4. HERD OF FAT CATTLE ON PASTURE IN THE ARGENTINE PAMPA
MOST OF THESE ANIMALS SHOW A SHORTHORN STRAIN, A BREED ACCOUNTING FOR MANY ARGENTINE CATTLE.

temperature of 140° F. (60° C.) will destroy the virus in from 5 to 30 minutes. In 1942, after many British epidemics had been traced to feeding unboiled swill to swine, a law was enacted declaring the feeding of unboiled swill or allowing animals access to raw bones or unwashed meat wrappers a statutory offense.

It is upon the question of length of life of the virus in frozen and chilled meat that difference of opinion exists. Felix J. Weil in his recent book *Argentine Riddle* states:

> British Foot-and-Mouth Disease Research Committee established that the virus does not remain alive over 42 days in the blood of frozen carcasses nor over 76 days in the bones. Consequently, a quarantine of 21 days for imported boneless meat, added to the 22 or more days it takes to bring a shipment from Buenos Aires to New York, should be more than sufficient precaution.

Mr. Weil does not tell the entire story. The Second Progress Report of the British Research Committee shows that virus remained alive in bone marrow up to 87 days, in kidney 83 days, and in guinea-pig pads 102 days. Other work reported by the Committee (Fourth Progress Report) demonstrated survival of the virus at $-2°$ C. for 82 days in tongue, 99 days in liver, 96 days in kidney, 107 days in bone marrow, and 145 days in tendon. Continuance of the work with larger numbers of animals slaughtered in the infective stage of the disease and more numerous inoculation and feeding experiments may show longer periods of survival.

From the above statements it is evident that foot-and-mouth disease research is active. Owing to the great infectiousness of the disease, experiments are not conducted within the United States. Experimental work by the U. S. Department of Agriculture has been done in foreign countries by arrangement with their veterinary and other public officials. Various vaccines have already been discovered, and some of them are said to be highly successful. No doubt, in time, vaccines will be found which may prove as great a boon to the world beef industry as present-day hog cholera vaccines and serums are to the swine industry. In spite of research progress, however, more evidence seems needed on the life of the virus in

boned, frozen, and chilled beef before shipments from infected areas may be admitted safely into the United States.

Noninfected Areas of Argentina. Argentine cattlemen would probably object but little to quarantine against meat coming from those parts of their country where foot-and-mouth disease is endemic. But they do object strenuously to a blanket sanitary barrier against the whole nation. Prior to 1930 the restrictive sanitary measures against importation of chilled and frozen meats from countries affected by the disease applied only to *regions* within such countries where infection was known to exist. With the coming of the Smoot-Hawley tariff act, however, embargoes were applied to every part of a country in which infection exists or which has been exposed to infection, even though well-defined areas of the country are known never to have been affected with or exposed to foot-and-mouth or any other objectionable disease.

In a letter to Senator Key Pittman, Chairman of the Senate Foreign Relations Committee in August 1935, ex-Secretary of State, Mr. Cordell Hull, had the following to say concerning infected regions and countries:

> A serious barrier to international trade is found in sanitary measures which restrict trade more than is necessary to accomplish the purposes for which they are imposed. For example, on Jan. 22, 1929, the United Kingdom placed an embargo on animals, hay, straw, and alfalfa meal imported from Colorado. The reason given for the embargo was an epidemic of foot-and-mouth disease then prevalent in California. Protests were made and following the determination of the fact that the disease was restricted to California, the embargo on products of Colorado was lifted in February of that year. On May 2, 1932, because of an outbreak of foot-and-mouth disease in California, an order was issued in the United Kingdom prohibiting importation of live ruminants, swine, hay and straw from any part of the United States. On May 21, it having been discovered that the outbreak was local to California, an amending order was issued limiting the prohibition to products originating in California. In the meantime, however, the application of the prohibition to the whole of the United States had acted to the detriment of the non-infected districts. I cite these

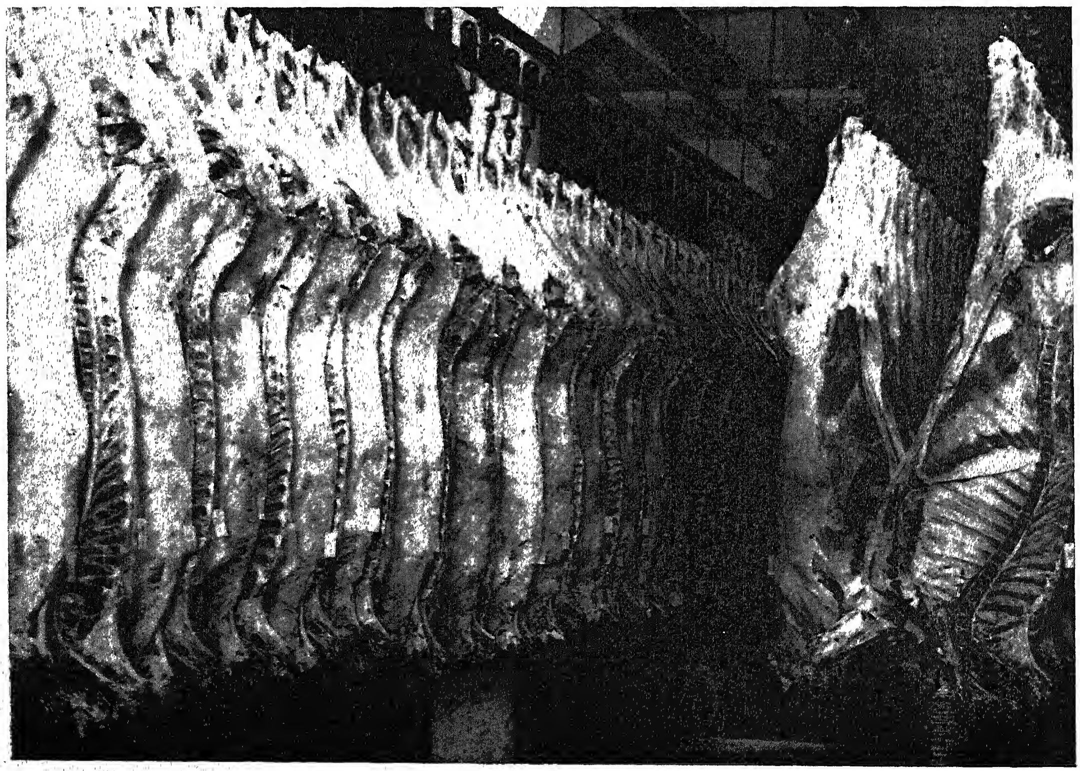


FIG. 5. CARCASSES COOLING IN AN ARGENTINE PACKING PLANT

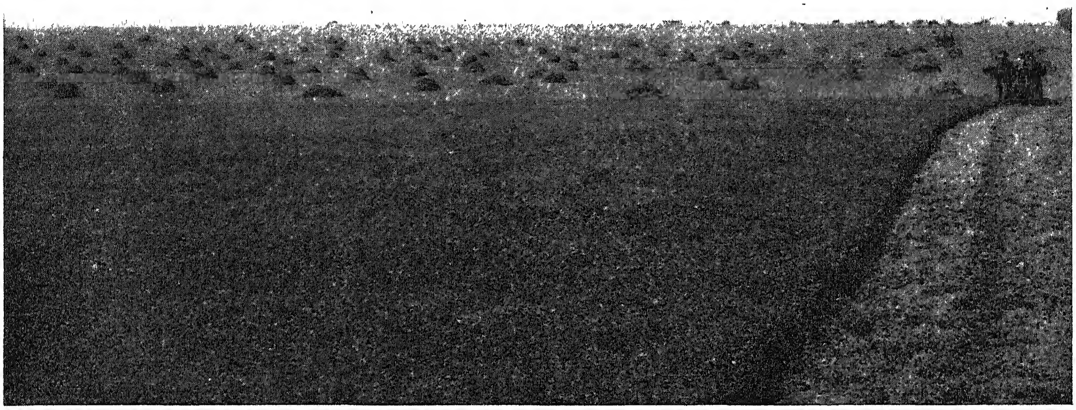


FIG. 6. CUTTING ALFALFA HAY ON THE PAMPA
MOST CATTLE ARE FATTENED BY PASTURING ON ALFALFA FIELDS, A MARKED CONTRAST TO U. S. CORN FATTENING.

examples to show the harmful effects of embargoing products of a whole country when only a part of that country is subject to infectious or contagious disease. It is difficult to make representations against such practices by foreign countries when we ourselves have a section of the Tariff Act which appears to foreign countries to be a glaring example of such legislation. Our own practice is even less defensible than the examples I have cited, when, as in the case of Argentina, the embargo on meat applies against a large section of the country which our own experts hold to be free from foot-and-mouth disease.

Article Three of the Sanitary Convention of 1935 takes care of the objectionable word "country" in the Smoot-Hawley Tariff Act of 1930. "Country" is the word replacing "region" of the previous sanitary enactments. Article Three reads as follows:

Each Contracting Party recognizes the right of the other party to prohibit the importation of animal or plant products originating in or coming from territories or zones which the importing country considers to be affected with or exposed to plant or animal diseases, or insect pests dangerous to plant, animal or human life, until it has been proved to the satisfaction of the Party exercising such right that such territory or zone of the other Party is free from such contagion or infestation or exposure to contagion or infestation. Neither Contracting Party may prohibit the importation of animal or plant products originating in and coming from territories or zones of the other country which the importing country finds to be free from animal or plant diseases or insect pests or from exposure to such diseases or pests, for the reason that such diseases or pests exist in other territories or zones of the other country.

Ratification of the Sanitary Convention Between the United States and Argentina, signed May 24, 1935, seems advisable. Argentines assert that former President Roosevelt, while on a visit to Argentina in 1936, promised Senate ratification. The Senate failed to ratify.

Foot-and-mouth disease is endemic to the Pampa, the great meat-producing region of Argentina (Figs. 4, 5, 6, 7). However, the disease usually occurs in a light form, the reason being, according to some authorities, that the cattle are grazed outside throughout the year. This is possible because of the mild climate, characteristic of the east-coast, subtropical location. Patagonia and Tierra del Fuego have been declared free from infection by Argentine officials. These two regions produce a large fraction of the sheep (Fig. 8) raised in Argentina and also account for a

few cattle. Acceptance of shipments of mutton and beef from southern Argentina would show Argentine livestock men that we are not hiding behind sanitary embargoes to keep out competition.

Argentine Canned Meat. As previously stated, a temperature of 140° F. applied to infected meat will destroy the foot-and-mouth disease virus. Meats for canning are heated beyond this temperature, and so it follows logically that no danger of infection exists from importation of Argentine canned meats. On more than one occasion, lobbying in United States Congress has defeated the purchase of canned meat from Argentina. Specifically, Argentines call attention to the Senate's failure to approve the Navy's purchase of a small quantity of Argentine canned beef in 1939 despite the fact that such canned beef was not produced then in the United States and that the Argentine product is thought by many, the writer included, to be the best and cheapest in the world.

There seems to be no legitimate reason why Argentine canned meats should not enter the United States tariff-free or under a reasonable rate if tariff seems advisable. The United States consumer would surely gain from large shipments in such trade, for it is an established fact that during normal times a large percentage of the United States population suffers from an insufficient meat diet, owing in no small degree to the high price of American beef.

An import of but 2 percent of our annual meat consumption—2 percent made up of canned meat and frozen or chilled meat from Argentine regions known to be free from infection—would take nearly a quarter of Argentina's present meat export and lay the basis for significant shipments of American goods in exchange.

Evolution of Argentine—U. S. Friction Over Trade in Beef. Friction over Argentine beef is of comparatively recent origin. Prior to 1880 little cause for controversy existed, for until artificial refrigeration became

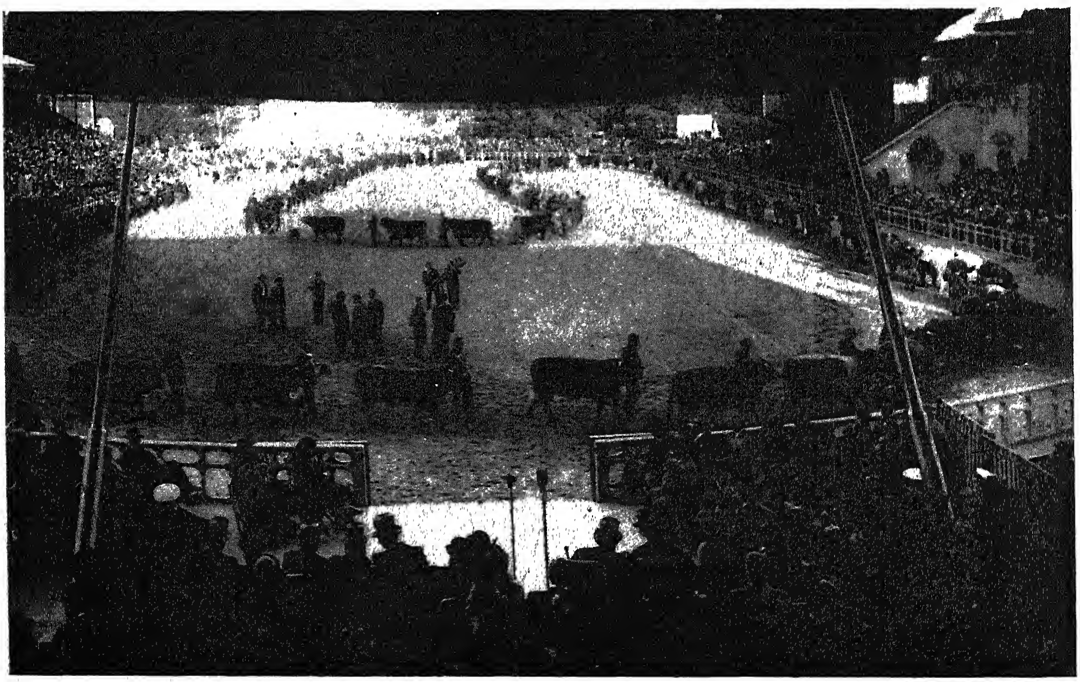


FIG. 7. THE GRAND PARADE OF PRIZE WINNING CATTLE

AN ANNUAL LIVE STOCK SHOW HELD AT PALERMO PARK, BUENOS AIRES. THE GRAND CHAMPION BULL OF 1925, WHICH SOLD FOR MORE THAN $50,000, DIED A FEW MONTHS LATER OF FOOT-AND-MOUTH DISEASE.

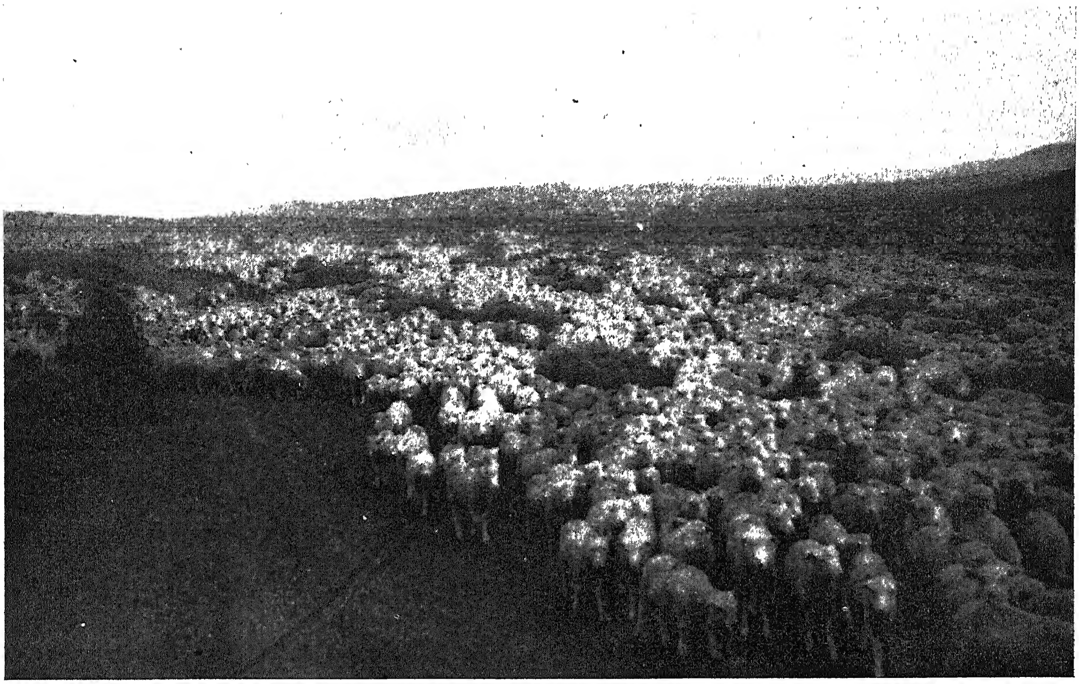


FIG. 8. LARGE DROVE OF MERINO SHEEP IN ARGENTINE PATAGONIA
THE MERINO IS BETTER ADAPTED TO THE ARID PATAGONIAN ENVIRONMENT THAN OTHER BREEDS OF SHEEP.

practical, neither country was able to ship meat with much profit to European markets. The United States was first in making practical use of refrigeration, as it was a simpler problem to ship beef across the temperate latitudes between our Eastern Seaboard and Europe than to forward it across tropical seas between Argentina and Britain. By 1900, however, both countries were competing for foreign markets. This competition did not last long, for growth of population in the United States and per capita decline in beef production in that country finally settled the issue in favor of Argentina.

Paul O. Nyhus in "Argentine Pastures and the Cattle Grazing Industry" (*Foreign Agriculture,* January 1940) gives an interesting comparison for United States and Argentine beef exports. In 1903 United States shipments of frozen and chilled beef to England reached 298 million pounds. Although showing a declining tendency these exports were fairly well maintained until 1907; but in 1908 they fell off sharply and continued to drop until in 1912 they practically disappeared.

Argentine export trade followed an opposite direction. Exports increased from 3 million pounds in 1895 to 53 million pounds in 1900. After that date, expansion was rapid: 330 million pounds in 1905; 548, in 1910; 1,071, in 1918; 1,615, in 1924. From the high level of the period 1924 to 1927 exports declined in 1928 and reached the low average of 839 million pounds for the years 1933–1935. In 1943 beef shipments totaled about 1,202 million pounds.

During the 1930's Britain placed Argentine beef under quota restrictions. Irked by these barriers, Argentina looked elsewhere for a market. The United States was a possibility, but tariff walls and sanitary embargoes have been far worse than in Britain.

Conclusions and Trends. It seems clear, in view of the highly contagious and dangerous character of foot-and-mouth disease, that American cattlemen should be extremely vigilant in keeping the dreaded infection from the United States. Sanitary barriers against animals and meat from regions infected with the disease should be strictly

enforced. On the other hand, it seems just as clear that injustice exists when Argentine livestock raisers in noninfected regions are prohibited from selling meat to the United States. In the last analysis the real basis for present friction over the meat industry is geographic. Location of the two countries in similar latitudes encourages similar production, similar production accounts for trade rivalry, and trade rivalry leads to international friction.

Some years hence, if we do not adjust our differences over the meat trade in the meantime, Argentine beef surpluses will probably cease to cause friction. It is not generally realized that Argentina consumes locally about three-fourths of her meat production. Her consumption in a recent year was 276 pounds of beef, 17.2 pounds of mutton, and 15.2 pounds of pork per capita. With only fourteen million people on a million square miles, Argentina is an underpopulated country. The nation could easily support two or three times the present population. Intensification of agriculture and further industrial development, changes which are likely to come, will encourage population increase (cf. SM, April 1945, p. 257). This in turn may expand beef consumption to a point where little surplus will be left.

Excerpts from B.A.I. Order 373

(The Law on Foot-and-mouth Disease)

. . . the importation into the United States of cattle, sheep, or other domestic ruminants or swine or of fresh, chilled, or frozen beef, veal, mutton, lamb, or pork, from the countries above named [p. 101] is prohibited.

No fresh, chilled, or frozen meat or meat product derived from wild ruminants or wild swine, originating in any country named [p. 101] shall be entered into the United States.

No fresh, chilled, or frozen organs, glands, extracts, or secretions derived from domestic ruminants, or swine, originating in any country named [p. 101] shall be entered into the United States except for pharmaceutical purposes.

Any animals, meats, organs, glands, extracts, or secretions specified [above] offered for entry and refused admission into the United States, shall be exported by the consignees thereof within 10 days or shall be destroyed in accordance with the directions of the Chief of the Bureau of Animal Industry.

No cured meat or product [this does not include any meat or product in hermetically sealed containers which has been sterilized by heat] derived from ruminants or swine, originating in any country named [p. 101] shall be entered into the United States unless the following conditions or requirements shall have been fulfilled: (a) All bones shall have been completely removed in the country of origin. (b) The said meat or product shall have been thoroughly cured by the application of dry salt or by soaking in a solution of salt. (c) The said meat or product shall have been held in an unfrozen, fresh condition for at least 7 days immediately following the slaughter of the animals from which it was derived.

No garbage derived from meats or meat products originating in any country named [p. 101] shall be unloaded from any vessel in the United States or within the territorial waters thereof: *Provided, however,* That such garbage, when contained in tight receptacles, may be so unloaded for incineration or proper disposal otherwise as directed by the Chief of the Bureau of Animal Industry, or it may be so unloaded under the direction of an inspector of the Bureau of Animal Industry for transportation beyond said territorial waters for the purpose of dumping.

No dressed poultry offered for importation into the United States from any country named [p. 101] shall be allowed entry unless the feet of such poultry have already been removed at a point above the spur or spur core, or are removed and destroyed or disinfected at the port of entry as directed by the Chief of the Bureau of Animal Industry. Such removal and destruction or disinfection shall be accomplished by the importer or his agent. . . .

AN ENGLISH PICTURE OF THE UNITED STATES

By WILLIAM LAAS

IN THE spring of 1943 the Ministry of Information of the British Government published a curious map of the United States that should be prized as an object lesson for atlas makers. The cartographer rarely receives from his ultimate consumer, the general public, a report on the effectiveness of his methods. Here for his inspection is a permanent record of the strange things that happen when people look at maps.

The British MOI, which roughly corresponds to the U. S. Office of War Information (OWI), produced this map as a small poster, 15 inches by 10 inches, for display in "pubs" and other rendezvous of the populace. From the title, "A Map of the United States of America,"[1] the lavish display of red, white, and blue, and the pointed selection of this nation's most superlative features, one infers that it was an effort to paint a reassuring picture of a great ally, at a moment in history when curiosity about the U.S.A. was intense throughout the United Kingdom.

The poster is commercial art rather than a serious job of cartography, apparently a quick tracing from some standard atlas. Thus it is, in effect, a lay interpretation of a published map of the United States, made by a person of education and intelligence, but untrained in geography and unfamiliar with America. The artist put down what he (or she) saw in an atlas, and left out what he didn't see, to include those details of greatest interest to the British people.

American geography is by no means a familiar subject in Great Britain, outside of professional circles. The odd ideas of the United States entertained even by well-educated Englishmen are a subject of humor on both sides of the Atlantic. The numerous errors, omissions, and oddities of detail in the MOI map are the result of this unfamiliarity, plus certain subjective influences and perhaps haste. But many of them are so natural, withal surprising, that they must be ascribed to obscurities in the atlas consulted. They show how the source map was read, or misread; how it failed to set the artist straight.

The users of atlases, American or foreign, must be presumed to lack special training. Some of the ideas that formed in this British mind upon examining a map of the United States may be expected to form in any mind not already familiar, through education, with this country's geography.

Professional cartographers, of whom the writer is not one, may therefore find valuable hints for the improvement of their art in this innocent British picture of the United States. In the following analysis of its detail, the attempt is made to adopt the point of view of the British copyist. The resultant psychoanalysis of a typical map reader may be more provocative than conclusive, but it at least demonstrates that in cartography for the general public, nothing may be taken for granted.

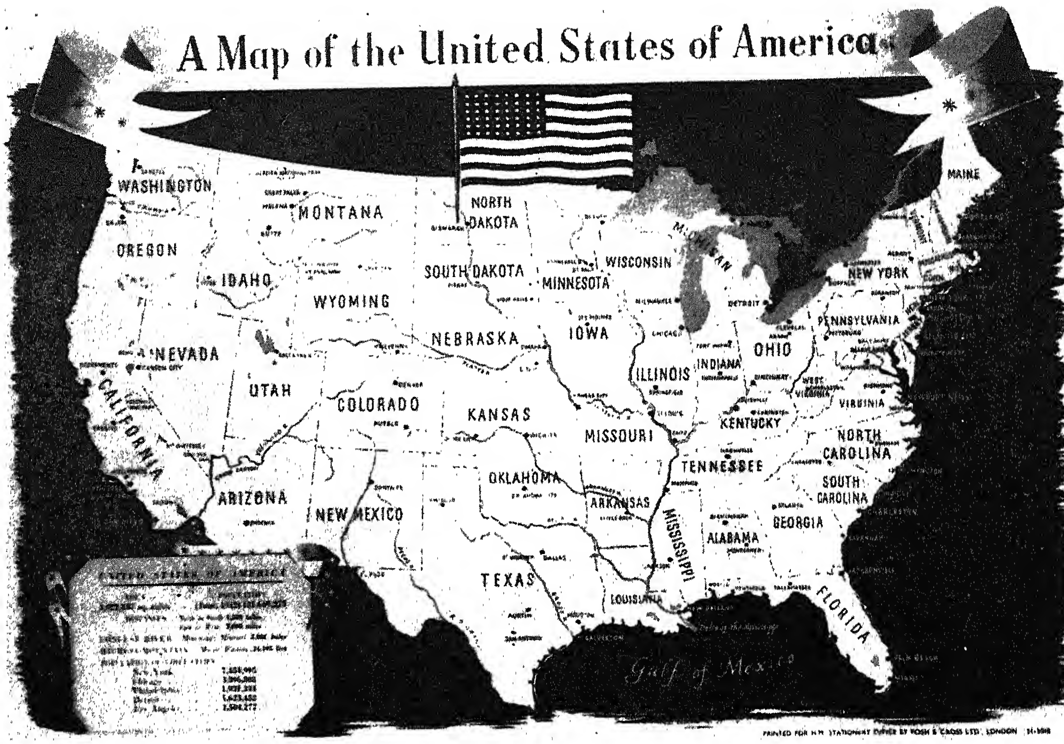
ANALYSIS OF THE MOI MAP

The British artist was greatly impressed by this nation's size, to the exclusion of other pertinent considerations. An inset box, under the heading "United States of America," contains a selection of statistics: area, population,[2] "distances,"[3] "longest river," "highest mountain,"[4] and "population of chief cities" (New York, Chicago, Philadelphia, Detroit, Los Angeles).

The principle of superlatives is pursued into the map itself. Mt. Whitney, the "highest," is the only mountain indicated anywhere; it appears as an isolated peak. All the rivers shown are (with two exceptions) the "longest" in the United States. The largest states, Texas and California, are the most thoroughly documented in place names.

It is likely that this approach was deliberate. As an item of home propaganda, the map's obvious function was to create an impression of size and power across the Atlantic, of an ally upon whose strength England could rely. The artist carried this assumed "directive" to an extreme.

He did not observe any relief, or consider it important. Except for some twelve rivers and the one mountain peak, the MOI poster



map is entirely barren of relief. Thus the United States looks like one vast plain watered by a few great, sluggish streams, a topography akin to that of the U.S.S.R. Moreover, since all the rivers (save the Ohio) are in the West, where they are widely spaced, an illusion of uniformity is created.

One of the freaks of this omission of relief is to give Great Salt Lake a phenomenal outlet. A tributary mistaken for the South Platte River appears to rise in the lake, climb the Continental Divide, and proceed across Wyoming to its junction with the Missouri.

A deduction is that the atlas consulted by the MOI artist did not make evident at a quick glance the general nature of the country's configuration, or give any idea of differences in terrain and natural routes of transportation, settlement, and trade.

The size of the West threw him off perspective in considering the East. On this map place names and natural features are apparently distributed among the States according to the space available. Thus Texas and California appear to possess the largest number of cities worthy of inclusion, while Ohio has only three, and the two smallest states, Rhode Island and Delaware,[5] have none at all.

Eastern rivers are omitted by a similar mental process. Compared on a map to the Mississippi or to the Columbia, the Hudson appears to be only a little river, so not worth including. The boundaries of Western States are drawn accurately, those of the East, inaccurately. The one mountain and the only named lake on the entire map are both in California, and all other such "features" are in the West. They include three national parks (Glacier, Yellowstone, and Yosemite), one natural wonder (Grand Canyon), and one great work of man (Boulder Dam).

Compromise in perspective is a basic difficulty with small maps of the United States, and here appears to be a leading source of false interpretation. The details of the crowded East are too hard to see; those of the spacious West, too easy. Their relative importance is in consequence obscured. The Ministry of Information artist could not have omitted the Hudson River had he been able to recognize it for a mightier waterway than the Thames and a rival of the Rhine.

He did not see boundary lines clearly or grasp their nature. The straight-line boundaries characteristic of the U.S. have always intrigued Europeans. The MOI artist tends to make them even straighter. His atlas evidently was not vivid enough in delineating boundaries to prevent his overlooking their quirks.

Parts of the Canadian and Mexican borders are covered by the decorations, so one does not know how such vagaries as Puget Sound, the enclave at Lake of the Woods, or the mouth of the Colorado might have been handled. The visible international borders are formalized and smoothed; Maine, for example, is almost of geometrical shape. In many cases, the existence of natural boundaries is not recognized. Thus the river-lake system west of Lake Superior, and the river connections between Lakes Huron, Erie, and Ontario are not indicated. Lake Memphremagog becomes a little bump on the Vermont line, and Lake St. Clair a bulge on Michigan's.

Most spectacular omission is the names of the Great Lakes, either collectively or individually. Nameless also are the St. Lawrence River and Lake Champlain, though on the southwest border the Rio Grande is named. Failure to name lakes is general throughout the map, with one unexpected exception. One may speculate how it happened to be Salton Lake, California.

The British copyist's tendency to portray the East with the same broad, bold strokes as the West is also evident in his internal boundaries. State borders are simplified by omission of meanderings and enclaves. Rivers and other natural boundaries are often omitted, and the smaller States get rather severe treatment.

Delaware is simply lost, and appears on the map as an empty panhandle of New Jersey. Rhode Island, labeled "R.I." (which must have mystified many a "pub" patron), is too small to permit including the city of Providence. Connecticut is "Conn." and Massachusetts is "Massachus's." Philadelphia wanders up and across the undelineated Delaware River to become a double city (two red dots) at the location of Trenton, N. J. Alabama's corridor to the Gulf of Mexico is lost by extension of Florida's northern boundary straight across to Mississippi, enveloping Mobile Bay. Even in the West, Nebraska's northern line crosses the Missouri River, nicking off a bit of South Dakota.

Most baffling of all must have been the complex borders of Maryland, Virginia, West Virginia, and the District of Columbia. Virginia's eastern shore on Cape Charles is ceded to Maryland. West Virginia's panhandle is lost to Virginia. The existence of the District is ignored. Washington is located well within the Virginia border, just about at the location of an actual little town in Rappahannock County called Washington, Va. The bend of the missing Potomac becomes a half-square projecting into Maryland, suggesting the squareness of the District of Columbia outline.

Withal, these errors are natural and excusable. If one examines a typical atlas, it becomes evident that many such details may easily be confused by a person unaware of the facts by other means. In England the concept of a Federal District and of the division of a country into sovereign States is only dimly understood, despite Dominion parallels, such as Canada and Australia, and the canton system of Switzerland. Maps do not clarify the point that Washington is in federal territory, as distinct from state territory. Obviously the MOI did not appreciate the absurdity of "Washington, Va."

He was unable to appraise the importance of coastal indentations and waterways. The artist's handling of seacoasts, rivers, and lakes indicates an effort to simplify, but his atlas did not make clear which details should be included no matter how drastic the simplification, and which might safely be omitted. His selections can be accounted for in no other way.

On this map, the outstanding feature of the coasts is the cities, with some surprising results that will be mentioned below. Of the coastal islands, only Long Island is named, with two off Massachusetts, and three off southern California also drawn. Sandspits are indicated along the Gulf coast of Texas and the Atlantic coast of Florida, but the important chain forming Cape Hatteras, and the keys culminating in Key West, are lopped off, lost in the slate colored ocean.

A number of bays are indicated, but just two are correctly distinguished by name. They are (for some reason) Long Bay, S. C.,

and Monterey Bay, Calif. Delaware Bay is labeled "Delaware River" (the river itself is omitted), and San Francisco Bay is called "Golden Gate." (As drawn, the "gate" appears to be about 30 miles wide.) Long Island Sound, Chesapeake Bay, and other important indentations are ignored.

Capes also are nameless, including even Cape Cod. Just one coastal feature is recognized, the Delta of the Mississippi.

Inside the country, the selection of rivers shows that their importance was judged entirely by length. The twelve on the map consist of the ten "longest" rivers in the U.S.A.: the Missouri, Mississippi, Rio Grande, Arkansas, Colorado, Columbia, Snake, Red, Ohio, and Platte (with its North fork), plus the "Brazo" (Brazos) and Pecos, which rank Nos. 12 and 15. Possibly the last two would not have qualified had the copyist noticed the length of the Canadian River, the Colorado (of Texas), or the North Canadian. For on this map there is no Hudson, no Potomac, no Delaware (except for its incorrectly named mouth), no Connecticut, no James, no Illinois, no Tennessee, nor any number of others. The Ohio is the only river east of the Mississippi, and on the map it is nameless.

Lakes, all unnamed except Salton, are handled as isolated bodies of water, with no attempt to link them to one another, as in the case of the Great Lakes, or to the river systems. It is evident that the British artist did not grasp their significance or relationship to the life of the nation. No canals are shown.

He was confused by duplication of names. Whenever the artist encountered a river with the same name as a State, or a city named after a lake, or two cities with the same name in different States, or other such duplications, he usually omitted all but one. The point is admittedly confusing, and apparently his atlas made it no clearer.

There are numerous examples. The omission of the Ohio River's name is probably one. So also is the omission of Delaware, State and river, and the bestowal of the name "Delaware River" upon the Bay. Similarly Great Salt Lake is not named, but Salt Lake City is. "Long Island" serves to identify the Sound as well. Wilmington, N. C., is greatly magnified in importance, quite possibly by confusion with Wilmington, Del., which is omitted. Washington, D. C., must have been mixed up with Washington, Va. There is a city named "San Francisco" but no bay.

He derived no consistent principle from the classification of cities. The cities selected in the MOI map are the most fascinating of all its unpredictable details.

To begin with, the cities are distributed among the States apparently according to the space available. This helps give the map a certain artistic balance which the nation does not enjoy in fact. They are marked with red dots and names of varying size, with some attempt to distinguish them according to importance. But even for the five "chief cities," the distinctions are inconsistent. Important cities are entirely overlooked, while inconsequential ones are included and others are overrated. Here one can appreciate that the artist put down only what was most easily seen in his atlas.

The map creates the illusion that the most important United States cities are all on the coasts. This is partly the result of the color scheme; the names of seacoast cities are in white, prominently "dropped out" of the slate blue ocean background. Also it would be natural for a wartime British ministry to emphasize the convoy ports of the "bridge of ships" to Britain. However, the classification goes awry even within the coastal group.

Here is the list of cities for which the largest and boldest lettering is employed:

Portland (Me.)
Boston
"New York City"
Philadelphia (in New Jersey)
Atlantic City
Newport News
Wilmington (N.C.)
Charleston (S.C.)
Palm Beach
Miami
Los Angeles
San Diego

Any "pub" patron would gather that all of these are the principal metropoli of the United States. Chicago and Detroit, two of the "chief cities" listed in the inset, enjoy

equally large, but much lighter lettering. Because they are in the interior, they pale by comparison with San Diego and Palm Beach.

The category indicated by the next largest size of lettering is equally amusing:

Portsmouth (N.H.)
Savannah
Jacksonville
New Orleans
Galveston
San Francisco

Also featured, by heavy lettering of smaller size, are "Brooklyn" (no doubt for its fame) and Long Beach, Calif. (for some reason). Typical of the muddle is that Norfolk is made to look like an insignificant suburb of Newport News.

Inside the country the name sizes vary practically at random. Cleveland, Cincinnati, Milwaukee, and St. Louis are somewhat larger than most, but the very smallest lettering is assigned to Baltimore, Minneapolis, "Pittsburg," and other vital centers. Seattle and Portland, Ore., are well-nigh invisible.

There is no indication of state capitals, and a number are omitted, as in Kansas, where Topeka is missing but not Dodge City. Washington is not identified as the national capital. In general, cities are omitted or obscured whenever they are in congested or minutely subdivided areas. The possibility that a small State may contain large cities of disproportionate national standing appears to have escaped the unsuspecting artist.

This suggests that typography alone can not be depended upon to classify American cities. The device of representing metropolitan areas on a map as colored areas may solve one aspect of the problem. What is important about Boston is not its "official" population of 770,000, but its metropolitan concentration of 2,350,000. Pittsburgh, St. Louis, Baltimore, Cleveland, San Francisco-Oakland, Minneapolis-St. Paul, and others are similar cases. Providence, in congested Rhode Island, should be as easy to see and as prominent as Reno, in empty Nevada. The MOI artist missed the former but did not fail to include the divorce capital.

Political centers apparently require a more distinctive marker. Washington and the District of Columbia in particular need special handling. The British artist's confusion between size and political function is shared by Americans, many of whom, for example, think of New York as the capital of its State.

Conclusion

The foregoing analysis suggests the conclusion that existing general purpose maps of the United States are most susceptible of false interpretation. Accuracy and much of the significance of details are lost upon the untrained eye. That eye could as well be American as British. The failure of U. S. maps to register facts is less apparent among Americans because geographical education fills in the gaps and unscrambles the detail. In this non-professional British effort the false impressions are absorbed, uncorrected, and set down as gospel.

For example, to a foreign reader, the name "Philadelphia" in an atlas may look as if it were attached to a spot in New Jersey, but an American, attuned to the familiar ring of "Philadelphia, Pa.," does not fall into the error. The foreign reader's mistake thus reveals to us the existence of an unsuspected obscurity in our maps.

The world of the immediate future, it seems safe to forecast, will demand authentic geographical information about the United States. The increasing importance of this country in global affairs will subject it to greater scrutiny abroad; the foreign business man, statesman, and scholar will seek a greater factual knowledge. The long neglect of the subject in the schools and thought of the Old World may be corrected.

Maps will form an important part of this educational reorientation, and the cartographer will be called upon to provide them. American cartographers will be expected to produce the most authoritative maps of their own country, drawn in such fashion that they cannot be misunderstood. The MOI's anonymous and well-meaning poster designer has shown us that they can.

1 "Printed for H.M. Stationery Office by Fosh & Cross Ltd., London. 51–3018." The last is a code number.

2 It reads: "Population (June, 1942) 131,669,275." Actually all population figures are from the Census of 1940.

3 "North to South 1,500 miles. East to West 3,000 miles."

4 "Mississippi-Missouri 3,988 miles," and "Mount Whitney 14,495 feet."

5 Delaware is not even included as a State (see *infra*).

THE CINCHONA-BARK INDUSTRY OF SOUTH AMERICA

By WILLIAM CAMPBELL STEERE
ASSOCIATE PROFESSOR OF BOTANY, UNIVERSITY OF MICHIGAN

THE discovery of the antimalarial property of quinine has been called one of the great events of medical history, since it represented the first-known specific remedy for any disease. One seventeenth-century writer said that Cinchona bark was more precious to mankind than all the gold and silver that the Spanish Conquest brought from South America. Nevertheless, the circumstances of this important discovery are shrouded in a great mass of tradition and outright fiction (see SM, July 1945, pp. 17–20). The only really dependable information which we have is that Cinchona bark was introduced into Spain and Italy early in the seventeenth century and that it was distributed by the Jesuit fathers. In the militantly Protestant countries of northern Europe, the use of so avowedly a Catholic remedy as the "Jesuits' powder" was unthinkable, and a half-century elapsed before this prejudice and bigotry could be overcome, even in the interests of humanity.

Within historic times malaria has been driven from northern and central Europe, and from most of the United States, largely through the use of quinine. It has been widely accepted that malaria was an important contributing cause of the economic and cultural decline of ancient Greece and of the Roman Empire. One cannot help speculating what might have happened if the Greeks and Romans had possessed our specific remedy for malaria.

The eventual and general acceptance of quinine for the treatment of malarial fevers soon led to an enormously increased need for Cinchona bark. The original center of bark exploitation was the Loja region of southern Ecuador, but as the demand for quinine increased, the industry expanded northward into Colombia and southward into Peru and Bolivia, and reached fantastic proportions during the late eighteenth and early nineteenth centuries. Far from inducing measures of economy or conservation, indications of impending bark scarcity only resulted in still more intensive and destructive methods of bark harvesting as the competition became keener. The complete extirpation of trees in order to remove the root-bark caused the virtual disappearance of Cinchona species over wide areas. Finally, during the early nineteenth century, the wild-bark industry began to collapse as rapidly as it had expanded. Although prices increased, bark quality decreased, partly because of the exploitation of grades of progressively lower quality and partly because of adulteration with related but totally worthless barks. By the middle of the nineteenth century, the quinine shortage became so serious that the British and Dutch governments could no longer ignore it, since they were able to maintain their far-flung colonial empires throughout the malaria-infested tropics only with an abundant supply of quinine. In 1852 the Dutch sent Dr. J. C. Hasskarl to South America to collect seeds and plants of every available species of Cinchona, but the live plants which reached Java in 1854 later turned out to be almost worthless because of the very low alkaloid content of their bark. Nevertheless, seeds of better varieties were obtained from other sources, and after many vicissitudes Cinchona plantations were gradually established on a firm basis in Java. In 1859 the British government also acted to obtain Cinchona seeds and stocks of high quality with which to establish plantations in India. On the slopes of Mt. Chimborazo, in Ecuador, Richard Spruce obtained seeds and young plants of a superior race of *Cinchona succirubra,* whose bark was very rich in alkaloids but relatively poor in quinine. Spruce's materials, supplemented by further collections of seeds and seedlings made contemporaneously in other regions, reached India early in 1861 and formed the basis of an enormous plantation development

"TROPICAL" VEGETATION

THESE TREE FERNS AND AROIDS OCCUR AT 8,500 FEET ABOVE SEA LEVEL IN COLD, WET CINCHONA FORESTS.

which grew up there and in Ceylon. After a search lasting four years, Charles Ledger (see also SM, July 1943, pp. 17–32) received from a Bolivian Indian, in 1865, seeds of a form of *Cinchona calisaya* which turned out to be extraordinarily rich in quinine, and which had been a sort of trade secret among a particular group of Indians. Ledger sent these seeds to London, where a small part was sold to the Dutch government and the remainder forwarded to British planters in India. Unfortunately, the value of this variety of *Cinchona calisaya* was not properly recognized in India, and it gradually died out there. The Dutch were more fortunate in its cultivation, partly because they had longer experience and partly because the government subsidized their plantations. The trees arising from the Ledger seed were studied carefully by the Dutch, with results which astonished everyone concerned, since it could be seen very soon that this variety was unprecedentedly rich in quinine. As early as 1872 the bark of one tree was found to contain over 8 percent of quinine, and in 1876 a tree was discovered whose bark had produced over 13 percent of quinine. Already in 1877 the Dutch government plantations resolved to use for propagation material no trees with less than 10 percent of quinine in their bark! The high quality of the bark of Ledger's variety, which Moens named *Cinchona Ledgeriana,* was reflected at once by its high price in the market. Since even the best races of *C. succirubra* cultivated in India and Ceylon produced less than 5 percent of quinine sulfate and the average was perhaps 3 percent, they could not compete successfully with the Dutch barks. The Dutch government supplied private planters with seeds and cuttings of *Cinchona Ledgeriana* without charge, if they were prepared to devote their land to its cultivation. Under this policy Cinchona cultivation grew in a remarkably short time to such enormous proportions in Java that the price of a kilogram of quinine sulfate was reduced from \$100 to \$10 in the decade between 1880 and 1890. In the face of a price war among the Dutch planters themselves, who were dealing in a bark of superlative quality, the Cinchona planters of India and Ceylon either

VOLCANO OF TUNGURAHUA

THIS SNOW-CAPPED VOLCANO PROVIDES THE VILLAGE OF BAÑOS, ECUADOR, WITH HOT AND COLD WATER.

abandoned their low-grade plantations entirely or turned to the cultivation of tea. The Dutch thus inherited the whole Cinchona culture, but at a time when market prices were lower than the cost of production. About 1913 the Dutch bark producers united with the quinine manufacturers in order to form an organization which could fix prices at a level high enough to guarantee a profit to all concerned. The control of this organization was placed in the hands of a committee called the Kinabureau, which is often referred to as the "Dutch quinine monopoly," since at the beginning of the first world war it already controlled 95 percent of the world's quinine.

Just when the Dutch and the British were establishing plantations, the greatest demands were being placed upon the wild-bark industry. Our Civil War has been blamed for causing the last Cinchona "boom," since the desperate need for quinine by both armies raised the prices to a level at which the bark producers could again make a good profit. The end of the war precipitated the final collapse of the highly speculative and uncertain business of exploiting wild species of Cinchona. The production of mediocre but dependable grades of plantation bark by the British in Ceylon and India gave the real death-blow to the South American industry, just as the excellent Cinchona bark cultivated by the Dutch later put the plantations in India and Ceylon out of business. In a very short time, wild Cinchona barks from South America practically disappeared from the world market, although production for home consumption continued for some time on a small scale.

The invasion of the Dutch East Indies by the Japanese in the spring of 1942 not only cut off our supply of quinine but also committed us to a long war in which enormous quantities of this essential drug might be required. The task of finding sources of quinine was accepted almost at once by the newly-

created Board of Economic Warfare (now superseded by the Foreign Economic Administration). Naturally, the most practical solution of the problem was to revive the extinct Cinchona-bark industry in the several Andean republics which had provided the world with its quinine supply a century earlier. After lengthy negotiations, several of the republics granted to the Board of Economic Warfare exclusive buying rights in exchange for our guarantee to buy all bark above a certain minimum alkaloid content, to furnish technical aid to the bark harvesters and dealers, and to establish nurseries and plantations for future use. Because of the good will of all parties concerned, these agreements worked out reasonably well, and we were able to obtain much more bark in a much shorter time than had been anticipated —but I am getting ahead of my story.

Colombia was the first country to sign an agreement and since it is closest to the United States of the Cinchona-producing republics, it was the destination of the first procurement group, which left Washington in October, 1942. This first party consisted of six men: two foresters, two botanists, and a chemist, all under the supervision of a lawyer as chief of the mission. We were joined in Colombia by L. R. Holdridge, a forester and a competent field botanist, who came directly from his work in Haiti to supervise our survey work. The preponderance of field men indicates the importance justly placed on survey work. As originally planned in Washington, two field parties were to be organized, each one consisting of a botanist to find and identify the quinine-producing plants, and a forester to estimate the quantity of bark and to arrange for its exploitation. This plan was followed during our preliminary surveys for the first three months, but the departure of Mr. Holdridge and the need for expanding the field work made it necessary for the four field men to head separate survey parties. The extreme complexity of the Andean flora in general and the large numbers of species and varieties of Cinchona in particular made the usual methods of cruising and estimating employed in our temperate forest quite unsuitable. Consequently, as the program developed during the next couple of years, more and more

PRIMITIVE BARK DRIER
A SMALL SHELTER OF THIS SORT WILL DRY ABOUT THREE HUNDRED POUNDS OF FRESH BARK AT A TIME.

botanists were brought in for survey work, while the foresters in large part turned to other important work and made real contributions in the construction of trails and bark-driers, in the expediting of bark handling, in the solving of transportation problems, and in the establishment of Cinchona nurseries and plantations.

I have mentioned elsewhere the important contribution made to the Cinchona program by the chemists. The Cinchona mission laboratories established in Bogotá, Quito, Lima, and La Paz made possible the prompt analyses of field samples and lots to be purchased. In Cinchona "booms" of former centuries, analyses were made only after the shipment reached Europe, months after the bark had been bought and the proceeds spent. This situation led not only to fantastic speculation on good barks but to excessive traffic in worthless ones. Many barks which were sold at high prices turned out to be totally lacking in quinine, although perhaps rich in

cinchonine or cinchonidine. Through prompt analyses we were able to stop the harvest of poor barks and to encourage the production of good ones. Many species of Remijia, Ladenbergia, and other members of the Rubiaceae closely resemble Cinchona to the untrained eye, and our technical aid, both botanical and chemical, has saved much energy and many thousands of dollars which dealers would otherwise have "invested."

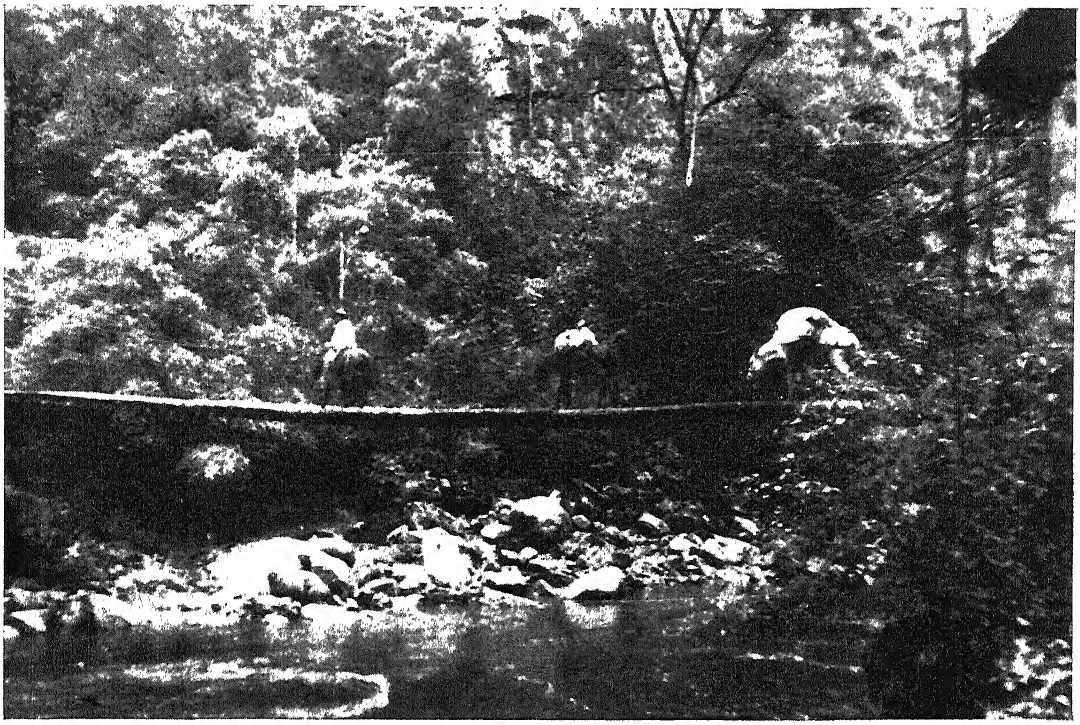
The published literature on all the aspects of Cinchona-bark production in South America is so extensive that a compilation of just the titles would probably fill a whole volume. The amount of unprinted folklore, legend, tradition, and popular belief among the people themselves is even more extensive. Since the ancient exploitations of Cinchona bark followed a very empirical system, under which any useful discovery was considered to be a trade secret, we could find few answers to the practical problems which confronted the survey parties. The only thing we were sure of was that a new and reasonably complete survey had to be made, and that all species and varieties of Cinchona and related genera must be searched for and tested as potential sources of quinine.

More than a dozen species of Cinchona occur in the Andes, and nearly all of them produce quinine or some alkaloid related to it. Some species produce alkaloids in such small amount that they are of no economic value at all, but only of botanical interest. In the field one may recognize Cinchona trees not only by their technical botanical features but also by the bitter taste of their bark. The different species and varieties differ greatly in their total content of alkaloids as well as in the percentage of each one. Each species of economic importance has so typical an alkaloid content that one may make his identification from the analyses of a bark sample without seeing a botanical specimen.

Linnaeus described the first species of Cinchona in 1753, calling it *Cinchona officinalis*. There are many varieties or races of this species, some of them without any

CHEERFUL ECUADORIAN PEONES

FIFTEEN OF US (NOT COUNTING VARIOUS CHICKENS, DOGS AND PIGS) LIVED IN THIS SHELTER A WEEK.

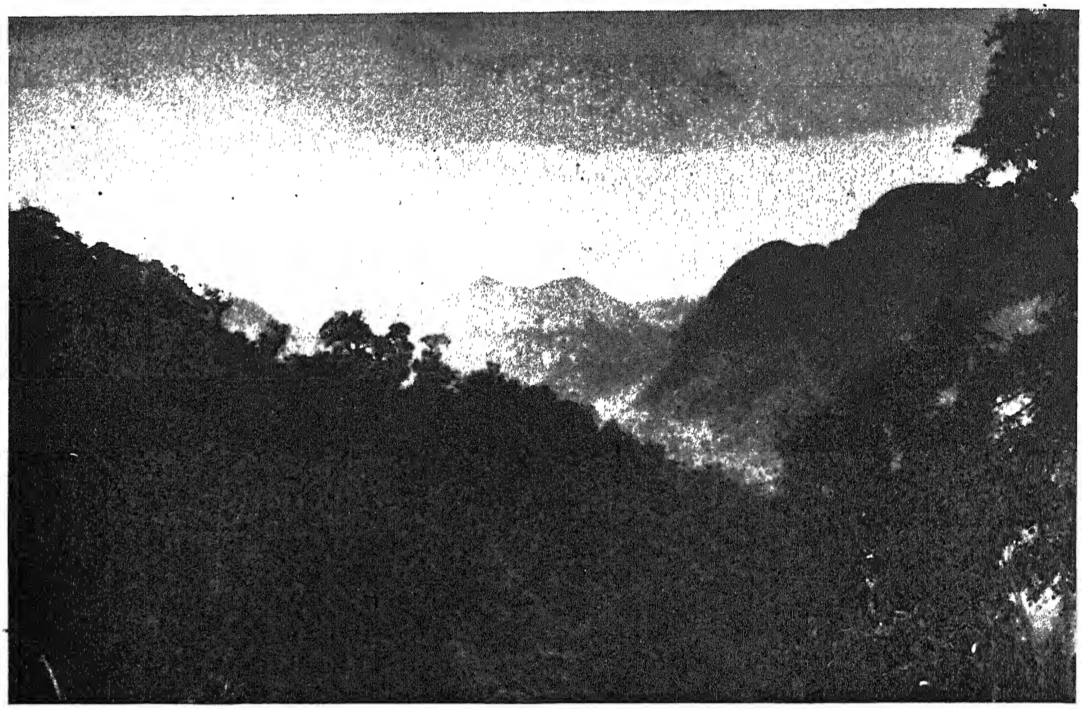


RELUCTANT MULES
THIS SUSPENSION BRIDGE OVER THE RÍO GUATIQUÍA, IN THE EASTERN ANDES OF COLOMBIA, IS TYPICAL.

quinine at all, and others, which have been called *C. calisaya*, the richest in quinine of all wild barks. The alkaloid most commonly associated with quinine in the bark of *Cinchona officinalis* is cinchonidine, although the *C. calisaya* types in Bolivia also produce considerable quinidine, and a much larger proportion of quinine than other alkaloids. *Cinchona officinalis,* in one form or another, is widely distributed from western Venezuela through the eastern Andes of Colombia and Ecuador into Peru and Bolivia.

The commonest species of Cinchona everywhere is *C. pubescens,* which is characterized by its large leaves, thick bark, and rapid growth. Unfortunately, its bark is commonly low in alkaloids and may lack quinine altogether. Nevertheless, there are some local races, called *C. succirubra,* which produce high alkaloid concentrations, and it was a race of this sort which was introduced into India from Ecuador in 1861. The bark of *Cinchona pubescens* usually contains much cinchonine and may produce no other alkaloid at all. This is the most widely distributed species of Cinchona, which occurs in Venezuela, in all three ranges of the Colombian Andes, in both ranges of Ecuador, and very widely in Peru and Bolivia. Not many years ago, a few trees were found in northernmost Panama and along the Costa Rican frontier. This is the only species of Cinchona native to North America.

One of the most interesting species has been *Cinchona pitayensis,* which was first discovered on the slopes of the Nevado del Huila in south-central Colombia, not far from Popayán. The bark of this species is not only unusually rich in quinine for a wild species, but the quinine is extracted unusually easily. However, this species was considered to be very rare by the early bark dealers, and it remained almost unknown to botanists. One of the real contributions of our survey work in Colombia was the rediscovery of *Cinchona pitayensis* in rather substantial quantities, and I shall always feel that my primary service to the program was the first discovery of this species in Ecuador. It seems incredible that the species of Cin-



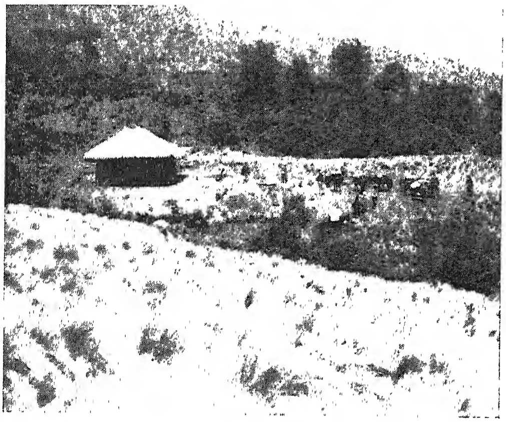
IN THE CINCHONA FOREST

THE WESTERN SLOPES OF THE ECUADORIAN ANDES ARE COVERED WITH DENSE AND EXTENSIVE FORESTS.

chona with consistently the highest-yielding bark should have remained unknown in Ecuador until the summer of 1943, but the tradition among Ecuadorian bark dealers that the better barks occur only at lower levels seems to have kept them from exploitations at high altitudes. Since the best "red" barks, from *Cinchona pubescens,* occur at about 4,000 feet above sea-level, it is no wonder that the dealers were skeptical when we pointed out to them a better Cinchona between 9,000 and 10,000 feet. This species has now been followed into Ecuador more than a hundred miles south from the Colombian frontier, and at last report may occur still farther south. The details of its discovery and some of its more important botanical features have been outlined elsewhere.

One of our biggest surprises was the discovery of a race of *Remijia pedunculata* in northern Colombia whose bark produced as much as 3 percent of quinine, with hardly any other alkaloid. This plant is not a Cinchona at all, although fairly closely related, but illustrates the importance of our method of studying all Rubiaceae.

Many profound treatises have been devoted to the classification of the various types of Cinchona barks, and dozens of different sorts were distinguished on the basis of color, surface, grain, inrolling or outrolling, etc. Unfortunately, the botanical source of the bark under consideration was generally ignored, and several distinct types in the old classification of barks might easily come from the same tree, depending on the part of the tree and the method of preparation. The thick bark from the base of the tree used to be dried in large, flat slabs, whereas the thin bark from small twigs and roots was dried in tightly-rolled tubes or "quills." The bark from the base of the tree is apt to be dark and rough, but the upper bark is often smooth, whitened with lichens, and shows transverse fissures and cracks. Our interest was not to distinguish all the ancient types of barks but simply to recognize the species from which the bark had come. One can develop the ability to distinguish the barks rather easily, especially if he has been trained in the discipline of systematic botany and its fine discriminations. The easiest

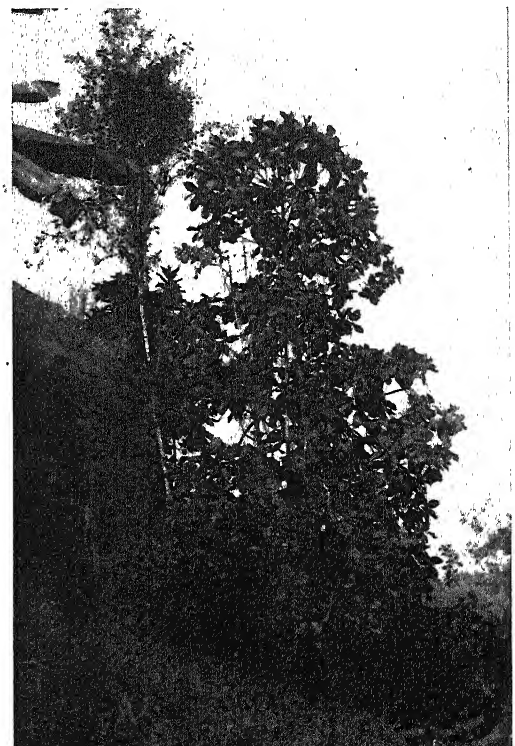


TYPICAL ANDEAN SHELTER
THIS SHELTER, TAMBO JUCAL, IS DITCHED TO KEEP LIVESTOCK AND OTHER ANIMALS FROM WANDERING IN.

bark to distinguish is from *Cinchona pubescens,* because it tends to form rather thick, flattened chips with an orange color. The chips are not inrolled, but rather tend to turn back or out at the ends. Perhaps the most constant feature is the torn and interwoven appearance of the fibers which were apparently distorted when the bark was stripped from the tree. The bark of *Cinchona officinalis* and its varieties is to be recognized by its darker, clearer red color, the tendency of the bark pieces to inroll strongly, and the parallel, undistorted appearance of the fibers which were next to the wood. *Cinchona pitayensis* has a rather yellow bark which resembles that of *C. officinalis* except that it rolls inward much more tightly, and the cuts at either end swell so that they are almost as thick as the bark where not cut. These two species also produce white upper bark with the characteristic transverse fissures, a character lacking in the bark of *C. pubescens.* Bark of all species of Cinchona shows needlelike fibers at the broken ends, to a greater or less degree. This character is especially useful to distinguish Cinchona barks from the bark of *Remijia pedunculata,* which may produce substantial quantities of quinine in certain areas. Remijia bark breaks with a brittle, glasslike quality, which serves to identify it.

The business of adulterating quinine-producing barks with the bark from other trees is a standard part of an old and not particularly honorable profession. It was always amusing to see the surprised expression on the face of a dealer when the supposedly innocent—or at least ignorant—field man was able to detect the admixture at once and reject the lot. A whole book could be written on the subject of bark identification, its folklore and anecdotes.

The forests which contain Cinchona are usually not at all easily accessible, because as soon as a road opens a new valley to the public, the forest soon disappears before the settlers' axes and Cinchona trees generally fall unrecognized. Even though the cut-over land be abandoned later, the better species of Cinchona seem to be extremely slow in returning. The outstanding exception is *Cinchona pubescens,* which will often repopulate an old field within a few years. It is unfortunate that this species has so little value, since it grows extremely rapidly in most habitats and under some conditions may become almost a weed. Its main usefulness seems to be as grafting stock in nursery work,



CINCHONA PUBESCENS
THIS COMMON ANDEAN TREE MAY REACH A DIAMETER OF THREE FEET AND PRODUCE NEARLY A TON OF BARK.

ECUADORIAN BARK-CARRIERS

STRONG BACKS ARE NEEDED ON STEEP, MUDDY TRAILS WHICH OTHER BEASTS OF BURDEN CANNOT NEGOTIATE.

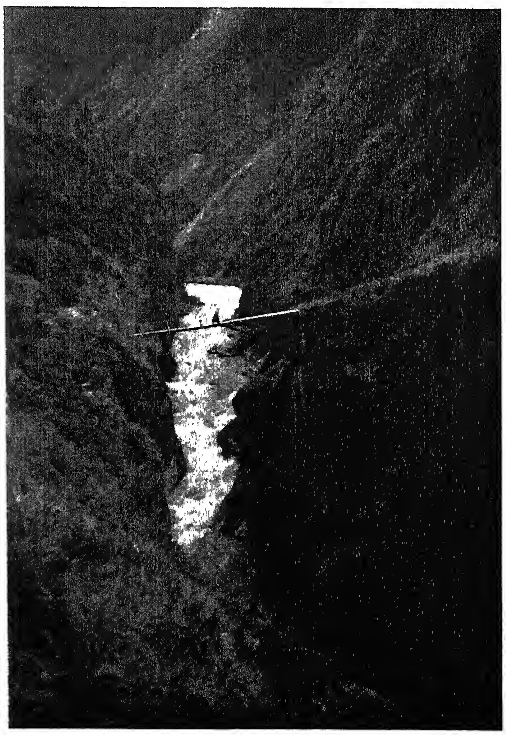
for giving good root-systems to weaker species. Along the railroad not far north of Popayán, one may see many trees from the train window, and dozens of trees occur along the automobile road between Quito and Santo Domingo de los Colorados. In both places, however, the bark is almost worthless. *Cinchona officinalis* and *C. pitayensis* are much more sensitive to change and have retreated farther and farther before the inroads of settlers and bark harvesters. It is very rarely that these species are to be found near a road, and then only because the country is too rugged for cultivation and the trees have not been recognized yet. Ordinarily, one may depend on a trip of from one day to two weeks by horse or on foot in order to reach Cinchona-producing forests, which are always on steep mountainsides in rainy regions. As moist air from the hot tropical lowlands rises along the slopes of the Andes, it cools and precipitates its moisture in rather definite bands. As one progresses from sea-level to the upper limit of tree-growth, at about 11,000 feet above sea-level, he will pass through several rain-belts, which are easily recognized by the much more luxuriant growth of plants, the mud, and the low-hanging clouds. Cinchona trees are restricted to wet forests, which are usually covered by clouds at night, and apparently cannot withstand a prolonged dry season. The standard joke of the field men in Ecuador was that during the dry season in the Cinchona forests, it rained only in the afternoons. An annual rainfall of 150 to more than 200 inches is not unusual and must be taken into serious consideration when planning any work of exploration or exploitation.

The Cinchona forests of Ecuador, especially in the eastern Andes, are so remote and inaccessible that a survey party must plan on making its own trails and packing all its equipment on the backs of men. On a survey trip lasting between two weeks and a month, each man will eat a large proportion of all the food he can carry, and in a general way one has to estimate that every two men need another man to carry food for them. Since several men are needed for cutting

trails and several others for carrying baggage, especially tents for protection against the continual rains, parties of 10 to 15 men are not uncommon. Fortunately, the diet preferred by the men is a very simple one and compact to carry, since it consists of barley meal, coarse brown sugar in large cakes, parched corn, rice, beans, lard, and coffee. I ate this same diet, not from preference, but simply because it was easier than carrying extra food. The only exception was that I carried enough canned meat to have some at least every other day. An occasional settlement or homestead in the wilderness would provide us with some eggs or rarely a muscular chicken. The staple meat in the backwoods of highland Ecuador is the guinea pig, which is delicious indeed after one has gone for several weeks on a diet light in protein. I will never forget the banquet in the tiny settlement of La Bonita when we were able to buy a whole hog, which furnished much-needed protein to the survey party and a real celebration for the isolated village. When the men become really discouraged, the gift to them of cheap cigarettes works wonders. Yet we always had to buy the cigarettes for the men out of our own pockets because the Cinchona mission auditor could not be convinced that this was a legitimate expense—even though he would never have been able to smoke them!

One of the surprising features of the Cinchona field-work in South America is that we suffered much more from cold than from heat although we were always within 15 degrees of the equator. Those of us who were engaged in exploration for *Cinchona pitayensis* between 8000 and 10,000 feet above sea-level often had to camp and travel in the Andean *páramos* above tree-line, where cold nights are the rule and snow-storms not infrequent. The rate of radiation of body heat is doubled at high altitudes, and one used to sea-level always has the sensation of being cold, especially if he is wet. Nevertheless, it seems ridiculous to suffer from cold when the temperature is no lower than 50° F. and he is sitting practically on the equator!

Our surveys were made under widely varying circumstances. At times they were sponsored by some land-owner who hoped that his forests might consist entirely of Cinchona trees. In such cases, we could depend on certain facilities in return for our technical advice; that is, the owner was glad to furnish us some transportation, to lend us guides and machete-men, and to allow us to use what houses might be on his property. Very few owners found it possible to accompany the survey personally, however. At the other extreme, we surveyed many large areas which had never been purchased or homesteaded. Much of the land of the Oriente of Ecuador is wild and unsettled—*terreno baldío*—which may be had for the asking from the government, under much the same rules as our own western homesteads. These lands are unsettled because of their lack of roads, their inhospitable climate, and their distance from centers of population. Here we sweat and struggled for weeks at a time, cutting trails, crossing flooded rivers, trying to keep the discouraged cargo-bearers from skipping out and leaving us and our baggage on the headwaters of some unknown river, far from civilization.



BRIDGE OF SIGHS
IN DAILY USE, THIS NARROW BRIDGE IS OVER THE DEEP GORGE OF THE PASTAZA AT BAÑOS, ECUADOR.

TIMBER LINE IN ECUADOR
THE FOREST AND THE PÁRAMO MEET EACH OTHER AT 11,500 FEET ABOVE SEA LEVEL IN CARCHI PROVINCE.

Although one square mile of virgin forest on the steep slopes of the Andes in Colombia, Ecuador, Peru, or Bolivia may have several hundred species of trees, Cinchona trees are not as difficult to find as one might expect. Fortunately, each species has a certain fixed and definite altitude preference or limitation, so that with an altimeter one can stay quite easily within the altitudinal range of the species for which he is searching. Furthermore, Cinchona trees tend to be grouped together, not in pure stands but in associations of 5–50 trees, called *manchas,* in reasonably close proximity, so that one tree may be seen from another. Also, one who is interested in plants may fix his attention on them so intensely that he will see only those which he wishes to see. My survey technique was to make myself aware only of members of the Rubiaceae, the plant family to which Cinchona belongs. In this way no species of Cinchona could be missed, and many interesting Cinchona relatives were automatically discovered for testing.

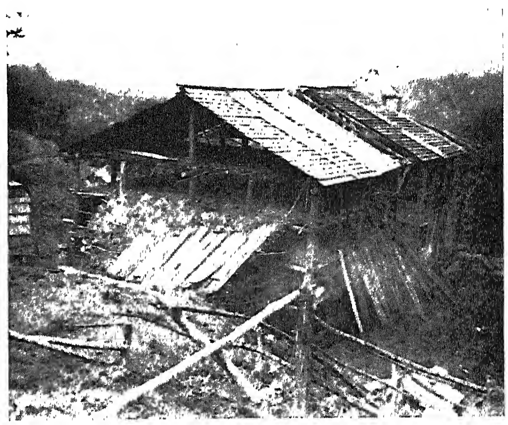
The estimates of the quantity of bark which the survey parties made were not only interesting but often also highly entertaining. Instead of the extremely accurate and scientific techniques available to a forester estimating board feet of lumber in a pine forest in the United States, we had no technique except shrewd guesses based on our past experience with the same species or variety under more or less similar conditions. We came to know fairly exactly those data which could be measured; for example, we knew the percentage of water in the bark of each sort of Cinchona. More than 80 percent of the bark of some species is water, in other species the water content may be as low as 65 percent. We knew the thickness of bark and rather accurately how it differed in trees of different ages and diameters. We even made tables based on a formula derived from the fact that the surface of a cylinder may be calculated from the diameter—from which could be tabulated the bark yield of a tree of any size. Unhappily for the scientific method, there are so many intangibles in the brand-new science of calculating bark-yield that the actual yield could be estimated just about as closely by an intelligent man with a good deal of experience. It is much easier to make an estimate on the basis that each three trees of a certain population will yield a hundred pounds of bark than to reach more or less the same conclusion by spending a half hour in mathematical operations. It was more important to see that the branches down to three inches in diameter were skinned of their bark than to worry too much about formulas, since if bark is left on the tree even the best-planned formula goes astray. A practical job of this sort is useful experience to any of us who have leaned rather heavily on theoretical evidence!

As soon as a tree is felled, either by a heavy machete or with one of the straight-handled, broad-bitted axes which our Latin brothers prefer, one or two men set themselves at once to the task of stripping off the bark. In some varieties the bark is very firmly attached to the wood, so that it comes off in the form of chips or untidy fragments. In other varieties often called *concha* (shell) the bark separates very easily from the wood, so that the men have competitions to see who can bring in the largest piece. I have seen a flat piece of bark proudly exhibited which was five feet long and three feet wide. Usually, however, the work is deadly serious and with strong economic limitations. If the price is low, only the heavier, more easily removed bark is harvested; if the price is high, then the smaller branches may be stripped. The usual tool is the ubiquitous machete, which may be specially ground for the job. In the region of Loja and Cuenca, where bark-harvesting

has been going on for two centuries, a special knife has been developed for separating the bark from the wood, and men from this region are very expert in their knowledge of barks and how to handle them.

After our first surveys, we came to the conclusion that for most efficient exploitation the trees should be cut down. At first flush, this might sound like an extravagant waste of natural resources and a violation of all laws of conservation which we should encourage in ourselves and other nations—and we were accused of all these things. However, in the long run, we always managed to convince even our most determined opponents of the advisability of cutting down the Cinchona trees. One of the most telling arguments is the fact that the stumps of cut trees have the ability to produce sprouts almost at once, and that in the course of a few years each cut tree becomes a whole cluster of new trunks. In this way, if the rest of the forest is left undisturbed, the natural resource will regenerate itself more or less automatically. On the other hand, trees which are stripped of part of their bark while standing are apt to die, and trees which die while standing do not sprout. It is a hopeless task to argue with the bark stripper that he must take bark from only one-half of a standing tree. To him, bark represents money or food or something he wants, and to leave bark on the trees is to him as distasteful as it is for us to have him take it all. As a special concession, occasionally, a man would agree to take only half the bark from a tree, but then we would find out that his partner—the men usually work in pairs—was harvesting the other side, so that neither man was taking more than half the bark from the trunk! The important point of all this is that girdled trees which die do not regenerate themselves and have no further value. Furthermore, a standing tree can be stripped of only a quarter to a half of the bark which can be gotten from a felled tree, and a much greater efficiency is reached in bark production by cutting the trees. The most important point to be made, and the one which appeals most to the logical Latin economists, is that almost 100 percent of the wild Cinchona bark now being harvested in the Andean forests was worthless in the world markets before the war, and presumably will be worthless again after the war, just as soon as plantation bark containing from 7 to 10 percent of quinine sulfate appears on the market. In other words, here was the chance to reap a harvest, to sell a commodity at a good price which under normal conditions could not be sold at all in the world market. So it was not difficult to prove that a law prohibiting cutting of the trees—even if it were observed—would result in an economic loss to the country which passed the law.

In the beginning, one of the great gaps in our knowledge was how to dry bark most efficiently, with least loss of total alkaloid or the transformation of quinine to other and less valuable alkaloids. There is a widespread superstition among the bark harvesters and dealers of every country which produces Cinchona that sun-drying is injurious to the alkaloid content. We soon discovered that the greatest source of loss of alkaloids was fermentation, or "heating," of bark which had been piled up wet and allowed to stand. Sun-drying was found to have no injurious effect; in fact it is distinctly beneficial since it discourages fungus growth. Someone, in the early days of Cinchona plantations, remarked in print that 80° C. was the critical temperature for artificial drying, above which alkaloids would be destroyed. The basis for this recommendation may have been a careful, scientific study, but I doubt it. It is more likely to be somebody's opinion



CENTRAL BARK DRIER
WITH A CAPACITY OF SEVERAL TONS OF FRESH BARK THIS STRUCTURE WAS PLANNED AS A BUYING CENTER.

which just happened to get into print and has been copied painstakingly by everyone writing on the subject since. Anyhow, it is manifestly absurd to tell a group of Indians not to let their drying bark reach a temperature above 80° C., or even to furnish them with thermometers. Where bark is to be dried in small lots near its origin it is simply spread out on bamboo racks or platforms under thatched roofs, and open fires lighted underneath them. Imagine trying to maintain scientific precision under such conditions! By trial and error methods and the use of a chemical thermometer, we finally arrived at a method of controlling the heat. We found that if the racks were 120 centimeters (about four feet) above the ground, even a bright bonfire beneath would do no apparent damage. When heat from the fire was safe for the bark, we found that we could place our hands palm up on the bottom of the racks over the bonfires without pain. Even the most primitive dweller of the primeval Andean forest can understand directions of this sort. We found that one square meter of rack can accommodate a hundredweight of wet bark and that fires at intervals of two meters under a rack two meters wide give satisfactory results. With practical information of this sort, we could advise a dealer on the size of the drier he must construct to handle the bark available to him, in order that no bark would ferment on standing and yet so that he would not overexpand. If an area was especially rich in Cinchona or if local conditions were such, through lack of materials or through excessive rainfall, that small driers could not be constructed in the forest, then a more efficient drier of greater capacity had to be built. It is obvious that when open fires are maintained under a rack covered with wet bark, much of the heat will escape at the sides and the process will take a longer time. Yet the investment of building a larger drier, in which the heat is directed through the bark by having the sides boarded up or covered with sticks and mud, can hardly be justified in a small-scale business. In the Javanese plantations, where bark can be brought in from the same trees year after year in predictable quantities, the driers are very complicated and expensive machines which dry the bark under controlled conditions. In the exploitation of wild barks, however, the cost of an expensive drier might make the difference between profit and loss to a small dealer. In the Andes one not rarely finds deep, dry valleys which are rain-shadow deserts in the midst of areas with excessive rainfall. In several localities in Ecuador and Colombia it was possible to carry the wet bark several miles and dry it in the open air with a shelter or sometimes without any cover. In the rainy zone it is hopeless to dry bark without artificial heat. I have seen bark kept under a shelter, not exposed to rain but only to the air, which had not become completely dry at the end of three months. In the same region bark which had been painstakingly exposed to every bit of available sunshine for a month was not really dry.

After the bark is dried it is packed for shipment in jute bags which will hold 100 pounds. The dry bark, which has lost approximately 70 percent of its weight, is tamped tightly into the bags, yet because of its bulk a hudredweight of Cinchona bark is twice as large as a hundred-pound sack of sugar. The bulk of bark always creates problems in hauling and storing, yet several thousand tons of Cinchona bark were harvested and shipped during the last few years.

In Ecuador the bark was harvested on the outer flanks of the great Andean chain and always had to be brought over the top of either the eastern or western range in order to reach highways or railroads. Many of the original trails through the mountains were in such bad condition that they had to be extensively repaired or new roads built into bark-producing regions. Trails built for occasional travel broke down soon when subjected to everyday use by hundreds of men, horses, mules, oxen, or llamas carrying food and supplies in and Cinchona bark out. As soon as the bark reached a highway most difficulties were over, in spite of shortage of gasoline and tires, and delays of every sort. Sooner or later, by truck, train, or river boat, the bark reached a seaport—Callao, Guayaquil, Buenaventura, or Barranquilla—where it could be loaded onto ships bound for American processing plants, thus making all our work worthwhile.

"INTELLECTUAL" VERSUS "PHYSICAL" PEAK PERFORMANCE: THE AGE FACTOR

By HARVEY C. LEHMAN

PROFESSOR OF PSYCHOLOGY, OHIO UNIVERSITY, ATHENS

IF THE chronological ages at which athletes achieve their peak performances are to be ascertained successfully, the following facts need to be taken into account: (1) The more vigorous skills, such as boxing, football, ice hockey, and tennis, tend to deteriorate relatively early, the median ages and the peaks of performance age-curves for each of these behaviors occurring prior to age 30. Other skills, such as rifle and pistol shooting, bowling, duck-pin bowling, and billiards, which require less explosive outbursts of speed and energy for their successful performance, deteriorate more slowly. (2) At the time of exhibiting their best performances, certain kinds of more recently born athletes have been somewhat older than were their predecessors within the same field of sport. Thus, the earliest pitching and batting annual champions of the two major American baseball leagues were somewhat younger than were the major league annual champions during the decades just prior to the outbreak of World War II.

Possibly, with the passing of time and the increase of public interest, professional baseball has become so profitable financially that the stellar performer tends to participate therein as long as he is able. This age increase may also be due in part to the fact that the more recently born athletes have learned better how to preserve and maintain their physical fitness. Be that as it may, when the ages of seven groups of earlier-born athletic champions were compared with the ages of seven groups of those more recently born, the field of sport being kept constant in each comparison, the later-born exhibited both older mean ages and also smaller standard deviations from their mean ages in six of the seven comparisons. This finding could not be a result of conditions brought about by World War II, because all age data for athletes presented herein were obtained prior to the outbreak of World War II.

Detailed comparison of the chronological ages of greatest productivity in the several sciences reveals, likewise, a number of complicating factors. For example, men born from about 1775 to 1850 did their best creative work at somewhat younger chronological ages than did men who were born prior to 1775. Thus, in 12 of 15 comparisons that were made, the contributions of the more recent era were made at somewhat younger average ages.[1] It seems clear, therefore, that if a valid comparison is to be made of the ages of greatest productivity in the several sciences the century of birth of the several kinds of creative thinkers must be kept constant. In order to accomplish this goal the creative works of individuals who were born more than about two centuries ago have been omitted from the present study whenever such omission resulted in a significant difference in the average age of achievement.

Still another complicating factor was the finding that quality of output and quantity of output are imperfectly correlated. Therefore, no very accurate comparison of the ages of greatest proficiency in the several fields of science can be made unless the contributions under consideration are first equated upon the basis of their quality or merit.[2]

One other explanatory remark should perhaps be made. In Table 1 the number of creative achievements within each separate field of endeavor is purposely kept small because, when the number of assembled cases is large, less select creative works are likely to be included in any given list. When less select works are included, the average age of the performers is likely to shift either upward or downward, but more often the mean age moves upward. Hence, if care is not taken to avoid it, when the ages of various kinds of creative thinkers at time of stellar performance are found to differ, those age differences may be due to a difference in the quality of the performance rather than

TABLE 1
CHRONOLOGICAL AGES AT WHICH MEN HAVE MADE NOTABLE CONTRIBUTIONS TO VARIOUS SCIENCES, MATHEMATICS AND INVENTION

Type of endeavor	Number of individuals	Number of works	Peak years	Median chronological age
Practical inventions	86	135	30–34	33.36
Mathematics	27	42	20–24	34.00
Chemistry	47	52	26–30	34.00
Physics	44	80	30–34	34.60
Introduction or discovery of remedial drugs	44	73	30–34	34.70
Anatomy	70	86	35–39	35.71
Bacteriology	30	39	35–39	36.50
Pathology	84	107	35–39	39.00
Physiology	54	59	35–39	39.33
Classical descriptions of disease	52	77	30–34	40.17
Botany	50	144	30–34	40.33
Geology	65	99	35–39	41.08
Surgery	76	103	35–39	41.75
Entomology	86	86	35–39	42.00
Psychology	50	85	35–39	42.58
Astronomy	68	92	40–44	44.20

to the type of endeavor *per se*. Since the present study is concerned with achievement of the very highest merit, rather than with mere quantity of output, a studied attempt has been made to include herein only very select, and hence of necessity, small numbers of achievements within each separate field of endeavor. But this precaution introduces a new hazard, since small numbers of cases make for large probable errors in the means.

Although the present writer has tried to meet each of the above-mentioned difficulties as best he could, this could be done only approximately. For example, the problem of obtaining equally select creative works from within diverse fields of science, some of which fields obviously are much more backward than others, could not be solved in a perfect manner. Therefore, in studying Table 1 the reader should realize that for performances which have similar averages the differences in the median ages at time of achieving may not be significant. For behaviors listed near the top, as compared with those listed near the bottom of the table, the median age differences may be of genuine significance. But one should not be too certain that even this latter assertion is wholly valid. Therefore, in the construction of Figure 1 the data for the several kinds of creative work listed in Table 1 have been treated collectively.

Ages of Maximum Proficiency in Science, Mathematics, and Practical Invention. Figure 1 and Table 1 present the chronological ages at which approximately 933 deceased creative thinkers either made or first announced 1,059 outstanding discoveries in science, mathematics, and practical invention. The word "approximately" or "about" is used here and hereinafter in referring to the number of individual contributors because some individuals contributed to more than one field of endeavor. If it may be assumed that the present writer has succeeded in his

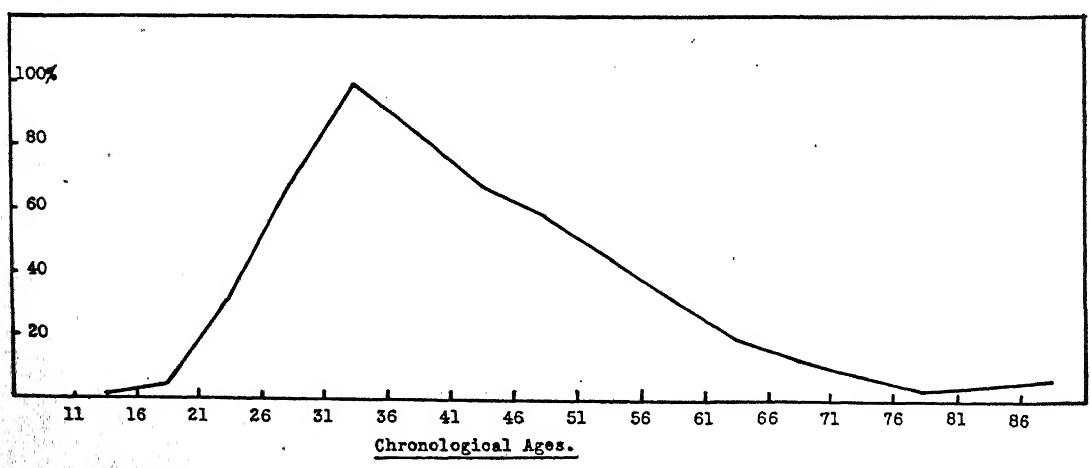


FIG. 1. AGE VERSUS PRODUCTION IN SCIENCE, MATHEMATICS, AND INVENTION REPRESENTING 1,059 SUPERIOR CONTRIBUTIONS BY 933 INDIVIDUALS NOW DECEASED (OR 1.13 WORKS EACH).

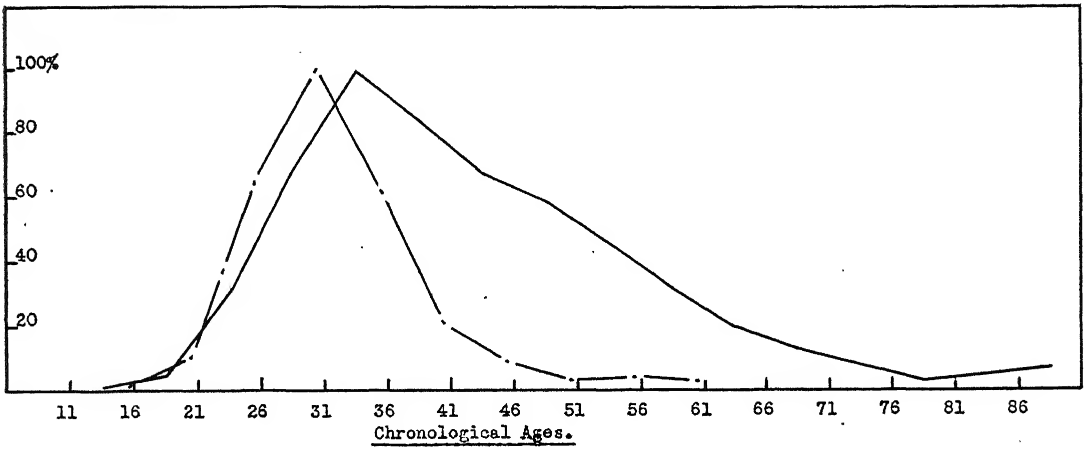


FIG. 2. AGE VERSUS OUTPUT IN SCIENCE AND PROFICIENCY IN CERTAIN SKILLS
Solid line, SAME AS FIGURE 1. *Broken line,* 1,175 CHAMPIONSHIPS IN 17 CLASSES LISTED IN TABLE 2.

attempt to include a good sample of man's most notable discoveries and inventions in Figure 1, this figure may perhaps be regarded as a generalized age-curve which shows the rise and fall of man's best (or almost his best) output in the fields indicated.

In the construction of each of the age-curves that accompany this article the several graphs were reduced to a comparable basis, and proper allowance was made for the mortality rate, by a method that has been described previously.[1] If, regardless of the number of workers that remained alive, the 50-year-olds had contributed at the same average rate as did the 33-year-olds, the curve in Figure 1 would remain as high at the 50-year-old age level as it is at the 33-year-old level. Actually, the curve exhibits a very noticeable descent beyond age 33, with no secondary peak at any of the older age levels. Figure 1 thus reveals that in proportion to their numbers men have made their outstanding contributions most frequently to the indicated fields during their early thirties.

When 5-year age intervals are used it is customary to employ age intervals 10–14, 15–19, etc. Detailed study of the tabulated data revealed clearly that the most productive 5-year period was that from ages 31 to 35, inclusive. In order to set forth this finding most vividly, the conventional age intervals were therefore abandoned in the construction of Figure 1, and those intervals which best fit the assembled data were used.

Athletic Success versus Scientific Discovery. In order to reveal its similarity to certain other age-curves, Figure 1 is reproduced in Figures 2 to 6 inclusive and also in Figure 11. Thus, the solid line of Figure 2 is identical with Figure 1. The broken line of Figure 2 sets forth, on the other hand, the ages at which the 1,175 important athletic championships listed in Table 2 were won.

When age data were assembled separately for only the best, or almost the best, of those relatively less vigorous championship performances in which precision of movement and nicety of neuromuscular coordination seem most likely to be the decisive factors in winning, i.e., for golf, billiards, rifle and pistol shooting, bowling, and duck-pin bowling, and when data for only the last-born 50 percent of the champion performers in these fields were included, the broken line of Figure 3 was obtained. For constructing this broken line of Figure 3 age data for 577 championship performances were employed. But, since some individuals held their championships for more than a single year, the number of different performers is somewhat less than is the number of performances.

Figure 3 discloses an important fact that would not be known if only the mean and the median ages were available. The difference in the mean ages of the scientists and of the athletes pictured in Figure 3, at the time of their stellar performances, is 7.58 years; the median age difference is 6.08 years. But

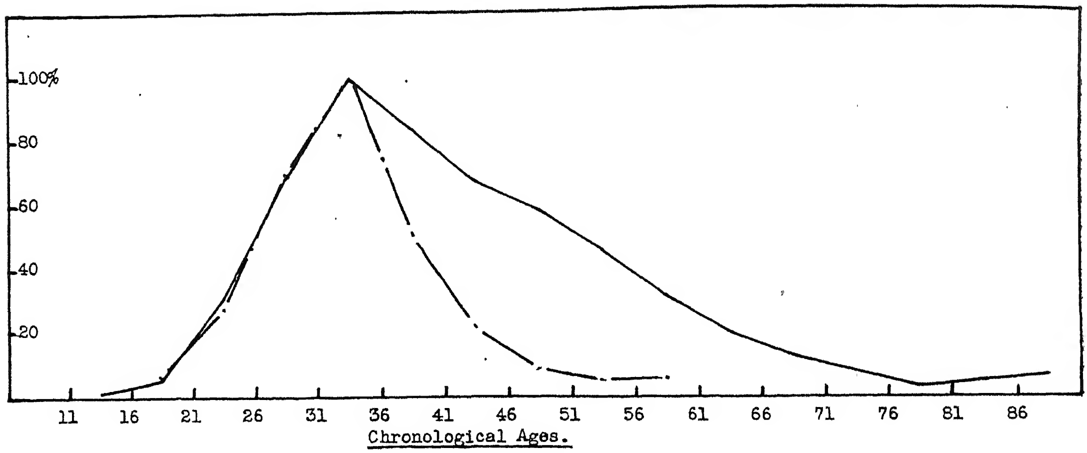


FIG. 3. AGE VERSUS OUTPUT IN SCIENCE AND PROFICIENCY IN CERTAIN SKILLS
Solid line, SAME AS FIG. 1. *Broken line*, 577 CHAMPIONSHIPS AT GOLF, BILLIARDS, RIFLE AND PISTOL SHOOTING, BOWLING, AND DUCK-PIN BOWLING—SKILLS OF THE NICEST NEUROMUSCULAR COORDINATION.

these differences are due almost entirely to the different rates at which the two curves of Figure 3 descend from their peaks. This difference is doubtless due in part to the fact that no time lag intervened between the winning of the athletic championships and the recognition that was received therefor. As regards the creative works, on the other hand, a long period of time must often have elapsed between the birth of the great idea and the

TABLE 2

AGES AT WHICH INDIVIDUALS HAVE EXHIBITED PEAK PROFICIENCY AT "PHYSICAL" SKILLS

Number of cases	Type of skill	Median chronological age	Mean chronological age	Peak years (mode)
89	U.S.A. outdoor tennis champions	26.35	27.12	22–26
49	Runs batted in: Annual champions of the two major baseball leagues	27.10	27.97	25–29
64	U.S.A. indoor tennis champions	28.00	27.45	25–29
77	World champion heavy-weight pugilists	29.19	29.51	26–30
31	Base stealers: Annual champions of the two major baseball leagues	29.21	28.85	26–30
82	Indianapolis-Speedway racers and national auto-racing champions	29.56	30.18	27–30
53	Best hitters: Annual champions of the two major baseball leagues	29.70	29.56	27–31
51	Best pitchers: Annual champions of the two major baseball leagues	30.10	30.03	28–32
127	Open golf champions of England and of the U.S.A.	30.72	31.29	28–32
84	National individual rifle-shooting champions	31.33	31.45	32–34
103	State corn-husking champions of the U.S.A.	31.50	30.66	28–31
47	World, national, and state pistol-shooting champions	31.90	30.63	31–34
58	National amateur bowling champions	32.33	32.78	30–34
91	National amateur duck-pin bowling champions	32.35	32.19	30–34
53	Professional golf champions of England and of the U.S.A.	32.44	32.14	29–33
42	World record-breakers at billiards	35.00	35.67	30–34
74	World champion billiardists	35.75	34.38	31–35

collection of supporting evidence, verification of the findings, etc.

Scientific Discovery versus Starring in the Movies. The broken line of Figure 4 sets forth the successive chronological ages at which movie actors (males only) have been found to be "best money-makers." As here used the term "best money-makers" refers to those actors who were the leading box-office favorites during the years 1915 to 1939, inclusive. The sources from which age data regarding the best money-making actors were obtained, and some observations regarding the actors, are published elsewhere.[3] The broken line of Figure 4 presents age data for 1,770 best money-making years but not for that number which descends more rapidly than does either of the curves of Figure 4. In view of the fact that the shapes of all performance age-curves are in part a function of quality of execution, one is led to wonder just how well the two curves of Figure 4 would coincide if a perfect technique were available for equating the two behaviors shown therein upon the basis of their rarity or difficulty of performance.

Contributions to Science, Philosophy, and Music. In Figure 5 the solid line which sets forth man's most creative years in science, mathematics, and practical invention is plotted by 10-year instead of by 5-year intervals. This makes it more directly compar-

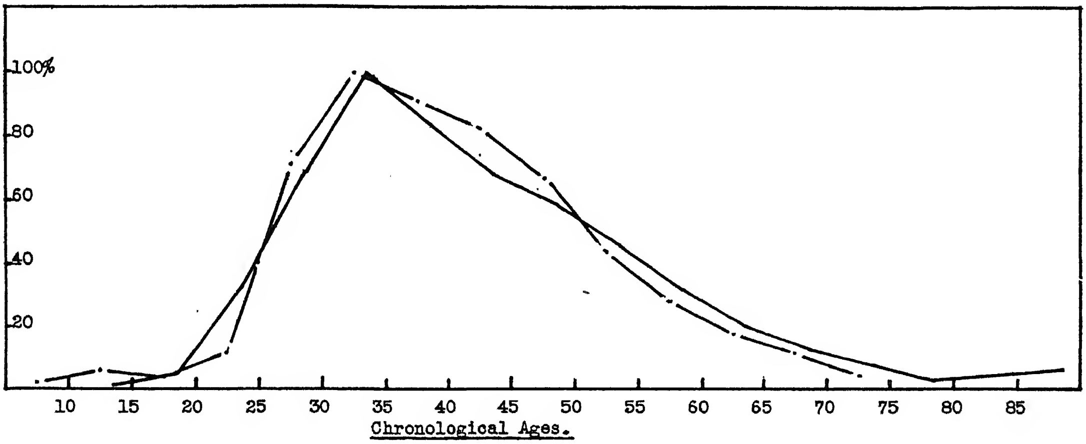


FIG. 4. AGE VERSUS OUTPUT IN SCIENCE AND BEST MONEY-MAKING MOVIE-ACTING
Solid line, SAME AS FIG. 1. *Broken line,* 1,770 YEARS AT WHICH MOVIE ACTORS LED AT THE BOX OFFICE.

of different individuals, since the same actor is often listed among the best money-makers for several successive years.

Although the solid line of Figure 1 is plotted again in Figure 4 by use of age intervals 11–15, 16–20, etc., those numerals which best fit the curve for the money-making actors have been used along the baseline of Figure 4. This procedure, which has been repeated in Figures 5 and 6, explains why the peak of the curve for scientific achievements seems to be slightly out of line (too far to the left) in Figures 4, 5, and 6.

The slight difference in the shapes of the two curves of Figure 4 should be interpreted in light of the fact that a more select list of only 131 best money-makers yielded a curve able with the two other curves which appear in Figure 5.

The dash line of Figure 5 presents the ages at which each of 97 noted philosophers, born from 1763 to 1850, either wrote or first published his one most widely quoted treatise. The 97 philosophers are the most recently born 50 percent of a group studied by the present writer. The reason for excluding age data for the earlier-born 50 percent of the philosophers has already been given. And the methods by which both the philosophers and the one most notable treatise of each were identified have been described previously.[4] When 5-year intervals were used, the resultant age-curve for the 97 philosophical contributions was found to be rather

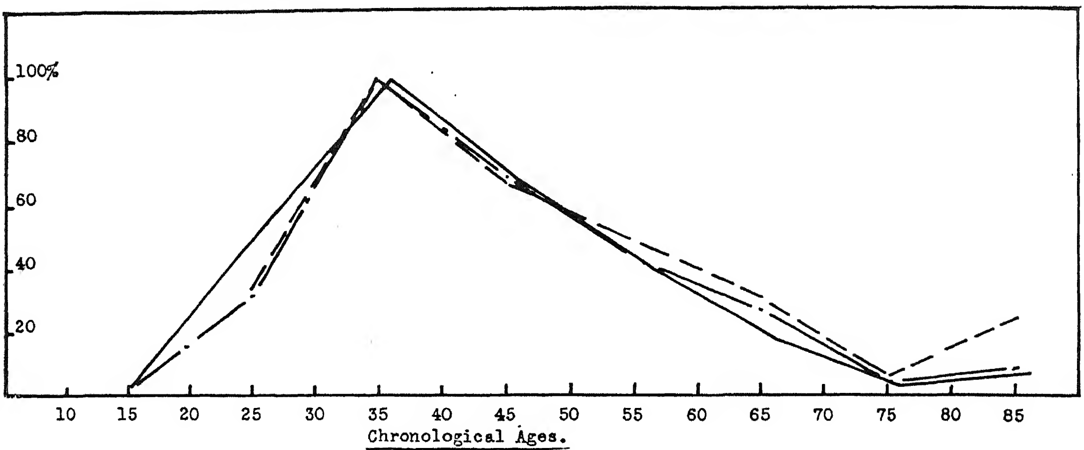


FIG. 5. AGE VERSUS OUTPUT IN SCIENCE, MUSIC, AND PHILOSOPHY
Solid line, SAME AS FIG. 1. *Broken line,* OUTSTANDING TREATISES BY 97 NOTED PHILOSOPHERS (1763–1850). *Dotted line,* 557 SELECT MUSICAL COMPOSITIONS BY APPROXIMATELY 382 COMPOSERS NOW DECEASED.

irregular. Therefore, the curve for the philosophical contributions was smoothed in Figure 5 by taking 10 years, instead of 5 years, as the unit. This procedure increases the number of cases within each class interval. It thus eliminates irregularities and sets forth more clearly the general trend of the age differences.

The broken line of Figure 5 is a composite curve which sets forth man's most creative years in seven different forms of musical composition—a total of 557 select compositions by approximately 382 deceased composers. The sources from which data regarding the musical compositions were obtained are given elsewhere.[5] All three curves of Figure 5 differ only slightly. For the three statistical distributions shown in Figure 5 the mean age differences are small and of doubtful significance.

Although it is popularly believed that teen-age contributors have been much more numerous in music than in other fields of endeavor, the similarity of the three curves of Figure 5 suggests that reconsideration of the foregoing hypothesis may be in order. Inspection of numerous lists of youthful achievements in diverse kinds of endeavor provides convincing evidence, not that the youthful musical composer has been overrated, but rather that prodigies in other lines of work have all too often been underrated or

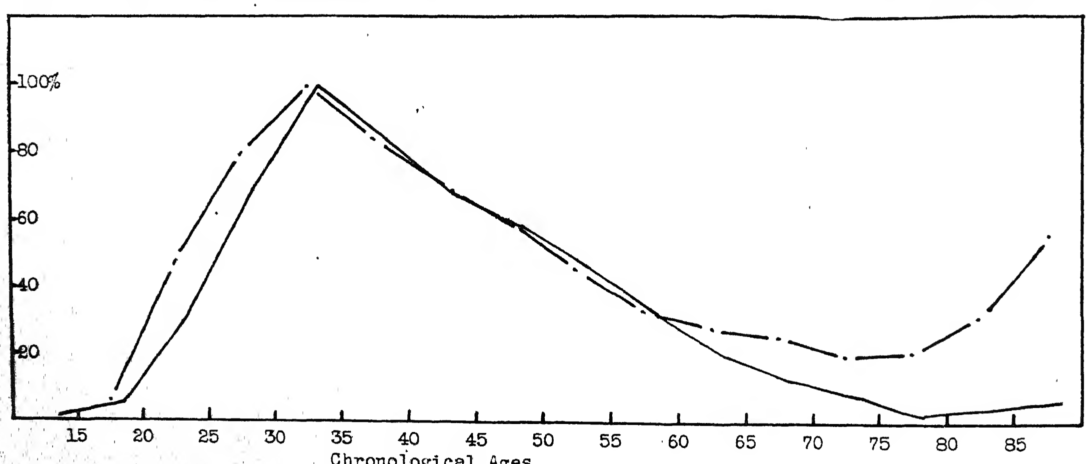


FIG. 6. AGE VERSUS OUTPUT IN SCIENCE AND LITERATURE
Solid line, SAME AS FIGURE 1. *Broken line,* 2,635 LITERARY WORKS BY ABOUT 242 DECEASED AUTHORS.

entirely ignored. In a very real sense these other prodigies have not been able to get the attention of so large an audience as have the teen-age contributors to music.

Scientific Discovery versus Literary Productivity. The rise and fall of man's best output in science, mathematics, and practical invention are shown again in Figure 6 by means of the solid line. The broken line of this figure reveals man's most creative years in 18 types of literature—2,635 literary works by approximately 242 deceased authors. The sources from which data regarding the literary selections were obtained have been published previously.[6, 7] The early ascent of the broken line of Figure 6 is probably to be accounted for in part by the fact that the literary works include numerous poems. And the late upward turn of the broken line of Figure 6 may be due in part to the fact that some of the prose selections used for constructing this curve are less select than are the scientific contributions used for drawing the solid line of Figure 6. For the scientific discoveries, the average number of contributions per individual contributor is 1.47. But for the authors the mean number of contributions per individual is approximately 2.75. The difference in the mean ages of the scientists and of the literary men at time of achievement is only 1.03 years.

Maximum Proficiency at Billiards and Golf versus Painting in Oils. The solid line of Figure 7 presents the ages at which 153 of the world's greatest artists either painted or first exhibited their one best oil painting. The method by which the 153 artists and the one best oil painting of each were identified has been described previously.[8] The broken line of Figure 7 sets forth combined data for: (1) the ages at which 42 world billiards records were broken by professional performers and (2) the ages at which 53 national professional golf championships were won either in England or in the United States. The age data for breaking world billiards records and for winning the golf championships were combined for constructing the broken line of Figure 7 both because the data for these two outstanding performances differ only slightly and also because both feats must have demanded the utmost in neuromuscular coordination. The irregular age intervals 17–21, 22–26, etc., are used in Figure 7 because, for each of the three skills pictured in this figure, maximum proficiency occurred at ages 32 to 36, inclusive.

Although the two curves of Figure 7 almost coincide from ages 19 to 39, inclusive, beyond age 39 the solid line which reveals the production of superior oil paintings sustains itself much better than does the broken line which reveals top-flight success at golf and at

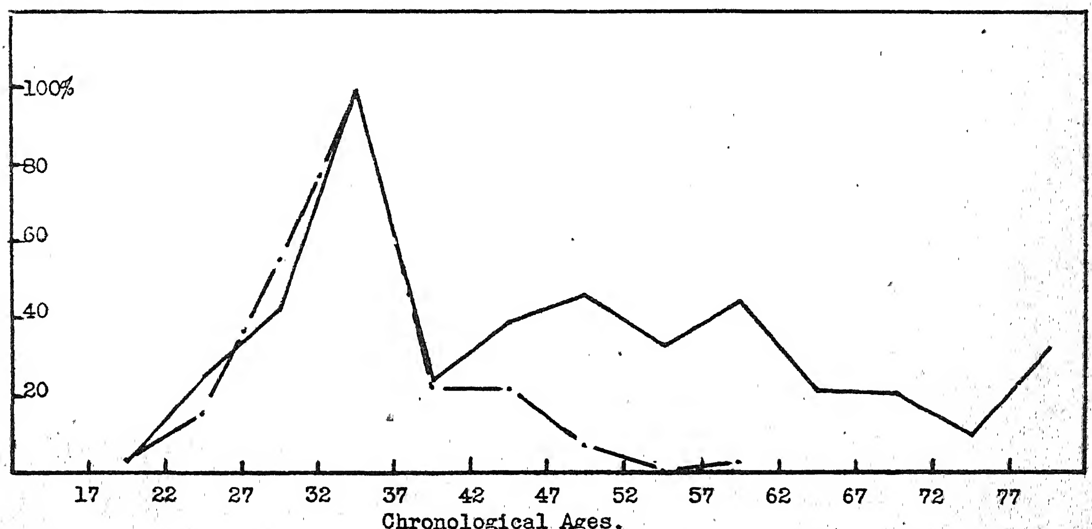


FIG. 7. AGE VERSUS PROFICIENCY IN BILLIARDS AND GOLF AND OIL-PAINTING
Solid line, 153 NOTABLE OIL PAINTINGS. *Broken line,* 93 BEST PERFORMANCES AT BILLIARDS AND GOLF.

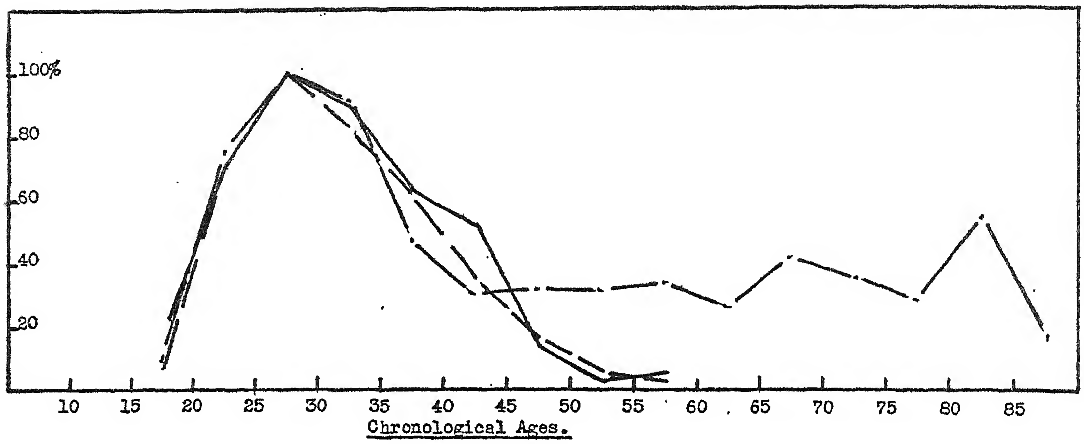


FIG. 8. AGE VERSUS MARKSMANSHIP, FECUNDITY, AND WRITING OF POETRY
Solid line, 630 RIFLE AND PISTOL CHAMPIONSHIPS. *Broken line,* AGES OF FATHERS OF 2,039,811 CHILDREN BORN IN UNITED STATES IN 1931. *Dotted line,* 797 NOTEWORTHY POEMS BY 82 WELL-KNOWN POETS.

billiards. The difference in the mean ages of the oil painters and of the athletes under consideration is 7.76 years, the means being, respectively, 41.37 and 33.61 years. If only these mean ages were known, one would hardly have suspected that the two curves of Figure 7 would so nearly coincide from ages 19 to 39. Obviously age-curves can reveal facts which bare statistical averages are unable to reveal.

The reader will perhaps find it easier to accept the early loss of maximum skill on the part of the golfers and on the part of the billiardists than to believe that a similar loss of skill overtook the oil painters during their early thirties. However, as has been explained in a previous study,[2] the accompanying age-curves should be interpreted with constant awareness of the fact that they reveal not the *rate* at which skill of the individual declines but merely those age levels at which peak performance is most often exhibited. Therefore, the long abrupt descents of the two curves of Figure 7 should be understood to mean only that ages 32 to 36, inclusive, are almost certainly the 5-year interval beyond which the typical performer has passed his zenith in the indicated fields of endeavor.

Composition of Poetry versus Biological Fecundity versus Marksmanship. The legends beneath Figure 8 supply the information needed for its interpretation. All three curves of Figure 8 have been published separately elsewhere, together with comment.[6,9] These curves are superimposed and republished here because of their marked similarity. The conventional age intervals are used in Figure 8 because the data for man's biological fecundity were grouped thus in the United States Census report. The sample of marksmen for whom data are presented by use of the solid line of Figure 8 is much larger and probably somewhat less select than are the two samples of marksmen for whom data are presented in Table 2.

Chemistry Contributions versus Professional World Championships at Billiards. In Figure 9 the solid line presents age data for 52 very superior contributions which were selected by two out of three university chemistry teachers as among the 100 greatest chemistry contributions of all time. The broken line of this figure sets forth the ages at which 136 professional world championships at billiards were either first won or retained. (The broken line of Figure 7 presented data regarding what is probably an even more difficult feat, i.e., the breaking of world records at billiards.)

For many activities usually thought of as being primarily of a "physical" nature, it is possible to find surprisingly similar age data in the realm of so-called "intellectual" achievement. For example, Figure 10 shows that for both football professionals[9] and also

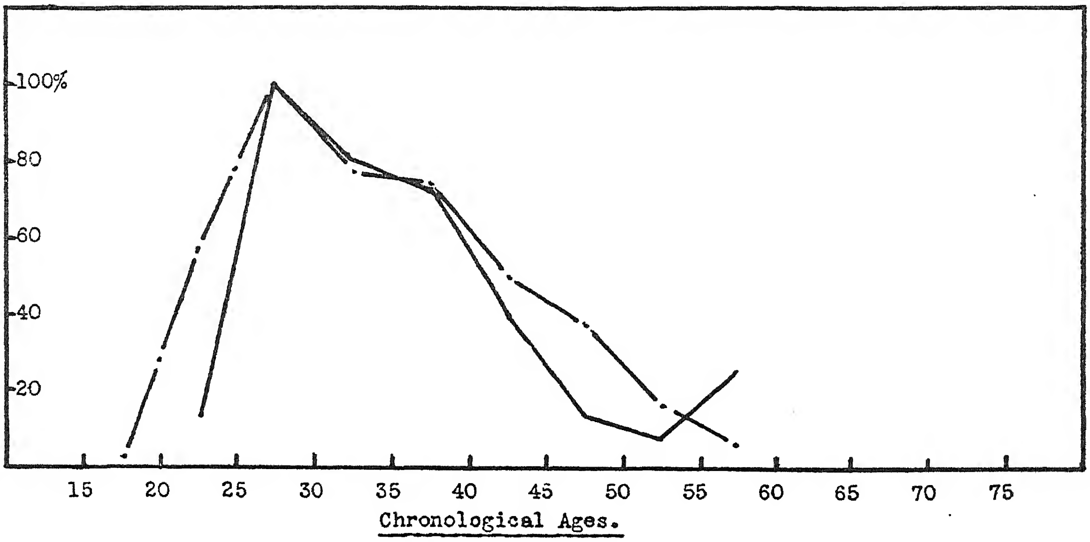


FIG. 9. AGE VERSUS OUTPUT IN CHEMISTRY AND PROFICIENCY AT BILLIARDS
Solid line, 52 GREATEST CHEMICAL CONTRIBUTIONS OF ALL TIME (SELECTED BY THREE UNIVERSITY CHEMISTRY TEACHERS). *Broken line,* 136 PROFESSIONAL WORLD CHAMPIONSHIPS AT BILLIARDS. SEE FIG. 7.

for composers of lyrics and ballads[6] the one most proficient 5-year interval is that from ages 22 to 26, inclusive. This finding for the football professionals may remind one of the preference on the part of the United States military authorities for servicemen who are less than 26 years old. One can easily believe that the ages of superior fighting ability and of superior football ability are concurrent. But why should the best lyrics and ballads be composed most frequently by youths who are the same age as football professionals?

Scientific Discovery versus Geographical Discoveries and Explorations. Are geographical discoveries and explorations "intellectual" or are they "physical" feats? Like all other human behaviors, they are at one and the same time both intellectual and physical. All thinking is regarded today as a biological phenomenon and the modern

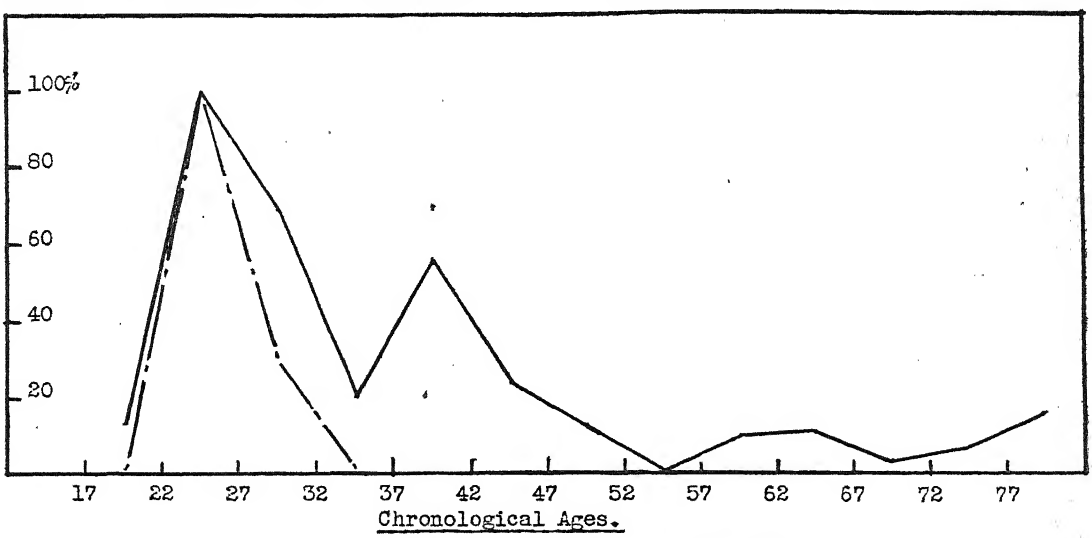


FIG. 10. AGE VERSUS FOOTBALL PROFESSIONALISM AND LYRIC-WRITING
Solid line, 148 LYRICS AND BALLADS BY 36 POETS. *Broken line,* 485 TOP PROFESSIONAL FOOTBALL PLAYERS.

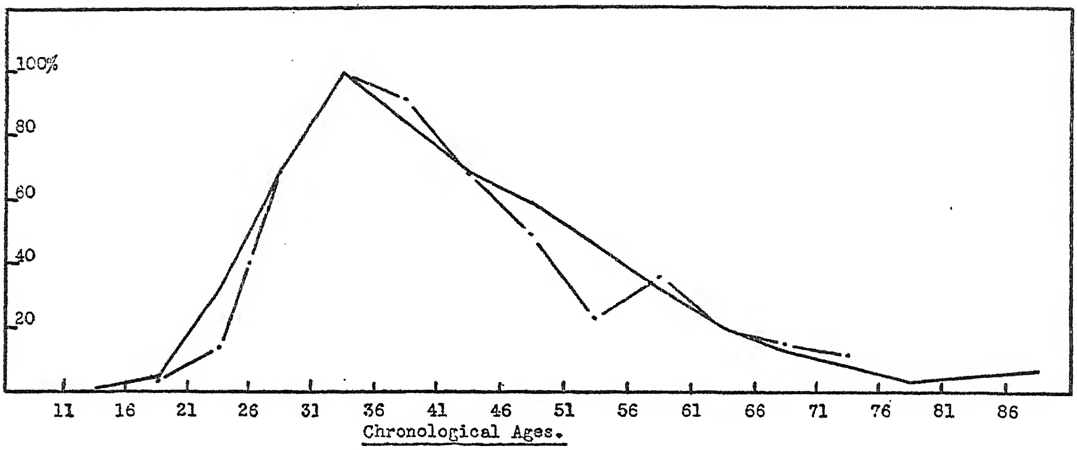


FIG. 11. AGE VERSUS OUTPUT IN SCIENCE AND GEOGRAPHICAL EXPLORATION
Solid line, SAME AS FIG. 1. *Broken line,* 202 GEOGRAPHICAL EXPLORATIONS AND DISCOVERIES (INCLUDING POLAR EXPLORATIONS) BY 152 PERSONS NOW DECEASED, AVERAGING 1.33 ACHIEVEMENTS PER INDIVIDUAL.

psychologist regards as futile the ancient attempt to distinguish between intellectual and physical performances. From this point of view the title of the present study is a bit naïve. The title was chosen because of the traditional use of the words "physical" and "intellectual."

In Figure 11 the solid line is a reproduction of Figure 1. The broken line of Figure 11 reveals the frequency with which notable geographical discoveries and explorations (including polar explorations) have been made at successive age levels.[10] Oddly enough, men seem to have explored and discovered important geographical facts with about the same frequency at different ages as they have made their other more important discoveries.

Concluding Remarks. It may require more intellectual acumen to formulate a fruitful hypothesis and to decide upon the kind of data that need to be collected, and how to assemble them, than does the actual work of collecting the factual data. In order to make proper allowance for time elapsing between the inception and completion (time lag) of a brilliant scientific study, each scientist's creative work would have had to be made a separate research problem, requiring so much time and energy that this study could hardly have been published at all. Therefore, secondary sources have been used, and time lag has been ignored except in that minority of instances in which the amount of time lag was specifically mentioned in the secondary source. When time lag was thus ascertained, fractional credit was allotted equally to those age intervals during which a given idea was being developed. However, for the reasons stated above, it seems probable that on the whole this procedure has yielded age-curves which are slightly overgenerous rather than undergenerous to the older age groups.

In this study we have purposely avoided consideration of the ages at which various types of leadership are acquired and retained. It has been shown in an earlier study that in our present-day society positions of authority, prestige, responsibility, and influence tend to be vested in (or gravitate to) middle-aged and elderly individuals.[11] The present discussion is limited, therefore, to the several types of performance cited herein. For these there is perhaps much more overlapping in the modal ages of peak performance than might have been anticipated.

For mathematicians, for inventors, and for the several other kinds of creative thinkers listed in Table 1, the median ages at time of contributing their best (or almost their best) work range roughly from 33 to 44, inclusive. But when the contributions of Table 1 are treated collectively, the one year of maximum productivity (the modal year) is age 33.

For seven kinds of choice musical selections the median ages of the composers at

time of composition or first publication range from 27 to 42, inclusive. When assembled into a single statistical distribution, however, the modal year for the composers is age 33. For the authors of eighteen types of superior literary works the median ages at time of contributing range from age 26 to age 44, inclusive, almost the same age range as that found for the composers. But when the literary contributions are combined in a single tabulation, the one year of maximum output is age 32.

The above observations possess additional interest in view of the finding that the more important championships at golf, billiards, rifle and pistol shooting, bowling, and duck-pin bowling, have also been won most frequently at ages 31 to 36, inclusive (see Figure 3). For movie actors and for the more recently born philosophers maximum success has been attained most often at age 32. For oil painters the best work has been done most frequently at *not later than* age 34.[8] Collectively, the writer's investigations to date reveal, for the following rather diverse types of performance, that peak attainment is most likely to be achieved during and early thirties:[12]

1. Golfers—professional champions, and the most recently born 50 percent of the Open Golf Champions of England and of the United States.
2. Billiardists—World Record Breakers and the most recently born 50 percent of the World Champions.
3. Pistol shooters—Individual World, National, and State Champions.
4. Rifle shooters—Individual National Champions.
5. Bowlers—National, Individual, and All-events Champions.
6. Duck-pin bowlers—National Champions.
7. Composers of seven types of choice musical selections.
8. The authors of eighteen types of select literary works.
9. Painters in oil and etchers (see Ref. 8).
10. The most recently-born 50 percent of the kinds of creative thinkers listed in Table I.
11. The most recently born 50 percent of the greatest philosophers.
12. Best money-making movie actors (see Ref. 3).
13. Geographical discoverers and explorers.

For the more intellectual performances listed above the probable error is about 7 or 8 years. For the less intellectual performances the probable error is approximately 4 or 5 years. These probable errors are approximate only; they vary somewhat with type of achievement, quality of execution, size of the sample, etc. On the whole it seems apparent that the nicest neuromuscular coordination and the best creative thinking must occur (most frequently) at very nearly the same chronological age level. This seeming agreement in such widely different fields of endeavor, as regards the age level at which peak attainment is most likely to be achieved, seems too good an agreement to be the result of mere coincidence. Could it result largely from some fundamental characteristic of the human organism—something in the nature of a fixed order of human development? It, of course, is much too early to answer such a query as this with great positiveness. However, these data seem to conceal something that needs to be explained and which, when better understood, can hardly fail to be of utmost importance to *Homo sapiens*.

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ELECTRICAL COMMUNICATION

By JOHN MILLS

DIRECTOR OF PUBLICATION, BELL TELEPHONE LABORATORIES, NEW YORK

Electrical communication embraces a number of arts, such as telegraphy, telephony, radio broadcasting, and television, that in their early development seemed widely different but today are recognized as based on common principles of physics and electrical engineering. The separate arts differ most obviously in their terminal apparatus—a telephone transmitter for transmitting information in the form of spoken sounds does not look like a teletype sender for printed words nor like the picture-sending equipment of a "facsimile" system or of a television system. Nevertheless, all these instruments operate on common principles.

It is in the means for transmitting energy electrically between the various types of terminal apparatus that there is the greatest similarity. For example, in the long-lines plant of the Bell Telephone System a pair of conductors may at one time be carrying a telephone conversation and a little later, if the traffic requires, the same conductors may be used for the simultaneous interconnection of twelve teletype senders and printers; or they might be required to carry a program between a studio and a broadcasting transmitter, or for that matter, a picture from one newspaper to another. Much of the equipment will serve equally well for any type of electrical communication. Even when it comes to television, where the requirements are more severe because a complete picture must be transmitted every thirtieth of a second, there is still similarity of equipment and a common basis of principles. A wire or radio system, however, which can transmit a television program can be used, alternatively, for the simultaneous transmission of several hundred different telephone conversations; or, if required, it could transmit without mixing them several scores of ordinary sound programs for broadcasting stations.

In electrical communication the necessary operations have to do with either the transmission of the desired signals or the switching, routing, and control of channels over which the transmission takes place. The second group of operations is often as difficult technically and as expensive as the transmission itself. Two schoolboys with a telegraph line between their homes do not encounter this second problem of communication, but a telegraph company does. Whether its channels for communication are entirely wire or partly radio it wishes to use these facilities to their fullest, getting through as many messages per hour as possible so as to reduce the cost per message. That is one reason, for example, why machine sending from perforated tape and reception by teletype has replaced the old manual system of telegraphy; the machine can be set to run steadily at the fastest rate the apparatus and channel can take.

The problem of switching and controlling channels becomes most complex for telephone service in metropolitan areas where connections must be made between hundreds of thousands of subscribers. In a telephone central office, therefore, whether manually operated or mostly under dial control, there is always for each subscriber many times as much equipment as he has in his home.

In the electrical transmission of information the operations are basically the same whether the signals to be transmitted are according to a code of telegraphy or teletype; or according to the codes of spoken language or of music; or according to arrangements of light and shade, as in picture and facsimile transmission or in television; or according to special and arbitrary codes, such as are required in remote control by electrical mechanisms whether in industrial processes or in military operations, such as the electrical gun director of anti-aircraft batteries. Four operations are essential in every case, but two others are necessary in all except the simplest systems.

All these various operations are most easily explained by beginning with those corresponding to the ordinary transmission of speech. First there is required a power source to generate an effect that can be transmitted from the speaker to the listener; this source consists of the diaphragm and larynx of the speaker. The next requirement is a means for varying the effect so that it will convey the information it is to carry. The acoustic output of the larynx is therefore modified—*modulated* is the proper technical word. This is accomplished in the resonating chambers of the mouth which are controlled by tongue, palate, and lips. The next requirement is a medium which can transmit the modulated effect. And fourth and last, a mechanism is necessary that can detect the effect—or, in more general terms, that can *demodulate* and derive the information. The transmission is accomplished, of course, by a wave motion of air molecules, and the demodulation by the ear of the listener.

In their electrical counterparts these four basic processes are conveniently traced by considering successively an ordinary house bell-circuit, a telegraph hook-up and the simplest telephone. In the bell circuit a battery is the usual source of power; wires form the connecting medium through which can flow the electrical current; the push button is the modulating mechanism which varies the current from none to a maximum when it is pushed; and the ringer is the demodulator.

It is to be noted, that this simple circuit is limited in the information it can transmit—all it can tell a person in the house is that someone is at the door. By a simple code of long and short rings, intimates of the family can transmit somewhat more information. For more complete information a more elaborate code is required, such as those of telegraphy, but for such a dash-dot code push-button and ringer are awkward or unsatisfactory instruments; hence they are replaced by the telegraph key and the sounder which click-clacks with each down-and-up of the key.

This explanation of simple telegraphy does not follow the historical order of its development and so would seem to minimize the remarkable invention of Samuel F. B. Morse. Such, of course, is not the intent, for the only purpose here is to make clear the basic principles of all electrical communication. In the same way it is convenient to pass lightly over the wonderful invention of Alexander Graham Bell and to consider the present telephone as the next step in this exposition.

In a telephone system the key of Morse, which could vary the current only from no current to full current, is replaced by a telephone transmitter. In one form—the simplest for this exposition—the transmitter is a variable resistance, formed by loosely packed grains of carbon which respond by packing more closely or less closely as the transmitter diaphragm vibrates back and forth under the alternating pressure of the sound waves of the speech. The battery current is thereby modulated in essential conformity to the speech. The receiver, like the Morse sounder, is an electromagnet, but its armature is a thin diaphragm which can respond accurately to the variation in the current that the transmitter diaphragm causes; and so this diaphragm vibrates just as does that of the transmitter and sets the adjacent air into similar vibration, reproducing the sound.

The four essential operations of electrical communication are thus seen to be: generation of a current or other effect suitable for transmission, its modulation to put in the signal, its transmission, and its demodulation to recover, to re-create, the signal. In picture transmission, for example, the current is varied to correspond with the intensity of light reflected from (or transmitted through, when scanning films) the detail of the scene which the transmitter is observing. At the other terminal there is a light-producing mechanism which responds with light of corresponding intensity. These are also the essential elements in a television system.

The modulator and the demodulator go in pairs; corresponding to the telephone transmitter, which responds to sound, there is a receiver which reproduces sound. The former is sound-sensitive, the latter sound-active. Similarly, as noted above, in facsimile and television one element in the system is light-sensitive and the other light-

active. In a teletype system one responds to finger pressure, and the other, in effect, re-creates a finger pressure on a typewriter key.

The cause of the modulation does not have to be sound waves or light, as in the foregoing instances; it can be an electric current, as it is in the ordinary radio transmission of today. In early radio telegraph systems—which in those days were called wireless—this was not so; a current suitable for radio transmission was generated and supplied to the transmitting antenna and this current was modulated by a key in circuit with it exactly like the modulation of a battery current by a key in a wire telegraph system. Today in all radio systems for telegraphy, telephony, or television, the current for the antenna is modulated by current which comes from an ordinary wire system for telegraphy, telephony, facsimile, or television.

Radio uses in its sending antenna—and thereby establishes in a receiving antenna—a high-frequency alternating current. In the other systems so far considered in this article, the current which is modulated is a direct current like that from a battery. In the circuit connected to the battery, current flows in only one direction, determined by the way the battery terminals are connected. If a switching arrangement were introduced to flip-flop the battery connections, sending current through the circuit alternately in one direction and then in the other, the current would be called alternating. Unless the alternations took place very frequently, e.g., well above the audible limit, such an alternating current would be entirely unsatisfactory for telephony, since the slowly alternating current would dominate and mask the speech it was carrying. The current from power lines of public utility companies is alternating current. Its frequency is 60 cycles per second. That means that the current starts in one direction, rises rapidly to its maximum, decreases to nothing, reverses direction, increases to the same maximum intensity in this reversed direction and then decreases to zero current; and it goes through this whole cycle of variation 60 times a second. That cycle of variation is typical of alternating currents. Note also that "frequency" is "the number of cycles each second."

In an antenna a current of such low frequency as the usual power current would produce an entirely negligible amount of radiation. The higher the frequency, other things being equal, the more the radiation. For radio transmission currents are used which have frequencies in the hundreds of thousands; for example, WNYC in New York City, which has a frequency of 830,000 cycles per second or as more conveniently stated, 830 kilocycles, is about in the middle of the broadcasting range. Still higher frequencies are used for oceanic transmission, television, and many other purposes.

In radio transmission a current of high frequency is supplied to the transmitting antenna. *This current is modulated in accordance with a much lower frequency current upon which has been impressed by a previous modulation*—as in telegraphy and telephony—*the signal which it is desired to transmit.* Any effect produced by the current in the antenna is radiated and causes in a receiving antenna a corresponding but *much* smaller current. This is then demodulated—"detected" was the earlier word, in days when the only wireless transmission was telegraphic—and this demodulated current is equivalent except in strength to the current which originally modulated the high-frequency current. This detected current then travels over a wire circuit—short or long as circumstances dictate—to the receiving equipment, such as telephone receiver, loudspeaker, or teletype, where the final stage of demodulation is accomplished and the information it contains is made available.

This whole operation is conveniently described, in the usual technical terms, by saying that the audio-frequency current of a telephone or telegraph line is caused to modulate the radio-frequency current so as to be *carried* by radiation to the distant station. There demodulation of the antenna's radio-frequency current takes place in two steps, the first to produce a current *equivalent in significance* to the audio-frequency current, and the next to derive the significance embodied in this audio current.

The process of successive modulation or demodulation can be repeated. In so-called "carrier current" systems in the wire telephone plant of the Bell System an audio-frequency current derived from a telephone transmitter is caused to modulate a current well above the audible range; and this modulated current, in turn, is caused to modulate a still higher current. By successive operations of modulation a signal can be given a higher and higher frequency. Demodulation also can be accomplished in separate successive steps; and these steps can be chosen as desired, provided only that the total of their operations reduces the frequency of the received signal to the audio-frequency of the originating signal.

The fact that in radio transmission a signal, or a signal-bearing current, modulates a current of high frequency suitable for the desired jump through space is the explanation of the multichannel characteristic of radio. A large number of different signals—speech, music, or code—can be transmitted simultaneously over the same route, without mutual interference—provided that each signal is carried by a different frequency. In ordinary broadcasting there can be sent out a different program for every 10,000 cycles difference in the frequency of current used in an antenna. (That is true only of so-called amplitude modulation and even then depends upon the precision—and hence size and cost—of the radio receiver. In so-called frequency modulation the difference in "carrier" frequency of stations is usually 200,000, instead of 10,000, cycles per second.) To avoid interference between radio stations assigned to different frequencies there must be *stability* in the frequency of their carrier currents; this condition is accomplished usually through the use of tuning elements made of quartz. It is even more important that receiving stations shall be able to tune sharply to the desired transmission, accepting that and suppressing currents of all frequencies outside the desired frequency "band." These requirements are over and above any advantage in discrimination due to directional transmission and reception.

The ether, in other words, is a medium adapted to multichannel transmission provided that to each channel is assigned a distinctive band of frequencies and the receiving equipment is similarly discriminating. Whenever the same medium, ether, wire, atmosphere, or sea water, is to carry simultaneously and without interference more than one message there must be performed appropriate operations of *selection,* of which the tuning of home radio sets is a crude example. This is the fifth of the six operations which are basic to electrical communication.

Selecting according to frequency, years ago, made possible the simultaneous transmission over a single pair of wires of a telephone conversation and a telegraph message. A telegraph signal manually sent occupies a frequency band from about zero to a hundred or so cycles per second. A telephone conversation requires a frequency band from about 200 cycles per second up to two or three thousand. At the central long-distance office where they are received they can be separated by a "composite set," formed by inductances and capacitances, which lets through the telephone currents but diverts the telegraph currents toward their own receiving station and its sounder.

That was an early instance of multichannel operation, involving selection according to frequency. In a manner similar to that of radio transmission, using different frequencies to carry different messages, wire systems have been constructed which permit as many as 480 simultaneous but independent telephone messages.

In such wire systems—so-called carrier current systems over coaxial cable—the signal-bearing currents during transmission are highly attenuated, that is, weakened. The physical explanation is difficult and usually expressed with mathematical symbols, but the fact remains that alternating currents in traveling along wire lines, even perfectly insulated lines, suffer continuous reduction in strength, and more so in higher frequencies than in lower. In extreme cases the current may be reduced ninety percent a mile. That means a mile out it will be only one tenth its original value; and only a tenth of that, or one hundredth of its original, two miles out. Such weakening will soon make

the signal too small to affect the receiving apparatus, as well as so feeble as to be drowned out or masked by any interfering currents that may be picked up en route. Therefore, amplification along the route is necessary; this is accomplished by introducing electron tube amplifiers in proper circuit arrangements at regular intervals along the line.

This operation of amplification is the sixth of the operations required in electrical communication. These operations may now be summarized as follows: generation (of a current of appropriate frequency—remembering that the direct current of ordinary telephony is zero-frequency); modulation; transmission; selection, according to frequency; demodulation; and amplification at any stage where it is desired. In radio, amplification can be introduced only at the terminals, first to build up the signal before supplying it to the antenna, and second at the receiving station to increase the feeble effect to adequate intensity. At the latter station amplification may be introduced immediately upon reception and before any demodulation; and successively after each intermediate demodulation until the audio-frequency is supplied to the final demodulator which re-creates the signal. Along wire lines, whether carrying the audio-frequencies of ordinary telephony or the higher frequencies of carrier-current transmission, amplification can be introduced where and as required.

Electrical communication can be described in terms of signals and terminal apparatus as telegraphy, telephony, television, etc. It can also be described in terms of the medium through which transmission takes place, as through wire or by radio through the ether. The wire medium may be ordinary open wire, twisted pairs in cable sheaths, coaxial conductors where one is a cylinder surrounding the other, or so-called wave guides which, in effect, are metal pipes that enclose a portion of the ether and guide transmission. These media differ characteristically in the efficiency with which they can transmit various frequencies, but within those limits and when supplied with properly modulated current all of them can transmit signals of any of the kinds previously mentioned. Transmission also can be conveniently classified according to frequency; for example, audio, wire-carrier, and radio. The upper range of wire-carrier frequencies overlaps the range of frequencies used for ordinary broadcasting. The radio range, however, extends many times higher. The frequencies near its presently practical upper limit are also those which are suitable to wave guides.

Summarizing, it is to be noted that in electrical communication the historic divisions, like telephony or radio now overlap and are no longer sharply distinguished. In any communication system, however, will be found four, or more, of the six basic operations[1] of generation, modulation, amplification, transmission, selection and demodulation.

[1] Using the terminology of this article there arise cases where the operations of generation and modulation are performed by a single piece of equipment, as in the sound-powered telephone. In this instrument, which was the first to be invented by Bell, the sound waves vibrate a diaphragm in the field of a permanent magnet and so generate corresponding currents in a surrounding coil. In principle, the transmitter is just the ordinary receiver. Conversation is possible between two ordinary telephone receivers, speaking into one and listening to the other.

For simplicity in the preceding discussion the operations of converting energy of wave motion in air into signal current and vice versa have been called modulation and demodulation. It is usual, however, to restrict the terms modulation and demodulation to shifts in signal frequency and not to apply them where energy changes from one medium to another, as from electromagnetic to acoustic. Strictly speaking, the key, the telephone, the receiver and sounder are not modulators or demodulators. But little is lost by so calling them; and much, it is believed, is gained in simplicity of exposition.

A BIOLOGIST REFLECTS UPON OLD AGE AND DEATH*

By FRANCIS B. SUMNER
SCRIPPS INSTITUTION OF OCEANOGRAPHY, LA JOLLA, CALIFORNIA

A year ago I passed the biblical milestone of three-score-years-and-ten. One who has done this must admit to being an "old man." Now and then, we hear the boastful claim of a man of my age that he "feels as young as he ever did." I make no such absurd claim. While keeping up, to a limited extent, my professional labors, I am conscious of both physical and mental deterioration. I shall not discuss the former. Few persons outside the medical profession are interested in senile pathology. But the mental life of an aging man, if honestly told, may present some points of interest.

The chief change which I can detect is an obvious and familiar one, the lack of driving power. It takes me longer than formerly to think a thing through, and I am less likely to be successful in the attempt. I find it increasingly difficult to concentrate upon a problem or even a simple bit of reading. Also mental fatigue ensues far sooner. That earlier enthusiasms have largely waned is, of course, part of the same psychophysical picture.

On the whole, the world appears less vividly real to me now than in earlier life. My sense-organs are nearly as keen as ever, but some sort of subtle veil seems to have interposed itself between me and the rest of things. Impressions, whether of persons or objects, are likely to require repetition if they are to register beyond the passing moment. And when repeated they often come to me as wholly new experiences. Or at least they seem so until I have had time to recollect.

The failing memory of the old is only too familiar to everyone, whether he has already experienced it or has merely witnessed it in others. Almost equally familiar is the distribution of remembered facts in time. I can, for example, recall the scientific names of some animals and plants which I learned in my teens, and which I have seldom thought of since. Nowadays, the names are far less likely to stick. In some cases, too, I remember more of the contents of a book which I read in my student days than of one which I read a year or two ago. Naturally, learning has become a much more difficult task. From time to time I am made aware that some recent experience has dropped out of my mind completely. And I reached, some years ago, that rather embarrassing stage when I halt abruptly in conversation baffled by some elusive name or word with which I may have been familiar most of my life.

I have not followed the findings of present-day psychologists regarding these phenomena—whether they are attributed primarily to a failure in the mechanism of attention, or to a deterioration in the quality of the material in which the records are registered, or to something different from either of these. There is, or course, one obvious explanation of the greater persistence of earlier memories which would seem to have some inherent probability as a partial explanation of the facts. Those were the first experiences on the ground and they pre-empted the territory to the exclusion of later arrivals. One's capacity for the storage of these records must, at best, be limited.

I can fully confirm the general belief that original ideas are chiefly the products of younger minds. I should be hard put to it, I admit, to define an "original idea," but I think that we all mean pretty much the same thing by the words. They are things

* This essay is offered in the conviction that scientists as a class are "tough minded" enough to face every aspect of reality, even some of those aspects which may be repulsive to more timid souls. I am firmly of the belief, too, that the discussion which follows relates to matters which are susceptible of scientific approach, and that it is, therefore, strictly relevant to the pages of a journal of popular science. In fairness I should state that this essay was commenced, and most of it written, as much as ten years ago. This circumstance should be borne in mind by anyone interested in evaluating the present mental status of the author! This discussion has been revised from time to time, however, and it represents my present views upon the topics dealt with.

which frequently seem to come to us from without, which "enter our heads," as we say, with or without a previous period of mental effort. Yet, curiously enough, we prize them as the most nearly our own of all our possessions. Whatever their source, they pay much less frequent visits to the senescent brain. Much that we say and write in our later years is material which we threshed out earlier in life. It may seem to be new at the time of its final emergence—possibly we may never have said it before—but the matter, in many cases, was thought out years earlier. Most of the ideas and points of view in which I take any personal pride at the present time I can trace back to the third or fourth decades of my life, or perhaps earlier.

Nevertheless, I do not think that the facts justify the youthful conception of an old man's thinking as being the mere mechanical repetition of mental processes which were started in earlier life and are continued only through force of habit. A considerable capacity remains for the rearrangement of old elements into new patterns. Even the dotard who tells the same story for the hundredth time probably makes some little variations in telling it.

On the credit side of the account there is the generally conceded advantage of the older man from the standpoint of experience. His mental machinery may lose in efficiency with the lapse of time, but it has an increasing stock of knowledge at its disposal. And this stock of experiential material keeps on growing throughout life, or at least until senile amnesia gets the upper hand. Some time one of our experimental psychologists —if one has not already done so—will plot the curves of growing experience and of declining power in the life of the average man and determine the point at which the combined values of the two variables give the highest sum. This will reveal the time at which the "ripened judgment" of later life reaches its peak, at least for that mythical being, the "average man." One would have needed entirely different curves for Justice Oliver Wendell Holmes, Doctor W. W. Keen, or James McKeen Cattell.

Old age is frequently pictured as a bleak period—the "winter of life." This, as we all know, is subject to numerous exceptions, but statistically the picture is little exaggerated. The aging person not only loses some of his chief sources of enjoyment, but in a greater or less degree he loses his capacity for enjoyment. On the other hand, his sources of suffering usually increase, while his capacity for suffering is probably little diminished. "The best is yet to be" is a beautiful expression of pietistic optimism, but it is worth noting that the author of these words was far from having reached "the last of life, for which the first was made." Another poet is perhaps more realistic when he speaks of the "lean and slippered pantaloon."

There are many, let us grant, who pass serenely through this period of life to the end, and are able to say with truth that they are happy. In general, it is likely that such persons have not suffered the worst of the "slings and arrows of outrageous fortune." However, the chief explanation is doubtless not to be sought in external circumstances. Their happy state is a state of mind, dependent upon their temperament and their philosophy of life. So far as these are matters of organic inheritance, we can do little to control them. The most sanguine eugenicist has not proposed a way of breeding for optimism. But the probability is that temperament and *Weltanschauung* are in no small degree the product of early environment. If so, the ideal education would be that which would train the individual to face life, and in the end old age and death, not only courageously but contentedly. How far we are from such a goal is evident from the fact that few persons associate the word education with guidance in these directions.

This last is the traditional sphere of religion. But religion, in the sense in which most of the world uses the word, is rapidly losing its hold upon thinking persons. Plainly, some more intelligently conceived means must be found of bringing people to "accept the universe"—to appreciate its beauty and blink its hideous cruelty. This key, when found, will consist in a drastic remodeling of our psychophysical

reaction patterns and not in the establishment of mystical relations with any outside power. "The Kingdom of God is within you!" But only the wildest enthusiasts would date our arrival at this goal within any specified number of generations. In the meantime, those who are dominated by wishful thinking, along with that vast multitude who scarcely think at all, will take comfort in the notion of a "future life." Perhaps it is best so, at least for those who are not likely to be disillusioned by the growth of their own minds.

And now for my reasons for rejecting that comforting illusion. We may grant at once that science is not in a position to "solve" this problem in the sense in which it solves problems of physics or chemistry or biology. Very probably it never will be. But I do insist that, however poorly he may be equipped for the task, the scientist is better equipped on the average than the person who lacks the scientific background. I have in mind, of course, the scientist who is also a constructive thinker, not the mere cataloguer or routine observer.

Confessedly, the grounds for one's beliefs respecting such a problem as that of human immortality are largely matters of common experience which we all share alike. The scientist's chief advantage in dealing with these matters lies not in his exclusive possession of specially relevant data but in his more nearly impersonal and objective approach to the problems involved. This is written in full realization of the dogmatic and speculative character of much recent science, and a recognition of the obsessions which beset some of its votaries. To one who is layman both in theology and in recent metaphysical physics, there would seem to be little ground for choice between such a religious doctrine as that of "transubstantiation" and such a scientific doctrine as the "principle of uncertainty." They seem equally incredible, if not indeed both fundamentally unintelligible. But few would probably doubt that by and large the scientist is far more generally guided by evidence than the theologian or religious philosopher, and far less frequently by preconceptions or emotional bias.

It is true that these assertions respecting the *relatively* favored position of science in interpreting man and the Universe are quite contrary to claims which are often made by the devotees of organized religion, claims which are too often chivalrously conceded by scientists themselves. The two fields of thought, it is contended, are quite distinct and unrelated, and the problems of each should be left to its own votaries. "Render unto Caesar," etc. Who are better fitted to pass upon the problems of religion than the "specialists" who have devoted their lives to this field of study?

Such an argument ignores, of course, the fact that the "specialist" in religion receives his fundamental data through essentially different channels from the "specialist" in science. The latter is taught, wherever possible, by observation, demonstration, and experiment. In theory at least, every conclusion reached could be checked by the student, if given adequate time and equipment. The "specialist" in religion, on the contrary, receives his fundamentals mainly on authority, and under conditions such that any really searching criticism is discouraged or forbidden. That scientific branches are also frequently taught dogmatically to the student is a lamentable fact, but it does not affect the average accuracy of my statement.

What then does science have to say regarding this fundamental tenet of almost all religious faiths—the belief in the survival of the individual human soul after death? It is true that science, as such, has no direct answer to give to this question. But it can point to facts which must be reckoned with by anyone presuming to answer them. And it must insist that the question be answered, if at all, in the light of such facts as we have, and not as the mere expression of human hopes and aspirations, or of dogmas resting on authority.

The present writer can not presume to speak for that omniscient, if nebulous, entity "Science." He leaves that to the newspaper reporters and headliners. But he can speak for one scientist of mature years and rather varied experience, who has given thought to these matters throughout most of his life. And he can add his personal conviction that the views here expressed will meet with the

assent of many, if not most, of his professional colleagues.

To my mind, any belief in the permanence or ultimate importance of the human individual is rendered untenable by considering the ephemeral nature of individuality in general, organic and inorganic alike. Our physical Universe is made up of temporary aggregations of matter and energy, ranging from atoms to spiral nebulae, from Mendelian genes to highly organized plants and animals. Drops of water, crystals, all "objects" of solid matter, waves on the ocean or in the "ether," rivers, lakes, mountains, continents, worlds and galaxies; chromosomes, nuclei, cells, leaves, bones, organs and entire organisms; social aggregations of insects and of man: all these enjoy a period of individualized existence, be it measured in fractions of a second or in billions of years. All, in time, are disintegrated into simpler constituents, and lose their identity in a common magma, out of which a new stock of individuals is continually being differentiated. Something there is, to be sure, which is permanent, quantitatively speaking at least, be this called matter or energy, electrons, protons, photons, quanta, or any other symbol for the unknown. But the integrated units which are organized out of this are in perpetual flux. In the living organism, indeed, the constituent matter itself changes continually, even during the lifetime of the individual. Such brief permanence as the latter enjoys is one of form rather than of substance, a condition which it shares with a wave, a waterfall or the flame of a candle.

In any realistic description of nature, nothing seems more fundamental than the situation here portrayed. It was recognized by some of the Greek philosophers, notably perhaps by Heraclitus and Democritus, and was picturesquely set forth by the authors of the Indian Upanishads. "As from a blazing fire sparks, being like unto fire, fly forth a thousandfold, thus are various beings brought forth from the Imperishable, my friend, and return thither also." (*Mundaka Upanishad*, translated by F. Max Müller). Intellectually considered, this seems to me to rise immeasurably higher than the crude anthropomorphism of Hebrew and Christian theology.

Why should the human individual be an exception to this seemingly universal flux? Save for the powerful emotional bias, largely a post-mortem projection of the biological instinct of self-preservation, it is hardly likely that anyone would seriously make such a suggestion. To the biologist—at least to the biologist who is self-consistent—the notion would seem to be quite untenable. What we know of man's racial history and of the psychology of his nearer animal relatives leaves no room for the supposition that the individuality of man is a thing quite unique in the world. Any "Next World," whether it be the conventional Heaven and Hell or some mysterious existence free from the limitations of time and space, must be shared with the higher animals, if not with the entire fauna and flora of our planet. Such a possibility as this last has, of course, been frequently contemplated and almost as frequently rejected. Those who have attempted any scientific argument on the subject have sometimes made reference, by way of analogy, to the existence of "critical points" in perfectly continuous series—points like the melting-point of ice or the boiling-point of water. It has been assumed that the capacity for immortality suddenly appeared in a natural way at a definite level in evolutionary history. Is it any mere chance that this level is commonly identified with the emergence of our own species, *Homo sapiens?* An intelligent ape would doubtless set it slightly earlier in evolutionary history. The intellectual snobs among us, on the other hand, would probably exclude the denizens of Main Street—anyone, in fact, with an I.Q. lower than their own.

In addition to these more general considerations, it must be insisted that biological and psychological evidence, at the present time, seems quite inconsistent with the survival of the individual mind after death. The chief of these lines of argument have been presented many times. Since, however, they are persistently overlooked by those who adopt an affirmative standpoint on the question, they may be enumerated briefly.

Individual development, as well as the evolution of animal life, point unmistakably to the dependence of mental capacity upon

the complexity of the central nervous system. The physiology and pathology of the human brain tell the same story in greater detail. Certain mental functions are known to have a definitely localized basis. Definite injuries have definite, sometimes predictable results. One's entire complex of reactions to the outer world, including some of those which we regard as fundamental to "character," are profoundly influenced by the secretions of one's endocrine glands, or even by various drugs which may be introduced into the circulation. And here once more we may allude to the dwindling mental life of senescence.

In many well-authenticated cases the original, single personality has given place to two or more personalities, differing considerably in mental and moral traits, which control the thought and actions alternately, or even to a certain extent simultaneously. In some cases, such divided personalities have been fused together again under appropriate treatment.

There are cases in which traits that are universally recognized as "moral" ones—attributes par excellence of the "soul"—may be very considerably altered as a result of medical or surgical treatment. Which shall we regard as the "real" soul of the individual, the original, "natural" one or the therapeutically improved one? And which of the stages of our life-cycle is to be immortalized—childhood, the "prime" of life, or sensecence? And if the second, as of course you would prefer, what right do you have to make such an arbitrary choice? These and a host of other questions, as unanswerable as they are unavoidable, must be faced by anyone who essays to defend the doctrine of human immortality.

There is one line of argument to which passing notice must be given. This is the evidence derived from so-called "psychic phenomena"—the revelations from the spiritualistic "mediums." That the vast majority of these depend upon fraud, credulity, and self-deception can hardly be doubted. That there is a certain residue of fact which it is impossible to explain according to any scientific principles now known I, for one, am ready to concede as probable. That any of these facts prove or even render likely the personal survival of the individuals concerned I regard as highly improbable. This for two reasons: first, the fragmentary and downright absurd nature of the "revelations," purporting to come from those who have passed into the Great Unknown; second, the existence of a much less incredible explanation, that of telepathy or clairvoyance on the part of the entranced medium. This hypothesis—however vague and unscientific it may seem at present—is supported by some further evidence, and is less glaringly in conflict with the totality of known facts.

Such a dismissal of the arguments from spiritism is doubtless somewhat dogmatic. It may, of course, prove in time to be unwarranted. If so, our whole discussion of personal immortality would have to be rewritten, though this possibility seems at the moment to be extremely remote. In any case, it must be recognized that crucial evidence as between these two rival interpretations of the phenomena will be extremely difficult, if not absolutely impossible to obtain. I mean such evidence as is ordinarily demanded in scientific procedure.

There seems to be little doubt that the craving for personal survival is almost universal among civilized mankind. The fear of death is only partly a fear of the probably painful final struggle. What we dread primarily is extinction, and this is doubtless true even of vast numbers of persons who *think* that they believe in immortality. They think so only until they face death in reality—either their own death or that of some loved one. Then it is only too plain that their confidence has left them, else why their terror or their grief? One of the most curious and pathetic facts of human existence is the tenacity with which most persons cling to life, however tragic that life may have been at all times, and however painful its last stages. The patient "struggles" or "fights," we say, for life, and that, in many cases, is no exaggeration.

How shall we explain this tenacity with which we cling to an existence which may have contained a heavy preponderance of suffering? From the evolutionary standpoint, it is easy to understand our powerfully negative attitude toward death, as an

"adaptation" which we owe to the operation of natural selection. Within the limits of its applicability, such an "explanation" is doubtless altogether correct. As William James says: "An animal that should take pleasure in a feeling of suffocation would, if that pleasure were efficacious enough to make him immerse his head in water, enjoy a longevity of four or five minutes." (*Principles of Psychology,* vol. 1, p. 143).

But this instance shows in a striking way the incompleteness of the kind of explanation which natural selection is able to furnish us for our mental life. To say that in the struggle for existence all those individuals were eliminated which failed to "cling to life" with sufficient tenacity is doubtless strictly true, but to say this is hardly to render a satisfying account of the mental attitudes concerned, *from the inside.* It is probable that most persons have a more or less profound horror of extinction, and it is likely that most persons in advanced life find existence tolerable only in so far as they exclude that prospect from their thoughts. Suicide, far from being "cowardly," as is so often pretended, is for most persons an act of supreme courage. Is it not really cowardice which has prevented most lives from being ended prematurely at moments when conditions seemed intolerable?

Why should there be anything so repellent in the idea of going to sleep peacefully and failing to awake? One reason seems to be that we are unable to picture to ourselves a state of nonexistence and do not really have in mind a passage into total oblivion, but something far more positive than this. Do we not in reality picture to ourselves a bleak, lonely, shadowy sort of existence, deprived of everything that we have valued or enjoyed? As John Burroughs wrote: "We look upon death as an evil because we look upon it from the happy fields of life, and see ourselves as alive in our graves and lamenting that we are shut off from all the light and love and movement of the world. Does our prenatal state seem an evil?" (*Accepting the Universe*). And very similar words were written by Lucretius nearly two thousand years earlier:

> Therefore when you see a man bemoaning his hard case, that after death he shall either rot with his body laid in the grave or be devoured by flames or the jaws of wild beasts . . . he does not methinks really grant the conclusion which he professes to grant . . . nor does he take and force himself root and branch out of life, but all unconsciously imagines something of self to survive. . . . Hence he makes much moan that he has been born mortal, and sees not that after real death there will be no other self to remain in life and lament to self that his own self has met death. . . . (*On the Nature of Things.* Munro's translation, London, 1913, p. 113.)

When we can exorcise this devil from our minds, death and old age will have lost one of its chief terrors. Will this release ever be possible except by displacing that old illusion with some new one? To a vast number of us, the consolations of religion in this field have totally lost their effectiveness. Whatever spiritual reorganization may be in store for mankind will probably be in the direction of humanism—a stimulation of our race-consciousness and a repression of our individualistic self-seeking. We can hardly doubt that a person thoroughly imbued with this spirit would face his own annihilation with greater complacency. A belief in the continuance and progress of his race would be a potent source of consolation. It would be a matter of interest to know whether the fanatical zeal for social betterment which is said to pervade nearly all classes in present-day Russia has not affected the attitude of the individual toward death. Be this as it may, any such spiritual reorganization, for mankind at large, lies too far in the future to benefit those of us who will soon face the issue.

However absurd it may be to think of all these innumerable human personalities, past and present, as surviving as such after death, there is one sort of immortality which seems to me altogether credible. Physics and chemistry have long insisted upon the conservation both of energy and of matter. While these assertions do not mean exactly the same thing now which they did thirty years ago, there still seems to be a unanimous belief in the persistence and indestructibility of certain fundamental elements which underly physical phenomena. This has prompted many to suggest a probable "conservation of spirit;" and to those who

recognize the thoroughgoing interdependence of mind and matter, the suggestion has much in its favor. Regarding the interminable philosophical puzzle of "the relation between mind and matter" I have nothing to say. But one thing seems to me certain. The individual "mind" is no more likely to be permanent than the individual material "object." If there are unitary, indestructible mental elements, they are subject to the same vicissitudes of aggregation and dispersion as are the elements of the physical world. Indeed, the underlying elements in the two spheres may be identical.

There would seem to be little basis here for the kind of "immortality" which the human individual craves. The cosmic deck is continually being shuffled, and new hands continually dealt out. But this "deck" comprises billions of billions of cards, with possibilities of combination which could not be represented in figures, even though our figures were made to fill all the printed pages of all the books that ever existed. And supposing the same combination of mental elements did chance to be repeated, should we have the same "person?" And if so, what shall we say of the vastly greater proportion of more or less close approximations to the original pattern which would come to pass according to the principles of probability?

This question of personality is incredibly puzzling. Should we value a future life between which and our present life there was a complete gap in our memory? And would it be the same "person" who survived? If such an imagined situation seems altogether unsatisfactory to our present cravings, how much more so is the notion of a universal shuffling of the mental elements?

At the time of recording the foregoing train of thought some years ago, I was under no illusion regarding its originality. I was, however, somewhat amazed to learn a few months later how closely I had paralleled the thoughts of Lucretius, at that time quite unknown to me.

> And if time should gather up our matter after our death and put it once more into the position in which it now is, and the light of life be given to us again, this result even would concern us not at all, when the chain of our self-consciousness has once been snapped asunder. So now we give ourselves no concern about any self which we have been before, nor do we feel any distress on the score of that self. For when you look back on the whole past course of immeasurable time and think how manifold are the shapes which the motions of matter take, you may easily credit this too, that these very same seeds of which we now are formed, have often before been placed in the same order in which they now are; and yet we can not recover this in memory: a break in our existence has been interposed, and all the motions have wandered to and fro far astray from the sensations they produced. (Munro's translation, p. 112).

Another very ancient notion, and one which also seems to have much plausibility, is the idea of a cosmic mind, possessed of a perfect memory. From this the individual soul has proceeded; into it it will return. "As the flowing rivers disappear in the sea, losing their name and their form, thus a wise man, freed from name and form, goes to the divine Person, who is greater than the great." (*Mundaka Upanishad,* Max Müller's translation). The insistence of the wishful thinker that all his efforts and experiences and sufferings shall not have been in vain would realize its fulfilment in such a picture. But in that picture would *he* be there at all—except as part of a picture? By wishing hard enough, our wishful thinker might be able to answer the question in the affirmative. But I cannot.

THE SPIRIT OF TRUTH

By DAVID MOFFAT MYERS

CONSULTING ENGINEER, NEW YORK, N. Y.

It is expedient for you that I go away: for if I go not away, the *Comforter* will not come unto you; but if I depart, I will send him unto you (John 16: 7). Howbeit when He, the *Spirit of Truth,* is come, He will guide you unto all truth: for He shall not speak of himself; but whatsoever He shall hear, that shall he speak: and He will shew you things to come (John 16: 13). And that He may abide with you forever; even the *Spirit of Truth* . . . (John 14: 16). But the Comforter, which is the *Holy Ghost,* whom the Father will send in my name, He shall teach you all things, and bring all things to your remembrance, whatsoever I have said unto you (John 14: 26).

Here we have Christ's own clear and concise definition of the Holy Ghost as the Spirit of Truth, which is also the Comforter. All three terms are used synonymously.

We have been taught to think of the Creator in terms of the Holy Trinity, God the Father, God the Son, God the Holy Ghost. Concerning God the Father and God the Son, the Church has preached and taught throughout the centuries. Its most erudite scholars and theologians have instructed our minds to conceive, each one for himself, no doubt, but nevertheless to conceive, some idea of God the Father and a much clearer idea of God the Son.

But the Church has neglected to give us any commensurate teaching regarding the Holy Ghost. Yet the fact remains that on the eve of his departure Christ instructed his disciples that in his absence he would pray the Father who would send the Holy Ghost, the Spirit of Truth, and "he shall teach you all things."

The Holy Ghost is the Spirit of Truth. Upon this Spirit of Truth must we depend according to Christ's own words for "being taught all things." How simple this instruction, how direct! Many fine Christians wish they were able to ask Christ for his answer to some questions, to solve some one of life's baffling problems. He is answering us now. He tells us to open our heart and mind to the Spirit of Truth.

The instruction is simple and direct, yes. But the solving of the answer may require much of honest thought and time and effort in many an obtuse situation. But only through exercise of the Spirit of Truth can Christ's answer to our problems be obtained.

If we are satisfied with an answer based on prejudice or bias or with an answer influenced by gain or aggrandizement then the Spirit of Truth, the Holy Ghost, dwells not in us, and the answer we accept will not be Christ's answer.

Every false philosophy is a sin against the Holy Ghost, the Spirit of Truth. The war which today curses the world with the fires of hell is chiefly the result of wrong thinking, of false gods arising from failure of peoples and governments to be guided by the Spirit of Truth.

Thought and time and effort are essential to the searching out of Truth. The scientists of this world are notable disciples of the Spirit of Truth. They enthusiastically devote their entire lives and energies to the searching out of some small bit of truth. But as long as it is a part of Truth they are indeed rewarded. For every minute finite particle of absolute truth must by definition comprise a unit in complete harmony and agreement with the infinite truth of God himself and the universe of his creation.

The element helium, so intimately known and so usefully employed in our little familiar world, is by spectrum analysis found to exist in the stars millions of light years distant from our small planet. This and numerous other similar concrete cases exhibit the awe-inspiring principle of the universality of truth.

The mechanical and electrical engineers who design a power plant, the civil engineer who designs a bridge, and the architect who designs a great building are guided by years of study and training in the knowledge and application of the fundamental natural laws involved in each special field. The efficiency and strength and utility of the accomplishment in each and every case depend upon

the designer's thorough understanding of and complete adherence to truth and to truth alone.

Why then in designing our very lives should the Spirit of Truth be less regarded, less studied, or less emphasized?

Complete love of God and love of Christ cannot exist without love of the Holy Ghost. For love without the Holy Ghost is to love only a part of God, only a part of Christ. Therefore unless the Spirit of Truth is loved and somewhat understood, the Kingdom of God on earth will be virtually impossible of achievement.

Let us illustrate. Many of our social reformers are good and sincere people. Many of them are not so good. The good ones are actuated solely by a desire to benefit humanity, their every impulse being motivated or augmented by their love of God. Each of them usually has a special theory for improving society or some section thereof. This tends to provide them with a single track mind. They are unable or unwilling to see the viewpoints of other people. Facts, statistics, history, science, even the moral principles involved bounce off their minds like water from a duck's back. Their minds are closed to any truth which might show the falsity of their pet ideas while at the same time their souls' desire for the improvement of some class of humanity is most sincere.

For example many high-minded and unselfish reformers are devoted to the cause of Labor. They go all out for Labor's program, which denies to the individual his free-borne and God-given right to sell his services to any employer willing to hire him. Organized-labor forces such a man by fear or violence to pay tribute to the Union in order to get a job. The Union commonly and notably limits the amount of work a member may perform in a day and the day's work is limited to that of the poorest worker. Regimentation replaces individualism, and the incentive for a better quality or quantity of work is frequently killed. The enthusiastic labor-reformer is prone to argue that the exactment of a fee for the right to work is no worse than some practices in which some employers have indulged. Two wrongs, however, do not make a right, and it would seem that the reformer goes to some trouble in dodging the Spirit of Truth. This is most poignantly striking since a reformer of this class would be the first to claim his loyal adherence to the principle of individual liberty and his utter abhorrence of regimentation in any form.

On the other side of the picture there is a class of people who have no real knowledge of Labor's problems, its real suffering in many situations, and its history of evolution from wide-spread injustice, much of which still remains to be corrected. Yet the same people do not hesitate to rail at Labor and its struggle for better things. This is because they have never tried to find out how the other half lives, or to determine the truth of the problem without prejudice. Here again the Spirit of Truth is the great necessity.

The deplorable condition of our home front today is attributable chiefly to false theories. Hordes of well-intentioned people are aiding in the battering down of sound principles, aiding others in high places who are using the cloak of humanity for purely selfish and political objectives. That part of God known as the Spirit of Truth is not here, and chaos threatens. The love of only a part of God by men results only in confusion and evil.

The Church itself has been a notorious sinner against the Holy Ghost, substantially retarding the progress of man toward Truth and God. The Church insisted the sun revolved about the earth and persecuted Galileo for teaching otherwise. She regarded the growth of science and general knowledge as her deadly enemies. She countenanced the burning of witches, substituting superstition for the Spirit of Truth. Right here in this enlightened country some backward religious sects still regard the conception of evolution as antagonistic to the word of God. They are afraid that Truth may be at variance with their religion. Fear of this type prevents the clear independent thinking of many good church people. They fail to comprehend Christ's own clear definition of the Holy Ghost, the Spirit of Truth whom He designated as our guide and who would teach us all things.

We have Christ's direction in his absence to depend on the Spirit of Truth for our

guidance in all things. Never let us forget these last instructions of our Lord and Master. Truth and true Christian religion cannot fail to agree, for the Spirit of Truth is Christ's own teaching. When, as may often occur, they do not appear to agree in some particular we may be certain that our conception of one or the other is at fault.

Science in its broadest definition from the Latin *scientia* means knowledge. In the more specific sense it designates formulated or systematized knowledge, as applied largely to the discovery and application of natural laws.

It is interesting to observe that since Christ designated the Spirit of Truth for our guidance, He must have considered the human race capable of discovering sufficient truth to enable it to find its way and plot its course of development.

The emphasis should be on the *spirit;* that is, the willingness and the will to truth. Only the mind open to truth can possess the spirit. It has ever been the prejudiced closed mind which has retarded the progress of the human race toward truth and light and salvation.

Politicians in extremely numerous instances are notorious sinners against the Holy Ghost. It is so easy to prey upon the ignorances and prejudices of the classes and the masses. It is so much easier for them to secure votes for themselves by this means than to teach the truth without fear or favor. Such men are ungodly. They wickedly and selfishly retard the progress of civilization and the coming of the Kingdom. It is the duty of the Church and of all Christians to unseat these false leaders of the people.

"Thou shalt love the Lord thy God with all thy *heart,* and with all thy *soul* and with all thy *mind.* This is the first and great Commandment." This suggests that one may love Christ with the heart, God with the soul, and the Spirit of Truth or the Holy Ghost with the mind; and thus may one truly love the Creator of us all, the glorious Trinity.

A man may give his heart to Christ and his soul to God. But if he opens not his mind to the Spirit of Truth then indeed he fails to love completely, and his life can neither be fully effective nor fully godly. It should be remembered that Christ himself both lived and died for the Spirit of Truth.

The very marvelous materialistic developments of our century have been due to the scientific approach to the problems in hand. All have been achieved through the open minds of scientists and engineers, the undeviating search for truth in the field of natural law and its varied applications.

When churchmen, philosophers, statesmen, reformers, and humanity in general arrive at the point in their evolutionary development where they can and will undertake the study of the problems in their special fields with that same assiduous longing for truth which marks so distinctively the scientist and the engineer; then indeed will the Holy Ghost, the Spirit of Truth, be worshipped and glorified. Then indeed with the understanding of Christ's last instructions shall we approach the Kingdom of God upon earth.

"And I will pray the Father, and He shall give you another Comforter that he may abide with you forever; even the Spirit of Truth."

SCIENCE ON THE MARCH

PHYSIOLOGICAL RESEARCH IN WARTIME CHINA*

A REPORT has recently been received giving details on the work and organization of the Tsing Hua University Physiological Laboratory. The laboratory was established in the fall of 1938 shortly after the outbreak of hostilities between China and Japan and the removal of the university from the Peiping campus to its wartime site at Kunming in southwest China. It is one of the three divisions of the Tsing Hua University Institute of Agricultural Research and is devoted to the investigation of fundamental problems in general physiology and their application to Chinese agriculture.

The laboratory had its start in a rented classroom in one of the local colleges, with a very limited amount of equipment, secured mainly from one of the drug supply houses in Hong Kong before the outbreak of the war in the Pacific. There were difficulties in obtaining a suitable site, and during these six years it has moved not less than four times. It is now located in its own buildings in the suburbs of Kunming; its previous quarters were severely bombed by the Japanese in the fall of 1940. During that bombing, a direct hit was scored on the laboratory storeroom the day after the major part of the equipment had been moved to the new suburban site.

During the six uncertain years of its existence, the Physiological Laboratory has been fortunate in being able to carry forward a comparatively uninterrupted research program except for the inconveniences caused by the almost daily bombings of the city by the Japanese in the earlier years of the Sino-Japanese war. In addition to the laboratory's permanent staff there have been no less than two score of younger, experienced research workers who have used the facilities of the laboratory for varying lengths of time. The regular seminars of the laboratory, formal and informal, have continued without interruption and have been the source of much inspiration and stimulus. An unusual esprit-de-corps has been developed, due largely to the energy and personality of the director, Dr. Pei-sung Tang.

The permanent senior staff includes: Dr. Pei-sung Tang, Dr. Hung-chang Yin, Dr. Cherng-how Lou, and Dr. Tung Shen. All these men have studied abroad and received their graduate degrees in the United States.

THE topics of research in the field of pure physiology include:

Electrophysiology. A study is being made of the electric potentials developed in plants, especially in the sensitive plant, during excitation; in addition to visual and microscopic observations, the phenomena are being studied with the use of a cathode-ray oscilloscope. Electrophoresis measurements are being made on Chinese lacquer. Electrolytic oxidation and reduction of organic substances are being studied, including the oxidation of glucose to gluconic acid and calcium gluconate, and its conversion, through reduction to sorbitol, eventually to vitamin C.

Chemical Investigations in Plant and Animal Metabolism. This work includes a detailed study of the protein metabolism of the silkworm, especially the mechanism of silk formation in the silkworm as revealed by chemical analysis and X-ray diffraction patterns. It was for this work, carried out in collaboration with the members of the Metals Research Institute of Tsing Hua University, that the members of the laboratory received the Ting Prize award of the Academia Sinica in 1942. At present there is under investigation the mechanism of fat and protein formation in the peanut; this study is in its third year. The effect of potassium chlorate on plants is also being investigated to obtain information on the mechanism involved in the use of this substance in weed control.

Plant Physiology. The physiological applications of the auxins and vernalization are being investigated. In connection with this work an invitation has been extended to Dr. Yin to visit the University of Cam-

* A résumé of activities of the Physiological Laboratory of Tsing Hua University, 1938–1944.

bridge for the coming year. At present, a detailed study is being made of the application of auxin to the rooting of the cuttings of the tung oil tree, and the use of cold temperature, auxins, and high frequency radio waves in the vernalization of wheat.

Cellular Physiology. A series of observations has been completed on the physiology of autotetraploid barley plants, which were obtained by treatment with colchicine and which have been maintained to the fifth generation. Cellular respiration studies, a field in which the laboratory has been particularly active, are being continued with *Monascus.* The problem of the relationship between oxidation and cell development is being carried forward using the eggs of the frog as experimental material.

In the applied field, efforts have been made to contribute to the more urgent problems of the war in agriculture, industry, and medicine. Work has been carried on in the following:

Nutrition. During the earlier years of the war, members of the laboratory staff served with the Medical Relief Corps of the Chinese Red Cross and conducted studies of the Chinese Army diets. The work is being continued. Members of the laboratory have also participated in the National Nutrition Conferences held in Chungking in 1941 and 1944. Laboratory studies have centered about studies on soybeans, and especially soybean protein. It is hoped to develop more extensively the use of local herbs and foods as sources of the vitamins.

Penicillium notatum. Penicillium spores have been mailed to the laboratory from investigators in the United States, and good progress has already been made in the production of penicillin. At present surgical dressings containing crude penicillin are being distributed to local hospitals in the city for clinical use. At the same time similar types of microorganisms from local sources are being studied.

Throughout the war it has been practically impossible for the laboratory to secure supplies from abroad. This has been the most serious handicap. While this difficulty has stimulated in some instances the invention of simplified techniques to replace the use of elaborate equipment, in other instances it has severely limited the scope and development of the research problems in hand.

In similar manner, soon after the establishment of the laboratory, the supply of scientific books and journals from abroad was cut off. The British Scientific Mission and the Sino-American Cultural Office have both rendered assistance in the supply of microfilm; most of the important English-language scientific journals and important books have been microfilmed. Unfortunately the use of the microfilm projectors is dependent upon an adequate electric current supply and this has not always been available.

Papers from the Physiological Laboratory have from time to time appeared in British and American journals. To meet the more urgent needs for a publication medium, however, the laboratory has itself sponsored a wartime publication called the *Biochemical Bulletin.* This is issued at present in a mimeographed edition of four or five pages monthly and serves the research fields of physiology and biochemistry in Free China.

The report of the activities of the Physiological Laboratory includes a list of sixty papers published during the last six years.—William H. Adolph, School of Nutrition, Cornell University.

THE MAIN PROBLEMS OF PHYSICS IN THE U.S.S.R.*

The main problems of physics are still those which are connected with the war. It is difficult to enumerate—and for certain reasons impossible to name—the defense jobs of the Soviet physicists. One may judge of the importance of this work by the fact that for their utilization many new organizations have been set up under the defense Comissariats. Institutes of Physics have been entirely converted to defense work. Many physicists transferred their work for the duration of the war from laboratories to plants, ships, etc. The most delicate methods of the physics of the atomic nucleus are now being used for purposes of solving concrete military and industrial problems.

* Translated and condensed by the American Russian Institute from *Izvestia,* November 25, 1944.

In addition to defense work, there are a great many scientific problems on which Soviet physicists are working. Within the next few years we intend to concentrate all our efforts on the basic problems of science brought forth by new developments, the solution of which will open up new perspectives in the study and utilization of nature.

The research work of the last decade proved the existence of two types of cosmic rays: light electrons and mesotrons two-hundred times heavier. Academicians A. I. Alikhanov and A. I. Alikhanian discovered in the composition of cosmic rays a new, third component. The study of the physical nature of these particles will comprise the most important problem of the expedition of 1945.

Long before the war, the French physicist Auger had already discovered at great heights showers of particles, covering an area of dozens of square meters. The total energy of all the particles of such a shower is so great that we do not know any one source in nature which could create it. The largest known concentration of energy is the heavy atom of uranium, where there is a concentration up to 0.3 ergs. Auger observed showers of thousands of ergs, and A. I. Alikhanov and A. I. Alikhanian, during this year's expedition, observed showers of hundreds of thousands of ergs. We are faced with a riddle, the solution of which may change our views on the structure of the universe.

Academician O. U. Schmidt brought forward a new theory of the origin of the earth and other planets through the accumulation of universal dust, which adhered to the planets while they were revolving around the sun, and through the participation of the entire solar system in the revolving of our galaxy. He has already provided a number of convincing proofs of his contentions. Their mathematical development is our next problem and Soviet scientists are going to work on it now.

Questions pertaining to the history of the earth are connected with the properties of the earth's crust, with the origin of earthquakes, with the composition of the upper strata and the finding of valuable minerals in them (especially in the Soviet Union). All means at the disposal of physics will be used for the solution of this problem: seismic waves, created by explosions; radio waves; and electric, magnetic, and thermal prospecting. The expeditions which were conducted by the Academy of Sciences in the Second Baku, in Bukhara, and especially, the expedition this year on the Apsheron Peninsula and on the Caspian Sea, assure the success of the work scheduled for next year. Next year we expect to be able to publish conclusions on the research work conducted during the past few years on the process of the movement of air-layers and their intermingling in the atmosphere; on the nature of fogs and mists.

The work connected with the complete solar eclipse of 1945 is of special importance. Many properties of the solar atmosphere, concealed from us by the bright light of the sun, can be studied only during an eclipse. Sixteen scientific institutions will participate in the study of the eclipse of 1945.

At all times, the most important problem of physicists and chemists has consisted of the study of the properties of substances, for the purpose of mastering them and developing technological materials with the necessary properties. Soviet physicists succeeded in solving a number of such problems. They are responsible for establishing the basic laws and physical theories which explain the mechanical and electrical properties of crystals and glasses, polymers (rubber, plastics), electric insulators and semiconductors.

During the past few years Soviet physicists developed new, valuable, and easily obtainable materials: frost-resisting rubber, heat-resisting "escapon" (a product of synthetic rubber) which is at the same time a wonderful insulator in radio and many other fields. Somewhat earlier Soviet physicists introduced polysterole and titanium dioxide, which later became widespread in the electrotechnical field. All of these products are being manufactured successfully by our factories and aid in solving many difficult technological problems.

The new semiconducting materials have—in the hands of Soviet physicists—increased tenfold the current in solid rectifiers of alternating current, in solid photoelements and

thermoelements. The work, which has been interrupted by the war, will be resumed next year. In addition to the solution of practical problems, our theoretical understanding of the properties of semiconductors has become richer and more profound. And this opens up new, still unused means for improving the quality of products and their properties.

The phenomenon of luminescence by way of irradiation with an electron beam, as well as by using some chemical and physiological agents, has been known for sometime. But only of late have they acquired an extensive practical significance in war work, in the technique of illumination. The fluorescent lamps, which convert the invisible ultraviolet rays into visible light, made it possible to use the light of the gas discharge instead of the heated steel wire. This makes for a great economy of electric energy and makes it possible to create light of any tint. Soviet scientists succeeded in creating conditions under which one may accumulate in phosphors large deposits of lighting energy and with a good coefficient of efficiency utilize this accumulated supply. The study of such phenomena, projected for next year, will clarify their mechanism and bring us to still more perfect kinds of phosphors.

The utilization of spectral analysis in our metal industry, in the fuel and chemical industries is being widely developed.

The most important discovery of Soviet physicists during the past few years is the superfluidity of liquid helium when it nears absolute zero. Academician P. L. Kapitsa found and proved that under such conditions the viscosity of helium, even if it exists, is billions of times lower than in all other known liquids. That is where all the amazing phenomena come from: for instance, helium in a tube at one and the same time flows in two opposite directions; sounds of two different velocities spread simultaneously, etc.

Here, near absolute zero, new quantum properties are opening up—properties which have created a revolution in physics of the twentieth century. Further research on superfluidity has great fundamental significance.

Thirty years have already gone by since the discovery of another analogous phenomenon, which is still not understood—the phenomenon of the superconductivity of certain metals near absolute zero. And here, too, Soviet physicists theoretically predicted and proved in practice that there is no intermediate state: in the transition from superconductivity to the usual properties the metal breaks up into a number of layers which alternate from one state to another. This discovery creates a new approach to the phenomenon of superconductivity.

The Hitlerite hordes inflicted much damage on Soviet science. The famous Pulkovo Observatory, which for a century had been considered "the astronomic capital of the world," has been completely destroyed. The Semeiz Observatory was destroyed and burned down, and all its instruments, books, and equipment were stolen by the Germans and carted away in 32 trucks. In the Crimea the only marine hydrophysical station in the world was destroyed; at that station "the voice of the sea"—sound waves several dozen meters long and inaudible to the human ear—were studied. This was a discovery of Academician V. V. Shuleikin. The Germans set fire to the mountain station of the Academy of Sciences at Elbrus, where for many years cosmic rays, the irradiation of the night sky, and the physiology of man at great altitudes have been studied.

All this must not only be restored, but also perfected. Next year reconstruction of the Pulkovo and Semeiz Observatories and of the marine station will begin.

Soviet physicists have set themselves large and difficult problems. We are certain of their successful solution. The enthusiasm for work which embraces our entire country has created among scientists the desire to give all of their strength and knowledge for attaining new heights in science.—Academician A. Joffe.

BOOK REVIEWS

EXPLORING THE INFINITE

What are Cosmic Rays? Pierre Auger. 128 pp. Illus. 1945. $2.00. The University of Chicago Press.

PIERRE AUGER'S book of 120 pages has the great merit of being very short and very well written, both from the standpoint of literary style, in which the French are past masters, and also from the standpoint of clarity and simplicity. His analogies and illustrations —great assets in popular exposition—are often very illuminating.

The merits of brevity and clarity necessarily carry with them the demerits of incompleteness and one-sidedness. One inevitably sees the landscape of which he is a part more clearly than he sees and correctly appraises what is going on, and what has often earlier gone on, in remoter lands outside the range of his vision. Let me illustrate this as follows:

The American student who in the late 19th Century went to Europe for his advanced training was able, merely because he was an American, to get, and actually did get in my case, for I was one of them, a more correct appraisal of the historical development of physics than did the European student, who lived close to where much of that development had taken place.

For from German textbooks and teachers I found that I had unconsciously acquired the habit of ignoring, in my thinking, French and English accomplishments, a habit quickly corrected by later study in Paris where I again made the same discovery but in the following modified form, viz., "that almost all the great advances had been of *French* origin,"—a bit of myopia afterward corrected by a few months of study in England, itself not immune from the nationalistic disease.

But we Americans, who up to say 1900 had made very few of the fundamental advances ourselves, were fairly immune from this kind of bias and were therefore in excellent position to make objective appraisals of the real relative contributions of the different European countries. We were not blinded by any nationalistic pride in our own accomplishments, having had very few to be proud of, and we knew it. It is possible that *now,* when we are increasing our own productivity at a rapid rate, we are in danger of losing our objectivity and beginning to overrate our own importance in science.

In any case, however, it will take a long time to even up with Europe the score created by the 150 years of amazing ignorance and nonrecognition on the part of European writers of the significance of the foundations of electricity laid by Benjamin Franklin, whose "single-fluid electron theory" of 1750 is now universally recognized as that to which the whole scientific world returned about 1900, in spite of a century and a half of utter neglect (due no doubt primarily to distance) of one of the world's greatest scientific contributions.

Professor Auger, however, is a European who tries to be fair to other cosmic-ray workers, and in view of the scope of his book he succeeds pretty well, though there are enough oversights to justify the advice to those who read his book to do some supplementary reading in say two or three other books on cosmic rays before they feel that they have very much of a grasp of this field.

For example, Dr. Auger's treatment of the discovery of the mesotron seems to me quite a misplacement of emphasis. For that was a discovery made wholly by two experimental physicists, Anderson and Neddermeyer, without any assistance whatever from Yukawa or from any theoretical reasoning other than that represented by the then generally recognized Bethe-Heitler theory of the origin of showers. The very scant consideration given to Anderson and Neddermeyer's work leaves the reader with a somewhat distorted historical perspective, especially in view of the extraordinary stress Dr. Auger lays on the importance of this discovery.

These two discoverers actually worked for several years with consummate patience and skill, beginning in 1934, to avoid the necessity of postulating a new particle to account for "their observed abnormal penetrating power of tracks otherwise resembling electron tracks," a penetrating power which theorists like Oppenheimer and Bethe and experimentalists like Blackett and myself

had suggested must be due to a change at high energies in the properties of the electron itself, since we were all fully aware of the fact that the ordinary properties of electrons did not permit of the observed high penetrating power.

Only when Anderson and Neddermeyer found it proved on the one hand that no such change at high energy of the electron's properties does actually take place, even when cosmic-ray electrons up to 15 billion electron-volts were under test, and on the other hand that nonshower particles (definitely not protons) of the same magnetic curvature as shower electrons had in fact such high penetrating power that they could not be electrons, did they make their announcement, published in November 1936 and more at length in early April 1937, of the discovery of the mesotron. (See *Phys. Rev.*, Vol. 53, p. 219, footnote 7, for history of this discovery.) Even today one of the world's most competent and famous theoretical physicists recently wrote me that there is as yet no good reason for supposing that this new particle, the mesotron, whose properties are all merely *experimental* findings, is to be identified with the object of Yukawa's theoretical speculations. He suggests that this last be given some other name. In any case, I cannot find that any suggestion of any sort in this field was made by Yukawa before 1935, one year after Anderson and Neddermeyer had in one of their publications commented upon the difficulty of interpreting the high penetrating power observed in some of their tracks as either electron or proton tracks.

It is true, as Dr. Auger says, that Leprince-Ringuet did a fine experimental job when (in 1941) he got a picture from which he found the mass of the mesotron to be 240 times the mass of the electron. It would not have been a misstatement if Dr. Auger had added that this checked nicely with Anderson and Neddermeyer's determination of the value of this mass made some three years earlier (in 1938) and with at least equal reliability and precision.

I shall close with the expression of the hope that there will be many readers of Dr. Auger's fine book on "What are Cosmic Rays," and that before concluding their quest for the answer they at least read the following recent articles in the *Physical Review*, not referred to by him, which some people think give the best evidence yet found for a positive answer to the query, "What are Cosmic Rays?"

Robert A. Millikan, H. Victor Neher, and William H. Pickering, A Hypothesis as to the Origin of Cosmic Rays and Its Experimental Testing in India and Elsewhere, *Phys. Rev.* 61, 397–407, 1942.

————, Further Tests of the Atom-Annihilation Hypothesis as to the Origin of the Cosmic Rays, *Phys. Rev.* 63, 234–245, 1943.

————, Further Studies on the Origin of Cosmic Rays, *Phys. Rev.* 66, 295–302, 1944.

H. V. Neher and W. H. Pickering, Results of a High Altitude Cosmic-Ray survey near the Magnetic Equator, *Phys. Rev.* 61, 407–413, 1942.

ROBERT A. MILLIKAN
CALIFORNIA INSTITUTE OF TECHNOLOGY

ELECTRONICS EXPLAINED

An Introduction to Electronics. Ralph G. Hudson. 97 pp. Illus. 1945. $3.00. The Macmillan Company, New York.

THIS book is an introduction to the science of electronics for the lay reader who has only an elementary knowledge of mathematics and physics. The preface contains a brief explanation of the notation used for expressing very large and very small numbers in powers of ten. Aside from this, no knowledge of mathematics is needed.

The first chapter of the book presents a review of modern theories of the constitution of matter. The Bohr model of the atom is discussed in some detail, with a brief consideration of the newer particles such as positrons, mesotrons and neutrinos. The principle of operation of a cyclotron is explained. The second chapter covers the flow of electricity in gases, liquids, solids and in a vacuum.

Succeeding chapters in the book deal with the essential aspects of radio communication, the reproduction of motion pictures, facsimile, sound and television, and the production of light and radiation. A chapter on industrial types of electronic power devices is included. The final chapter indicates the wide variety of possible applications of electronics. Descriptions are given of the operation of an electron microscope, the radiosonde, the radio compass, and diathermy equipment. A list of conversion factors for changing various quantities from one system of units to another is in an appendix.

The book is adequately illustrated and contains a large number of excellent photographs. It will be of considerable interest to readers who desire to obtain an acquaintance with the working principles of modern electronic devices.

George Sinclair
Communications Laboratory,
The Ohio State University

CALL OF THE WILD

South America Called Them. Victor Wolfgang von Hagen. 311 pp. Illus. 1945. $3.75. Alfred A. Knopf, New York.

The "them" in the title of Mr. von Hagen's book refers to four European naturalists who made explorations in South America between 1735 and 1866. They are, chronologically: Charles-Marie de La Condamine (1701–1774), Frenchman who led the illustrious Académie des Sciences Expedition to the Equator; Alexander von Humboldt (1769–1859), German baron who explored in Ecuador, Colombia, Peru, and the Upper Amazon; Charles Robert Darwin (1809–1882), Englishman who traveled on H. M. S. *Beagle* to Patagonia, Tierra del Fuego, Chile, Peru, and the Galápagos Islands and gathered enough material and ideas to revolutionize the science of zoology; and Robert Spruce (1817–1893), Yorkshireman who traveled in an open canoe over 18,000 miles through the waterways of the Amazon Valley and the Rio Negro, botanizing, filling European herbaria with the results of his prodigious plant-collecting, taking out cinchona plants for India, etc., etc., until he became one of the greatest botanical explorers in history.

It is useful to have the achievements of these titans of science recounted, and Mr. von Hagen, summoning his not inconsiderable abilities as a writer, has done a commendable job of it. He has focused our attention again on South America and on the events of the times that led to the early South American exploration. He does not include exhaustive biographies of these explorer-naturalists but describes in some detail their travels, vicissitudes, failures, and successes and stresses the importance of their discoveries in the total South American scientific picture; they are shown to be the main links in the chain of South American scientific history. Naturally, the story is not devoid of the spirit of adventure and romance, which the passing years sometimes manage to extract from even the most prosaic happenings. Furthermore, Mr. von Hagen writes with an enthusiasm for his subject that makes his book lively and readable.

As to sources, the author has drawn heavily upon the work of others for atmosphere as well as for facts, but it could hardly be otherwise in a book of this kind, which is historical and informative in its approach. Even so, the author has himself traveled in many of the South American countries he writes about and has studied firsthand their biology, ethnology, and geography. His personal experience is thus added to the documentary record to make a well-balanced and authoritative account. Primary sources, as would be expected, are the writings of the naturalists themselves—the journals of La Condamine; the voluminous letters, personal narratives, and voyages of Humboldt; Darwin's *Voyage of the Beagle;* and Spruce's journal and *Notes of a Botanist.* These have been supplemented, especially to fill in the background of the times, by many other works, including pertinent accounts of such modern writers as Stefan Zweig, Paul Russell Cutright, Robert Cushman Murphy, Geoffrey West, and S. E. Morison. All these are freely acknowledged.

The book bears this epigraph from Seneca: "Much remains to be done, much still will remain . . . nor shall any man born after the revolution of a thousand ages be denied the opportunity to contribute something. . . ." This is a truth that has animated scientists of all ages, but it seemed especially true of South America (and indeed of North America too) during the period when La Condamine, Humboldt, Darwin, and Spruce lived and worked. They opened new frontiers of scientific discovery on a virgin continent where much *still* remains to be done. Their story is fascinating and important and timely.

South America Called Them is well illustrated with halftone plates and maps and contains a perhaps adequate but none too thorough or accurate index. It is tastefully bound in blue cloth, over which is wrapped a singularly ugly green jacket. The format,

printing, and paper are above the average for these times, when, paradoxically, there seems to be too little paper for good and worthy books but plenty for colored "comic" books and other forms of unadulterated tripe.

PAUL H. OEHSER
SMITHSONIAN INSTITUTION
WASHINGTON, D. C.

MEDICAL POETRY

Poet Physicians. Mary Lou McDonough. 210 pp. 1945. $5.00. Charles C. Thomas, Springfield, Illinois.

THE attempt to select the best poetry on medical subjects by medical men of all time and from many lands is a considerable undertaking. Mrs. Mary Lou McDonough, the compiler of the present collection, is an artist in her own right. She is the wife of Captain Stephen McDonough of the Surgeon General's Office in the War Department. In her search for poetry she has had access to the finest libraries in this country, both public and private. The result is embedded in a very attractive little volume of 210 pages, very carefully designed and beautifully printed on excellent paper.

Good poetry is very rare in any field of thought and judging by this collection, made on the basis of quality, it is most unusual in the field of medicine. None of the poetry reprinted here descends to the trivial commonly seen in doctors' verse as it appears in medical journals, but on the other hand surprisingly little of it is of high quality. The best selections frequently have little to do with the practice of medicine. It is probable that poet physicians have reserved their best efforts for nonmedical verse. It is difficult to see a reason for this unless poetry is for the doctor an escape from stark reality. A famous Chicago physician told me that his day was so filled with tragedy that he could not enjoy operas filled with artificially concocted misery.

Why do physicians in writing pay so much attention to the morgue, dissecting room cadavers, death, and pestilence? The body is full of adaptive devices and regenerative forces that should be the subject of joyful verse. There is no lack of material for lines of beauty, rhythm, and suggestions of significance. For many years I have seen and deplored the change that comes over medical students as they mature from idealistic, widely interested, and spontaneous youngsters into efficient, narrowed, realistic (?) practitioners. Listen to successful physicians and surgeons at the lecture desk or in society meetings. Few have enriched their vocabularies, except for technical terms, since they left college. Imagination in a crowded medical life often shrivels to small dimensions.

The book has other merits than the 186 poems which it contains. Each of the 108 authors is introduced by an attractively arranged biographical sketch. These and the poems are chronologically arranged. A final index section contains the names, birthplaces, birth and death dates when known, of 401 medical poets. Dr. Merrill Moore of Boston, who contributes five verses to this volume, is said to have written more than 50,000 sonnets. He adds a short afterthought, or memorandum, on medical poets in which he commends Mrs. McDonough for her careful gleaning of "medical poetry," most of which is junk. The poems selected are varied in theme. Few have humor. China and Japan are represented by one each in translation. South and Central America contribute few. The older literature reaches a little into Italy, France, and Germany but most of it is English and most of the recent work is American.

This book is a large and worthy effort. The verse is interesting and is not easily accessible in other books or periodicals. Its publication may unearth some hidden gems from forgotten journals. It may also prod some who have the talent and experience until they contribute to a fine art.

JOHN G. SINCLAIR
UNIVERSITY OF TEXAS MEDICAL BRANCH
GALVESTON, TEXAS

REORIENTING THE LIBERAL ARTS

Education for Responsible Living. Wallace B. Donham. 309 pp. 1944. $3.00. The Harvard University Press, Cambridge, Massachusetts.

THE author of this book has an unusual background of experience in American business education. The Graduate School of Business Administration, with which he has been so potently related, has demonstrated that young men can be educated for greater efficiency and higher standards in business.

Since an acceptable college course is prerequisite to entrance in the Harvard Graduate School, and since young men have come to it from many colleges, it follows that there has been opportunity for conclusions regarding the general foundational education of the students. It is an old assertion, however, that preceding education is faulty when considered in terms of that which is to follow. Graduate and professional schools censure the colleges. Colleges complain about the high schools. Secondary schools talk about poor work of the elementary schools. The homes from which young children enter schools too often have abdicated many or most of their nonbiological relations with their children. And, when graduates of professional schools are employed in the affairs of life, they and the public find that there is always more to be learned and more adjustments to be made. It must always be so. Of course, the preceding schools should do better. They are constantly endeavoring to do so. Specific factual criticism is helpful. But each succeeding school must build on what it gets unless the foundations are too crumbly to serve even when considerable patching is done. The author is sometimes on one, and sometimes on another side of such questions, for in his closing chapter he says, "Of course, all educational ideals are unattainable, and by this test all education fails." By such statements from the author, the reviewer understands that the many stinging, severe, and well-founded criticisms of general education might properly have closed with assurances that much real progress has been made, but that this progress is decidedly inadequate when judged by our constantly improving ideals of what might and should be accomplished.

The discussions dealing with needful attitudes of mind and action are particularly cogent. These recur all through the book almost as an unstated fundamental theme. This fact is important since it indicates a conviction that whatever one's education or training, the most significant feature relates to any dependable attitudes which are guides in thinking and acting. In discussing engineering training and liberal education, it is claimed that a person with disciplined scientific training is likely to have a "narrowness of his point of view." It is also claimed that a person with such training "is disturbed by the confusion of facts as they occur in other aspects of life," and further that such a person sometimes resents the necessity of making conclusions in situations in which facts are unknown and possibly "unknowable." We need to recognize that certainty is relative. In the mathematical and physical sciences it is usually possible to acquire a high percentage of the facts needed for conclusions. In biological sciences, we are sometimes fortunate if we acquire half the needed facts, and in the social sciences, so-called, the variables and unknowns are such that conclusions are sometimes merely guesses regarding probabilities. The author urges that scientific training needs the broadening of liberal arts, partly to emphasize the necessity of judgments in areas in which factual foundations are scant or absent. "Every advance made by science brings a greater need for men capable of keen incisive action with reference to successive novel situations. Every such advance of science presents a more important need, a more obvious opportunity for liberal-arts colleges. . . . It is too bad that, instead of thoughtful analysis leading to affirmative demonstration of useful values, the liberal-arts colleges have so generally taken the easy attitude of disclaiming any intent or desire to be useful—truly a pathetic defeatist attitude for the custodians of the highest values and those most critically needed in the whole range of education and civilization."

The development of the sciences and consequent impact upon religion, behavior, and philosophy has removed at least part of former controls of human character. Some individuals have been "cut adrift" by removal of fear of Hell. And some base their hope of Heaven solely upon integrity of character and conformity to proved principles now observable in practical human life, and let it go at that. The tremendous gains in validity of scientific thinking have not become possessions of some people, though they may have lost the earlier anchorages to human character. Since "We have lost a sense of direction in the midst of overwhelming change," it is stated that in individual guidance and philosophy "many serious ob-

servers think we are drifting toward totalitarianism.'' This would seem to mean that some persons are afraid of individual responsibility and tend toward accepting orders from a superior who will accept full responsibility. In association with this tendency, possibly a part of the cause of it, is the decline in the extent to which family life and coherence are determining factors in human conduct.

The closing chapter (XXI) on ''Is General Education Possible,'' should be ''must'' reading for all college authorities and teachers. Undergraduate teaching is one job. Graduate teaching and specialized subject research is a decidedly different job. Eminent lecturers reciting to their undergraduate students are accomplishing little toward education. Back and forth thought analysis and conclusions based upon specific situations and problems (the case system) will require superior teacher guidance that differs distinctly from capacity and eminence in subject research. Such college teaching must become a worthy and fully recognized career, second to no other, in order that liberal college education may become a reality.

OTIS W. CALDWELL
OFFICE OF THE GENERAL SECRETARY
A. A. A. S.

FEMININE PSYCHOLOGY

The Psychology of Women. Vol. II: Motherhood. Helene Deutsch, M.D. 498 pp. 1945. $5.00. Grune and Stratton, New York.

THE first volume of this work was reviewed in an earlier issue of this journal. The present volume is devoted to women in the relation of motherhood. It is in many a ways a unique work. The contribution, indeed, which Dr. Deutsch makes in this volume towards the better understanding of feminine psychology in our society is, I believe, destined to assume classical rank. Writing out of a background of extended clinical psychoanalytic experience with women of all types both in Europe and in the United States Dr. Deutsch provides the reader with numerous basic case histories drawn from this experience and which, in this volume, are very lucidly examined and illuminatingly discussed. There is a tremendous amount of wisdom in this volume, and very deep and novel insights. All students of human nature will gain much from its reading.

In this volume, as in the first, Dr. Deutsch is concerned with the analysis and delineation of the mental life of the normal woman in our society. The pathologic aspects of woman's mental life will be dealt with in a future volume, and in still another volume the author promises to consider the cultural and social factors which influence the shaping of the feminine psyche. We look forward to these additional volumes with the greatest interest and trust that we shall not have to wait long for their appearance. The present volume cannot be too highly recommended to all students of human nature. If the succeeding volumes maintain the quality of this second volume the success of what will, indeed, be a great work is assured.

M. F. ASHLEY MONTAGU
HAHNEMANN MEDICAL COLLEGE
PHILADELPHIA, PA.

BOOK REVIEWER

(*r.i.p.*)

Beneath this mossy rock in supine rest
Lies one who read the bad, the good, the best
But died, alas, from simple lack of sleep;
For through the night instead of counting sheep,
He, wretched soul, distraught as well as drowsy,
Was hunting ant- and synonyms for ''lousy.''

—PAUL H. OEHSER

COMMENTS AND CRITICISMS

Some Insect Infants

Our bugs, the author tells us, as we ramble thru his lines,
Amount, no less, excluding fleas, to fifty thousand kinds!
Then on their worms he dwells at length, with fiendish glee and skill,
And by this time my lips are pale and I am feeling ill;
But when I learn of the worm's demise, brother, I'm sicker still!
Altho I'm loath—and with regret, put Science on the pan,
When I reflect on the worms I've et, says I—"that nasty man!"

—H. E. Woodcock.

Our Dear Watson

Let me compliment you on a recent article, that by Hedgpeth on Sherlock Holmes' Medusa. I think that your allowing him to have notes and references at the rear is a splendid thing. It makes the "Monthly" a "Scientific Monthly." I found his paper very interesting. I have always hoped that somebody would take Mr. Sherlock up on the question, and our author certainly did a good job.—E. W. Gudger.

Electronics

In the June (1945) Scientific Monthly, p. 459, will be found an excellent and instructive article on the above topic by John Mills. Since I have for the past few years been making a special study of the electrostatic and the electromagnetic fields of the electron, I was greatly interested in this paper on electronics. In general, the descriptive analyses by Mr. Mills of the various phenomena to which electrons lend themselves were entirely correct; there were, however, a few descriptions in the details of which one might wish for more scientifically expressed terminology. For instance at the close of his second paragraph will be found the sentence: "They [the electrons] can also be freed by the impacts of other electrons and from metal bodies by the application of heat." There is no question that electrons are freed from metals by the application of heat, but I would seriously question the freeing of electrons by the impact of other electrons. I recognize that this is and has been a frequent mode of stating a method of freeing electrons, but if we carefully analyze the mode of collision of electrons with matter (molecules, or nuclei of atoms), as in the case of cathode rays making collision with the material of the anticathode, X-rays and not photoelectrons are the product of the expenditure of the energy of the cathode ray electron. The cathode ray electron after collision and the generation of an X-ray quantum returns as a slow moving electron and is immediately drawn to the anode.

Again, the author remarks, p. 460, "Their production of light is essentially the same as in the lightning flash. When, during the discharge, the electron meets a positively charged ion, the two oppositely charged particles may combine to form a normal atom." It seems to the writer that a high-speed electron would not normally combine with a positive ion; only when it has lost most of its energy will it do so. This is quite clearly shown in C. T. R. Wilson's chamber. A further statement is made by Mr. Mills: "Then the energy of their collision is radiated as a speck of light—as a 'photon'." To me this is a new concept of the generation of a photon. However, it appears to closely approach my view that light quanta, just as X-rays, are generated by the collision of high-speed electrons with matter, and that the energy of high-speed electrons is, in general, spent in generating radiation quanta (just as was the case with the generation of radiant heat when Planck discovered the quantum theory) rather than knocking out electrons from atoms. The question, therefore, arises: "How are electrons freed and ions formed?" This the writer can fully and scientifically explain, the explanations, however, are technical and possibly The Scientific Monthly would not publish a technical article.

My object in sending this communication is not to criticize the author of the paper on "Electronics." I consider the article well written and highly instructive. My only object was to call attention to some present ideologies in the present mode of writing about electrons, especially in reference to radiation.—Samuel R. Cook.

Soporific

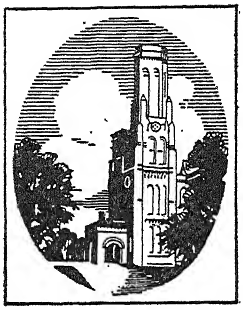
In your March number the article, "The Transplantation of Democracy," by A. G. Keller consumes twelve pages in this era of paper shortage.

I read the article carefully and could not understand the beginning; it moves on, a veritable diarrhea of words and constipation of ideas, and I find no conclusion was reached; "and Lo, the phantom Caravan moves on to the Nothing it set out from."

Formerly I always kept copies of The Scientific Monthly and often reread many of the articles. The Journal has deteriorated so much recently that I look through it rather rapidly and throw it aside.

I think of the old quotation "Now, blessings be on the head of Cadmus, the Phoenicians, or whoever it was that first invented books." Now I thank heaven for a capacious waste basket.—H. W. Soper, M.D.

THE BROWNSTONE TOWER



"When shall I receive my reprints?" No question is put to us more frequently than this and none is more difficult for us to answer satisfactorily. We should be happy if we could state here that reprints are mailed within a week after publication of an issue. However, under the exigencies of wartime the date of mailing has become variable and unpredictable. So far, authors have not had to wait more than two months for reprints; perhaps they are fortunate to be able to get them at all.

Some contributors may not know that the editorial office has nothing to do with reprints except to forward orders for them to the printer. Although we have no objections to serving as intermediaries in subsequent inquiries, we should point out that the author who orders reprints is doing business with the printer and can best expedite that business by corresponding directly with our printing company (for address see masthead or blue slip).

Galley proof with two order blanks (blue slips) for reprints is customarily sent only to authors of principal articles. However, galley proof of other contributions will be sent upon request and reprint orders accepted. We prefer that reprints be ordered on one blue slip at the time the author returns proof to the editor. The other blue slip may be retained by the author as a record of his order. The present blue slip shows a new scale of prices, which needs, and will here receive, more explanation than is given on the blank.

To express tangibly our appreciation to our contributors and at the same time to encourage conservation of paper, we have agreed to give 50 uncovered or self-covered reprints free to each contributor ordering that number. If the author wants more than 50 reprints, he receives none free but is asked to pay one-half the cost of the first 100 and the whole cost of additional reprints. The author pays the whole cost of covers on all covered reprints.

Those who study the table of prices on the blue slip may wonder why the first 100 copies cost about four times as much as the next 100. It is because the first cost includes the expensive labor item, which is practically the same whether one or one thousand reprints are printed. The cost of reprints beyond 100 is proportional to the number ordered, disregarding discounts on large numbers.

Unless the author asks for covered reprints, he gets either uncovered or self-covered reprints. In an uncovered reprint the first page of the reprint is blank or carries the first page of text. In a self-covered reprint the first page, which would be blank in an uncovered reprint, is printed to carry the title of the article, author's name, etc. In a covered reprint a heavy yellow 4-page cover is added, the first page of which is printed like the first page of a self-covered reprint. Because self-covered reprints are more attractive than uncovered reprints, we provide them whenever the first page of a reprint would otherwise be blank.

Ordinarily, reprints will be made on the same kind of paper as that on which the article appeared in the SM. Occasionally, however, an author has requested that his picture and biographical sketch from "Meet the Authors" be added to a blank page of his reprints. In that case coated paper might be used for reprints even though the article was published on rough paper. The printer should be consulted about the cost of this or other special services.

Contributors are warned that if they order too few reprints they may not be able to get additional copies from the printer, because he destroys type and engravings after he has filled the first order for reprints. This is done, of course, to make full utilization of metal. If an author needs additional reprints after the type is destroyed, he can have reprints made by a lithoprinter.

Anyone who wants a reprint of an SM article should write to the author and not to the AAAS or the editor. Mail addresses of authors are given in "Meet the Authors." Do not expect an author to send a reprint by return mail; he may have to wait weeks for them.

So-called "tear sheets" cannot be provided at this time. Our supply of extra copies of the SM is extremely tight; we have none to tear up. For contributors' scrapbooks upon request we can send clippings from galley proof or page proof.—F. L. Campbell.

THE SCIENTIFIC MONTHLY

SEPTEMBER, 1945

A LEND-LEASE PROGRAM FOR PHILOSOPHY AND SCIENCE*

By MAX BLACK

DEPARTMENT OF PHILOSOPHY, UNIVERSITY OF ILLINOIS

Nearly a century ago, some of the most eminent of British scientists were engaged in public agitation upon issues which are still central in all considerations of the place of science in education. The immediate objective of Huxley and his scientific colleagues was the sufficiently modest one of introducing science in small doses into the educational curriculum. At times they seemed to be asking only that science should have, as Huxley said, "as much time given to it as to any other single subject." But the immediate practical issue was merely one aspect of a more inclusive challenge, by the new and still disturbing doctrines of evolution, to established conservatisms and orthodoxies, in philosophy and religion no less than in education.

So it was that the grand debate upon the educational functions of science in an age we are pleased to call "materialistic" was not confined to considerations of direct utility. Naturally enough, the support which education in scientific subjects might give to technological progress and material betterment had great appeal for a generation as devoted as our own to the gospel of getting on in the world. But the contemporaries of Matthew Arnold and Ruskin were forced to think about the *ends* of scientific education, and to examine the details of practical educational reform in the wider philosophical context of the goals of society and the destiny of man.

The case for including science in liberal education was argued on the broad basis of the ability of science to contribute to culture. It was in this spirit that John Tyndall pleaded that his own century had "as good a right to its own forms of thought and methods of culture as any former centuries had to theirs." And Huxley, the most eloquent of all the evangelists for the new outlook, insisted that science had the right to join in the construction of "a complete theory of life." These Victorian scientists in fact were far from endorsing the narrow doctrine that the sole end of scientific instruction might be technical skill—"a process," as Huxley described it, "of manufacturing human tools wonderfully adroit in the exercise of some technical industry, but good for nothing else." For the Victorians, as for us, science must either be an essential component in the "criticism of life" or else have approximately the same educational significance as typewriting, bricklaying, or any other nonintellectual craft.

How far have the hopes for the participation of science in education been realized? If Huxley and Tyndall could be with us today they might take pride in the honorable position accorded to science in contemporary education; but it is safe to suppose they would be sadly disappointed with the fruits of the reform. Scientific ways of thought and investigation have hardly begun to influence social and political affairs; and in spite of Mr. H. G. Wells, science has not made a decisive contribution to the formulation of "a complete theory of life." Science, as now

* From the symposium on "History and Philosophy of Science" sponsored by Section L at the fifth Cleveland meeting of the American Association for the Advancement of Science, September 12, 1944.

taught in the schools and colleges of the western world, supplies neither a guide to action nor a satisfying *Weltanschauung,* and is in some danger of becoming a formal drill as illiberal in its own fashion as the older classical tradition in the days of its decline.

It would be a mistake to explain this relative failure as resulting solely from inadequate teaching methods, deficient physical plant, or the other familiar and persistent obstacles to successful education. The best pedagogical training and the most favorable educational environment will continue to prove ineffective so long as confusion remains concerning the aims of science and its relations to other disciplines. But this philosophical task has been relatively neglected.

While scientists have, in practice, neglected their proper aspirations to join in the creation of a new synthesis of culture in order to concentrate their main educational efforts upon the training of increasingly specialized technicians, their reflections upon the scope and objectives of their own discipline show a corresponding psychological overcompensation. Failure to achieve the cultural and educational objectives of science is accompanied by emphatic advocacy of the universal sovereignty of scientific method. Such pretensions constitute an intellectual isolationism which makes genuine educational advance unnecessarily difficult.

I SHALL assume, from this point onwards, that any substantial progress in the use of science as an educational and cultural agent requires great advances to be made in the common understanding of the specific procedures of the various sciences and their relations both to one another and to nonscientific studies. Since this program needs, as I shall try to show, active cooperation between philosophy and science, the view which is taken of the relations between these two disciplines will have crucial importance. My purpose will be to sketch in brief outline the reasons for the present divorce between philosophy and science and to suggest possibilities for reconciliation in the future. I hope it will be understood that no belittlement of the technological aspects of science is intended; and that if the discussion is conducted at a somewhat rarefied level of abstraction, there is no intention of forgetting the need for testing any educational theory in the light of practical consequences.

OUR discussion has implied a distinction, if not an opposition, between "utility" and "culture," notions as vague as they are indispensable in educational discussion. We shall be seriously misled if we allow ourselves to forget that "culture" is a variable having a wide range of interpretation, and that its adequate definition is itself one of the goals of the philosophy of education.

For Huxley's generation, we may be sure, "culture" tended to have a more personal and individualistic connotation than at present. It might stand for the fullest growth of the individual's latent powers through knowledge of "the best that has been thought and said in the world"—or its meaning might degenerate into the arts of "refined pleasure," providing genteel material for elegant conversation. But in either case the cultured individual of the Victorian conception is too much of an abstraction from his society to satisfy us today. The meaning attached to "culture" is itself one index of a culture's orientation and values.

We can happily find an invariant core of meaning in the alternating shifts from individualistic to social emphases in conceptions of "culture." Whatever else "culture" has meant, there has always been implied *the power to make connections,* to enter through literature and the other arts of human communication into a community defined by shared allegiance to distinctive modes of sensibility, understanding, and appreciation. "Culture" therefore requires the establishment of some *tradition,* no matter how immature or dimly apprehended. As here understood, there will be no highly developed culture without self-conscious awareness of norms and ideals manifested in admired works of the past; and there can be no vital culture where no attempt is made to modify the tradition for projection into the future. The breadth of the culture is determined by the extent to which the tradition is inclusive and tolerant of assimilation and adaptation; the degree to which the de-

fining ideals are explicitly recognized and understood will determine the momentum, the "drive," of the culture.

Where, then, shall we look for the historical origins of a worthy tradition and culture in which science can be required to play an important part? If we are to obtain a tradition deserving of our allegiance we shall need to recognize a continuous background of cognitive aims and methods reaching back to the Ionian Greeks and perhaps beyond. In so doing we are forced to recognize a fact of capital importance to the present discussion, that *in the Greek cultural matrix science and philosophy are interwoven strands in a single intellectual tradition.*

Greek science and Greek philosophy were equally dominated by the ideal of demonstratively certain knowledge; and both accepted essentially the same methods of inquiry and discovery. Not until the seventeenth century, indeed, with the gradual differentiation of experimental methods, "scientific" in a narrower sense, does there arise any "problem" of the relations between science and philosophy. Until that time "... there was no break between philosophy and genuine science—or what was conceived to be such. In fact there was not even a distinction; there were simply various branches of philosophy, metaphysical, logical, natural, moral, etc., in a descending scale of demonstrative certainty." The term "natural philosophy," indeed, lingered on until late in the nineteenth century, and in England at least the term "science" hardly reached its present meaning until the founding of the British Association in 1831.

The shift from "natural philosophy" to "science" marked a far-reaching alteration in the conception of the methods appropriate to the study of the physical world. The cognitive ideals of "natural philosophy," as of the other branches of philosophy, had been largely rationalistic. Euclid provided the pattern, and the search for knowledge was regarded as the deduction of necessary truths from intuitively certain premises. For finite human minds sensory experience might be the avenue to a priori truth; but sensory experience was the *occasion* for knowledge, not its locus; truth was to be found in an understanding of the character of an objective, rational, and intelligible cosmos.

The struggle against this type of philosophical position produced radical shifts in scientific ideals. In attacking a rationalism grown dogmatic and scholastic and supported by the formidable joint authority of Aristotle and the Church, the new experimentalists were naturally more conscious of their deviations from the tradition than of their continued adherence to some of its guiding principles; the struggle to institute experiment became oversimplified as the outright rejection of rationalism. The rise of physical science as a separate and distinctive discipline was felt to depend upon insistence on the superior authority of contingent experience; while the fruitfulness of that insistence and the delightful novelty of its products served in turn still further to discredit rationalism. It was in this climate of opinion that there emerged the separation between science and philosophy which is one of the major unresolved disharmonies with which our own times are plagued.

In the popular conception of today, the rationalistic and metaphysical aspects of philosophical activity dominate; and the mention of an Aristotle or a Spinoza evokes the image of stupendous feats of cerebration, always conducted in an arm-chair, directed towards the definitive revelation of the nature of some such mystery as "Being as such." And the practicing scientist, trained in an educational setting indifferent or actively malicious toward philosophical activity and happily unaware of the rationalistic legacy embodied in his own scientific theories, is glad to seize upon the hasty forays of philosophers into the realms of empirical facts as sufficient demonstration of the futility of all methods of inquiry which are not "scientific."

It matters little that the predominantly rationalistic and metaphysical conception of philosophy is a travesty upon history and overlooks the services of Aristotle to biology, Locke to political theory, Hegel to the analysis of social and cultural institutions, and of many another philosopher to empirical studies. Distortions of intellectual history

are none the less potent for being demonstrably false; and all too many of our contemporaries think of the relations or lack of relation between philosophy and science in terms of a stark opposition between experience and reason, fact and speculation, controlled observation and undisciplined imagination.

The most superficial acquaintance with the history of science and the nature of its present methods is enough to show the inadequacy of these crude antitheses. The work of such men as Duhem, Meyerson, and Poincaré has made plain the extent to which science uses symbolical and conventional artifices to organize and purify the crude materials furnished by direct observation. The privileged position of mathematics in relation to modern science is enough to dispose of the myth that the scientist deals with "facts" uncontaminated by any dealings with the methods of discursive reason.

Science, like philosophy, is an amalgam of components both rationalistic and empirical; but according as this fact is recognized or not there prevail a narrow and a liberal conception of the nature of science.

One influential conception is linked with the trend toward specialization which began in physics and has proved most successful in that science. In this sense all studies become "scientific" to the degree that they aspire to reach the condition of physics and seek to imitate closely the complex interweaving of selective observation, controlled experiment, and mathematical elaboration which is to be found there. Those who favor this definition may take as their slogan the epigram attributed to the late Sir Ernest Rutherford that science consists only of "physics and stamp-collecting."

This first sense of "science" and "scientific method," however narrow in application, has the great merit of definiteness. To make physics the paradigm of all reputable knowledge and to advocate the extension of its methods into chemistry and more distantly related subjects is to make a demand of some precision. But it is also to limit the cultural effectiveness of "science" in a manner which few will find acceptable.

For in *this* sense history and the social studies generally can be called "scientific" only by the kind of extravagent courtesy which would justify the description of a monkey as human. From this puristic standpoint the sociology, history, and philosophy of science itself are outside the concern of reputable cognitive inquiry. Where this narrow sense of "scientific" is adopted as standard, the most important fields of human interest and concern are abandoned to the irresponsible fluctuations of opinion or prejudice.

But "scientific" is an honorific term not lightly abandoned for the designation of cognitive pursuits to which importance is attached and upon which reasonable hopes can be set for advance in knowledge. And, as we have seen, "science," as equated with "natural philosophy," has an older and more significant connotation. One who speaks of "scientific method" in this broader sense can, however, hardly hope for definiteness. Indeed, in virtue of the essential continuity of the cognitive enterprise through the centuries, he comes near to meaning by "scientific method" *a good method for achieving knowledge, whatever the characters of that method prove to be.* On this basis, all studies become scientific *in so far as they yield genuine knowledge,* and the field of science is the whole of available knowledge however acquired.

Whatever plausibility is attached to the universality of scientific method in this generalized sense is specious; for the admonition that all studies shall become scientific is then hardly more than an exhortation to look well upon those who have successfully acquired knowledge—and then to go and do likewise. If any attempt is made to delineate the common features of these successful cognitive enterprises, the result is always of unhelpful vagueness and generality.

The most distinguished living advocate of the extension of "scientific method" into all fields of intellectual inquiry, Professor Dewey himself, can supply no better formula than a reference to "systematic, extensive and carefully controlled use of alert and unprejudiced observation and experimentation in collecting, arranging and testing facts to serve as evidence."

How unhelpful such advice can be to anybody who wishes to learn "scientific method" the example of ethics may illustrate. In order to be "scientific" about ethical questions, Dewey tells us, we must collect, arrange, and test facts in a systematic, extensive and carefully controlled fashion. But what is an ethical "fact" and by what signs is it to be recognized and discriminated from sheer illusion? The description of scientific method gives and can give no useful answer. For to be in a position to recognize ethical objectivity is already to have considerable and valuable ethical knowledge. To know what constitutes relevant *ethical* evidence is to be a long way along the road to the possession of ethical truth. There can be little in the analyses of our eminent contemporaries who have essayed to define scientific method to help in the discovery of ethical data and principles. The social studies can hope for little from such general pronouncements; for what they are told in effect is to seek knowledge by recourse to experience and reason; and while the admonition may be salutary and at least shows a sympathetic interest in the outcome of the projected investigation, it remains as indefinite as a pat on the back.

I HAVE been arguing so far that there is a crucial ambiguity in popular conceptions of science and scientific method, reflecting the historical shift from the inclusive aims of "natural philosophy" to the more restricted and specialized purposes of scientific technology; that the claims of science to have distinctive instruments of knowledge are fully justified only in the case of studies whose condition approximates to that of physics; and that elsewhere, in psychology and ethics, in politics and history, in logic and jurisprudence, the advocacy of scientific method is no more than a hope that better knowledge may at some time be achieved by methods at present unknown. All of which, as my readers will by this time have realized, was intended to lead up to a plea for a reestablishment of the ancient alliance between philosophy and science.

Once we have recognized the continuity of philosophical and scientific aims and methods and see clearly that the pursuit of knowledge requires the exercise of both reason and experience, we should be in no danger of setting up unnatural and constricting barriers between the two. And, by the same token, we can then eschew the partisan attitudes which are satisfied by nothing less than the universal dominion of their favorite discipline. The common business of philosophy and science will then be understood as the acquisition of knowledge by a process which in both cases requires the deliverances of sense-experience to be submitted to symbolic elaboration, tested by intellectual procedures established in an effective tradition of criticism and interpretation, and again checked and controlled by reiterated appeal to experience.

Against this general background of agreement in aims and objectives, it will then remain useful to make a broad division of spheres of interest. So long as men have immediate practical concerns, a major human preoccupation must surely remain the pressing of "studies," in Bacon's phrase, "for the relief of man's estate," the active prosecution of medicine, agriculture, metallurgy, electronics, in short of all cognitive activities directed toward humanitarian and utilitarian ends through mastery and control of the material and social environment. When we use the term "science" (as distinguished from "philosophy"), we may well think primarily of *such* activities—utilitarian in objective and technological in execution, bearing in mind, however, that all such practical concerns are inextricably bound up with the so-called "theoretical" or "pure" studies.

But the desire to *understand* is also a distinctively human preoccupation; and so long as men have interests in the ideal, there will persist an active concern in so organizing the cognitive enterprise that there may emerge a coherent, harmonious, and truthful presentation of the manifold variety of the experienced world in such a way as to satisfy the widest range of human commitments and needs. When we use the term "philosophy" (as distinguished from "science") we may well think of cognitive activities so organized—aiming at understanding and

theoretical adequacy rather than immediate practical application and control.

From this standpoint science and philosophy will have as common subject-matter the whole variety of human experience, so far as it lends itself to cognitive elaboration and report; and the two terms will signify different directions of emphasis in an unbroken and indivisible continuum of varying types of cognitive activity. When a researcher is establishing in the laboratory the physicochemical make-up of a virus of equine encephalomyelitis he may be thought of as acting in a fashion extremely scientific in the utilitarian-technological sense; but when the same man or another tries to determine the difference between a virus and inanimate matter he will be verging upon the philosophical. Toward the "scientific" end of our continuous linguistic scale will be found activities highly particularized, technical, self-contained; toward the "philosophical" end activities abstract, generalized, nondepartmentalized. But it will be well to remember that even this cautious differentiation is a device for facilitating exposition and thought, and that a great many cases of cognitive activity will occur so near the middle of the scale as to be regarded indifferently as "scientific" or "philosophic."

Science and philosophy are here being regarded as continuous in subject-matter, ideals, and methods. But unless one is prepared to say that red is the same as orange and an oak nothing but an acorn, it must be admitted that continuity is not the same as identity. Recognition of the unity of philosophy and science need not, and ought not, to preclude differentiation of appropriate procedures and a fruitful division of labor.

Thus the knowledge of the material world yielded by physics is of the greatest value both to science and philosophy; but it would be a ridiculous error to supppose that all questions which arise in connection with physics can be settled *in principle* within the laboratory. It would be impertinent for a layman to praise the experimental procedures of applied physics whose technological consequences are among the special glories of our age; yet the *concepts* of theoretical physics are notoriously opaque even to the most distinguished practitioners in the field. Once we ask seriously for an *understanding* of physics, seek to criticize its epistemological foundations, establish the meaning of its basic terms and their relation to the crude data of common-sense, we enter upon philosophy. To shrug away such inquiries as unimportant is to deny the dignity of science as an enterprise related to understanding as well as control; but to try to answer them by measurement or calculation is to play the part of a philosophical simpleton. Superhuman intelligence might, and genius occasionally does, master both the exacting disciplines of experimental research and the different but no less exacting demands of sound philosophical training; but for most purposes difference of training and specialization will be essential.

The physicist may manage sufficiently well without much knowledge of the history of his subject, since the instruments and techniques of his profession crystallize and embody the *answers* to older questions arising within the discipline of investigation. But for the philosopher of physics the history of science and philosophy is an indispensable propaedeutic. For intellectual criticism has no secure and tangible instruments to match the equipment of the laboratory; the very tools of philosophical analysis must be re-created in the mind by retracing the paths of the great predecessors. So the history of science and philosophy is no external affair of dates and places but itself a philosophical training. Without it the basic terms of philosophical inquiry, "knowledge," "objectivity," "experience" and the rest, are meaningless noises, and the thinker unwittingly reproduces the mistakes and the discarded fallacies of earlier systems. Sound historical study of any subject requires the mastery of techniques of interpretation which are not those of the physical laboratory; and the use of the history of science and philosophy as training in philosophizing requires further canons of interpretation which are not those of the general historian. To this must be added, in any catalogue of desirable qualifications, mastery of the findings of physics at a level of generality neither too far removed from the concrete particulars of the laboratory nor

prematurely committed to uncritical metaphysics. Comparable preparation in the methodology and history of at least one other science is desirable if not essential. When to this has been added the need for mastery of the new and developing fields of symbolic logic and semiotics (each now able to supply full-time employment to appropriately trained specialists) perhaps enough has been said to establish the case for differentiation of function and training.

Philosophy of science (and the wider field of general philosophy into which it merges) can yield valuable results only when pursued with the same seriousness of purpose as technology. When turned into an occasion for amateurish speculation concerning a mysterious universe by eminent scientists writing in a mood of relaxation from more serious concerns, it can serve only the purpose of an elaborate but ill-constructed fairy tale, in which materialism is always the wicked ogre, and "spirit," "value," or some such disembodied abstraction the providential and benevolent godmother.

The relevance of this analysis to the formulation of a program of cooperation between philosophy and science should by now be sufficiently plain. The general policy to be adopted will be one of seeking to discourage "mechanical" and to foster "organic" cooperation. By a "mechanical" transaction between systems I understand a process in which the giver remains substantially unchanged by the gift, and may have his initial condition restored by reversal of the operation; exchange of momentum through the collision of elastic bodies may serve as an example. By an "organic" transaction is understood a process which irreversibly alters the character of the participants; and of this the relations between a growing organism and its environment may provide a convenient instance. But we have excellent examples also in "lend-lease" cooperation between political states, where differentiation of function in the service of a recognized community of interests dictates a pooling of resources without a mathematical calculation of immediate profit. In urging the advantages of organic cooperation between science and philosophy there is no need to view mechanical relations with reproach: the two subjects can learn much from one another in ways which, leaving both substantially unchanged, are mechanical in the sense defined. But we may reasonably look forward to more intimate modes of collaboration which may profoundly change the character of both science and philosophy as now understood through the emergence of essentially novel disciplines of cognitive inquiry. What can be done to facilitate such cooperation?

There is, first, a need already pressing for the better dissemination of the findings and methodology of science. The type of exposition required falls somewhere between the highly technical reports of research journals and the diluted and often sensationalized accounts of popularized science. Such work as that of Norman Campbell on measurement and Ernst Mach on the principles of mechanics are excellent illustrations of the type of work needed—and also of the difficulty of the undertaking.

There is a correlative need to make available better expositions of philosophical method in a manner which will effectively assist in the furtherance of the cognitive enterprise. In this field also the same gap exists between the highly technical presentations which fill philosophical journals and the vacuous sensationalism of too many popular guides and outlines to philosophy.

Finally, it will be desirable to initiate programs of joint research in fields where there are yet to be found neither sufficient data nor sufficiently clear understanding of adequate procedures. In sociology and ethics, in education and religion, the methods of science and philosophy will interact in the joint search for knowledge *by whatever means prove effective in practice.*

Such a program as this must remain largely tentative in outline, since its definitive character can emerge only through attempts to realize it. To be avoided above all is any premature limitation upon experimentation through antecedent prejudice in favor of methods which have proved their worth in some special field. The standpoint

here advocated sets no arbitrary limitations upon the application of scientific method; it insists only that scientific method be used wherever it *proves* to be effective. We are asking only that the pragmatic temper be applied to the *methods* of scientific inquiry no less than to its findings. In this way we may avoid, both in philosophy and science, the dogmatic provincialism of undue attachment to methods characterizing a provisional stage in human culture and doomed, as all good methods are, to eventual supersession. Science and philosophy practiced in such an undogmatic spirit, recognizing each other's just claims, uniting in the common task of the search for knowledge, may well make contributions of the greatest importance to postwar education.

REFLECTIONS ON ENERGY AND ENTROPY

By Karl P. Schmidt

CHIEF CURATOR OF ZOOLOGY, CHICAGO NATURAL HISTORY MUSEUM

ENERGY

The flow of power in moving mass,
The waves of radiant heat and light,
And the electric sky-illuminating flash,
Are forms of energy.

The heat of moving molecule,
And in the recess of the atom,
Disclosed by modern chemistry,
The vast reserves of locked up force
Are also energy.

What is the law by which we unify
All this quicksilver of creation's veins?
These forms of energy exchange, but are not lost.
What is the Universe, and what are we,
Except creative Energy?

ENTROPY

The clock runs down, the water cools,
And Entropy forever grows.
Who shall wind or warm again,
Except with other energy?

The planetary systems are but clocks,
Their energy inexorably flowing
Outward from their central suns;
Which, cooling, keep the planets wound.

Does not the wider universe
Forever trend, by loss of heat,
Toward that last zero of all outer space
When even helium will turn to snow?
If Entropy's to be the end of all,
Who then shall wind us up again?

MEDICAL INTERNATIONALISM

By ROBERT A. MOORE

PROFESSOR, DEPARTMENT OF PATHOLOGY, SCHOOL OF MEDICINE, WASHINGTON UNIVERSITY

In the spring of 1840 the city founded by Pierre Laclede on the high banks of the Mississippi was a thriving community. The dikes and revetments of the river had been completed by Gratiot and Lee, and there was no longer danger that St. Louis might become an inland city. Steamboat arrivals anually exceeded 1700. The *Missouri Republican* boasted that one could now reach Chicago in three days, and New York in eight or nine. The center of interest at the moment was the manslaughter trial of William P. Darnes, who in an altercation with Andrew J. Davis had inflicted a depressed fracture of the skull with his cane. Judge Bryan Mullanphy presided in the Circuit Court, and Dr. William Carr Lane and Dr. William Beaumont testified.

On the other side of the world in this same spring of 1840 a new epidemic of cholera had broken out in Calcutta. Even if communications had then existed to carry this news to St. Louis it is doubtful whether the editors of the *Missouri Republican* would have given space on the front page to an outbreak of cholera in India. What did disease in Calcutta have to do with the inhabitants of a city halfway around the world? Nothing in 1840; but just nine years later one out of every ten persons in St. Louis was to die of this same epidemic. Let us trace the path by which the germs of cholera were borne by man from India to the Mississippi Valley.

The East India Company had for almost a century held a monopoly on the export of opium from India. Despite a prohibition on the importation by the Chinese government, the trade in 1830 amounted to over two and one-half million pounds. In March, 1839, all foreigners were forbidden to leave Canton, and all opium on shore and on ships in the harbor was seized. The value of this opium was estimated to be $10,000,000. The British Lion was irked by this summary action of the impotent Chinese government and England declared war. British soldiers set sail from Calcutta. Cholera was on the way. Mild to severe epidemics broke out in the Indies and in the port cities of China. Britain won the war and secured cession of Hong Kong and left cholera in China.

For three years the epidemic was swallowed in the wide expanses of southern and central China. A branch passed down what is now the Burma Road to Burma and northeastern India, but the main current was westward with gathering momentum; to Kashgar and Yarkand, in the corner of Sinkiang Province, and across the international border to Bukhara in southern Russia. From this hub of commerce cholera spread along the trade routes south to Balk, Kabul, and Herat in Afghanistan. Another branch passed southward along the river Indus to culminate in an explosive epidemic in Kurachi, in 1846, where at least four hundred and ten cases were observed in one British regiment. The main stream turned westward along the trade and pilgrimage trail to Meshed. The 460-mile jump to Teheran was relatively easy for the now fast-moving epidemic, which was reaching virgin soil where the people had never before been exposed to cholera. Teheran had a population of about 50,000 and only 60 years before had been made the capital of Iran by Agha Mohammed Khan. Caravan routes radiated in all directions, and cholera passed northward along the well-traveled roads to the lands about the Caspian and Black seas, reaching Tiflis, Astrakhan, and southern Russia by the fall of 1846. Moscow fell to the invader in September, 1847. Over one million persons died of cholera in Russia within a few years. In July, 1848, cholera appeared in Berlin and, two months later, in the Atlantic ports of Hamburg and Bremen, ready for the jump across the Atlantic.

The same year was marked by the revolution in Germany resulting in the emigration of over three hundred thousand souls from the oppressions of Europe to the freedom of America. On the twelth of October, 1847, the steamer *Guttenberg* set sail from Hamburg with 250 passengers. Before the landing at New Orleans, fifty-five days later, many

had died of cholera and been buried at sea. Shortly thereafter the steamer *Convoy* cast off from New Orleans and landed at Memphis; the *Amaranth* continued the voyage to St. Louis and when she docked had thirty persons with cholera on board. The first resident of St. Louis to contract the disease was taken ill at 4:00 P.M. on January 5, 1849, and died of cholera in the St. Louis Hospital of the Sisters of Charity at 2:00 A.M. on the next morning. On the same day the Board of Health prematurely declared the epidemic at an end. Deaths in January numbered thirty-three, in February nineteen, in March sixty-eight, and in April one hundred and thirty-one. There were one hundred and ninety-three deaths during the second week of May, but the tidal wave was temporarily halted by the great fire which destroyed the harbor and part of the city. On Tuesday, the tenth of July, one hundred and forty-five deaths in one day were attributed to cholera. Even before this it was evident that drastic action was necessary. On June 25 a mass meeting was called, and the mayor and common council abdicated in favor of a committee with absolute powers and a budget of $50,000. A quarantine was placed on arriving ships. Either from the strenuous efforts of the Ward Committee on Health or from the natural course of all epidemics, cholera subsided to endemic proportions by September, having taken a toll of 4,557 dead out of a population of 50,000.

It is not a part of this story to follow cholera to St. Joseph and out the Oregon-California trail to San Francisco, or to trace its importation into Boston and New York. The reason why St. Louis should have been interested in Calcutta in 1840 has been given. There could be no stronger support for the statement published each week in the Public Health Reports: "No health department, state or local, can effectively prevent or control disease without knowledge of when, where, and under what condition cases are occurring."

Medical Nationalism. The cholera epidemic of 1840 to 1850 is a striking example of the world-wide sweep of a disease: at the time, it was a disaster that shocked America to constructive action. The health officers already knew that quarantine regulations did not adequately protect the inhabitants of the port cities. Furthermore, regulations varied from city to city, and what was good practice and safe in New Orleans was bad practice and unsafe in New York. Nothing could be gained by exclusion of the disease at New York if it could be introduced through the back door by way of Boston or Baltimore. Merchants complained that some quarantine laws were cumbersome and restricted or delayed the movement of goods.

Undoubtedly many people, both medical and lay, were thinking about the problem, but one man did something. Dr. Wilson Jewell, a member of the Philadelphia Board of Health, suggested that a national convention of health officials be called. On October 29, 1856, a committee of three from the Philadelphia Board of Health was appointed to correspond with the Boards of Health of Boston, New York, Baltimore, and New Orleans on the propriety of calling a convention "for the purpose of a conference in relation to the establishment of a uniform system of revised quarantine laws." The convention met in April, 1857, with fifty-four delegates and, in a three-day session, passed many resolutions. "Stagnant and putrid bilgewater . . . filthy bedding and baggage" were condemned. Vaccination against smallpox for those not protected was recommended. Second and third conventions met in 1858 and 1859. At the third a resolution to abolish personal quarantine of patients with yellow fever was accepted eighty-five to six. The fourth convention met in Boston on June 14, 1860, under the presidency of Dr. Jacob Bigelow. A short paragraph from his address gives an excellent picture of the times:

> Our lot is cast in a perilous age. . . . Where shall we fly to escape from east winds and dog days, from pestilences that come and pestilences that do not come, from ships that bring us yellow fever, and quarantines that nourish and cultivate it, from cattle diseases that cannot be exterminated by exterminating the cattle, from lead pipes for water contrived to kill everybody except the animalcules, from fraudulent food and deleterious physic, from drugs that are poisonous and poisons that are adulterated, from infectious patients whose pulses must be felt with a pair of tongs and their chests explored with a tarred stethoscope?
>
> There are perils of nervous men and nervous com-

munities. . . . Like the amphibious animal of the showman who dies in the water and cannot live upon land, it is in vain that the unhappy inquirer resorts to his statistical tables to inform himself whether there is most danger in a steamboat or on a railroad—he unfortunately learns that the most dangerous thing a man can do is to go to bed, for more people die in bed than anywhere else.

For these evils, and many others yet unknown, we trust that the wisdom of future conventions will yet devise a remedy.

The next meeting was to be held in Cincinnati in May of 1861, but a shot at Fort Sumter intervened and the United States lapsed back to local control of disease. Twelve years later, in 1872, the American Public Health Association was organized. Article II of the Constitution states: "The objects of this Association shall be the advancement of sanitary science and the promotion of organizations and measures for the practical application of public hygiene."

In the first century of our existence as a nation legal opinion was to the effect that quarantine, both internal and external, was a governmental function reserved to the states. The health officers of ports had found it necessary to band together into voluntary conventions and accomplish by mutual cooperation what the lawyers said was unconstitutional. Enlightened public opinion was just thirty-one years ahead of the Congress and the Supreme Court. In 1878 the Marine Hospital Service, which had been founded in 1798 to care for sick merchant seaman, was authorized by Congress to impose a quarantine to prevent the entry of disease from abroad. The law was extended in 1890 to include imposition of quarantine to prevent the interstate spread of disease.

Medical nationalism was thus finally achieved, with the states in concerted action to protect the health of the whole nation. Out of sheer terror and suffering the people grasped at those opportunities for self-protection which public health practices seemed to offer.

An Island in a Sea of Disease. Those of us who have lived the greater part, or all, of our lives in the twentieth century have little realization of the sheer terror and suffering caused by disease in the preceding century and even today in many parts of the world. Let us examine the evidence to support the thesis that the United States is an island in a sea of disease.

Malaria is the greatest disease of all. It is conservatively estimated that there are 300,000,000 people in the world suffering from malaria. In India alone there are about 100,000,000 victims, or almost one person in every four. There are over 2,000,000 deaths each year in India directly or indirectly caused by malaria, more than the total deaths from all causes in the United States. What would be our economy if there were forty million persons with malaria instead of the one million which we do have? There are parts of the world where conditions are even worse than in India. An examination of about 7,000 persons in Para, Brazil, showed that 45 percent had malaria. In the British Solomon Islands it is estimated that 90 percent of the natives have malaria. In Burma the death rate for malaria in 1931 was 127 per 100,000 population.

In many parts of the world other dangerous infections are unchecked. Between 80 and 95 percent of the people on Hainan Island have hookworm. In China there are 35,000,000 persons with active tuberculosis, 4,000,000 lepers, and 15,000,000 individuals blind because of trachoma, smallpox, and gonorrhea. Filariasis exists in 35 percent of the population of Ceylon. Annually in Greece there are about two million cases of malaria and a million and a half cases of dengue fever. The death rate in Java is 20 and in India is 24.8 per 1,000 population, as compared with an average of 10 in the United States. The mortality from typhoid in Batavia, Dutch East Indies, is 365 per 100,000. In 1943 there were no fewer than 125,000 cases of epidemic typhus fever in North Africa from Casablanca to Port Said.

Provisions for the care of the sick throughout the world vary inversely with the incidence of disease. The latest available statistics have been collected by the Division of Preventive Medicine, Office of the Surgeon General, United States Army. In the United States there is approximately one physician for each 745 in the population; in Ceylon the comparable figure is 6,400; in the Netherland East Indies 1,139; in India 10,000; in China 45,000; in New Guinea 27,000; and in

the Solomon Islands 12,000. In the United States there are 9.7 hospital beds per 1000 population. The figures for Ceylon are 1.7; for the Netherland East Indies, 0.9; for India, 0.3; and for China, 0.06.

The Consequences of the Introduction of a Disease. With good sanitation and reasonable care an epidemic of an intestinal disease transmitted directly from man to man, such as cholera, will gradually die out completely. But the story of a disease with an animal reservoir of infection is quite different. Let us follow the travels of bubonic plague, especially as they concern the United States.

Plague is a disease caused by a bacterium and occurs not only in man but in rats and other rodents. The rat flea feeds on a sick rat and takes up the bacteria from the blood into its stomach. The flea may then bite a man and thus transfer the bacteria. Man becomes ill and the lymph nodes enlarge and produce swellings, or buboes, which give the name to bubonic plague. Rarely the bacteria are passed from man to man in droplets, as is the virus of the common cold. This type is known as pneumonic plague.

On March 6, 1900, a Chinese man living in the Chinese quarter of San Francisco died of plague—the first recorded appearance of plague on the North American Continent. What followed in the next few years is one of the brightest and the darkest chapters in the history of the control of the disease in this country. Plague was a loathsome disease associated with vermin and no self-respecting American city in 1900 could admit harboring it. Accordingly, newspapers, public officials, influential citizens, and even the courts and the medical profession denied that plague was in San Francisco in 1900. Unfortunately, rats could not read the newspapers and continued to scamper about the Golden Gate. Infected fleas passed from one generation to another, occasionally seeking new pastures in the deep furry coat of a ground squirrel. The rodents, rats, and ground squirrels moved east, and by 1910 the migration had proceeded halfway across California. In 1930 the western border of Montana had been reached, and in 1940 the advancing edge was well into Montana, Wyoming, Utah, and New Mexico.

So long as sylvatic plague remains in a rural and thinly populated region it is not a serious problem, except to the hunter or farmer who handles a sick or dying animal. But now infected rodents are approaching within striking distance of the dense rat and human populations of the large cities of the Midwest. No one knows just when or how the plague bacilli filtered through the quarantine at San Francisco, but since 1900 no less than 499 individuals have fallen ill and 314 have died of plague in the western and southern United States. Herculean efforts of the United States Public Health Service have kept the disease under control much as the coal-mine fires of southern Ohio are controlled—always smoldering, rarely bursting into flame, but never extinguished.

This 44-year experience with plague should teach us that we cannot, even with modern medical science, erect a leak-proof wall around the country. Medical isolationism is as fallacious in concept and practice as economic and fiscal isolationism.

A Disease Is Conquered. Yellow fever is caused by a virus and is carried from man to man, and probably from animals to man, in most instances by the mosquito, *Aedes aegypti.* Infected mosquitoes and sick human beings followed the great routes of sea trade. Periodically major epidemics appeared in the port cities of the world. Examples are: Barcelona in 1821 with 20,000 deaths; New Orleans in 1878 with 4,046 deaths; Philadelphia in 1803 with 3,900 deaths; and thirteen times in New York between 1791 and 1807 with a loss of a tenth of the population. The last great epidemic was in Rio de Janeiro in 1928–29 with 435 deaths.

Throughout the nineteenth century health officers struggled against the unknown. The terror in Philadelphia in the 1793 epidemic was recorded for posterity by Dr. Benjamin Rush. People fled from the city, friend avoided friend, relatives were abandoned to die in solitude, burials were made unceremoniously with only the negro driver of the death cart present in the middle of the night.

Armies had come to grief in the western Indies. Napoleon's troops sent to reconquer Haiti in 1800 were wiped out by yellow fever,

as his grand army was decimated by dysentery and typhus in Russia. Toward the end of the nineteenth century American troops occupied Havana, which had been visited by yellow fever every summer in memory. William Crawford Gorgas, a young American physician, was appointed chief sanitary officer of Havana in 1898. He believed that yellow fever was caused by filth and set out to houseclean Havana in the most approved New England fashion, to the delight of the American press. The next year there was another devastating epidemic of yellow fever. The situation was so serious that in 1900 the Surgeon General of the United States Army appointed a commission to visit Cuba and investigate the cause of yellow fever. These were the men—Walter Reed, James Carroll, Jesse W. Lazear, and Aristides Agromonte—who were to solve one of the most baffling of mysteries—the transmission of yellow fever.

In 1793 Dr. Rush had noted that "moschetoes" were plentiful around Philadelphia, and Noah Webster in New York in 1795 wrote "musquetoes were never before known by the oldest inhabitant to have been so numerous." As in the rare conjunction of the planets, the year 1900 and the city of Havana brought together a galaxy of men—Reed and his commission, Gorgas, Dr. Henry R. Carter, and Dr. Carlos J. Finlay. Several years before in Virginia, Carter had collected data suggestive of an insect vector. Carlos Finlay, the prophet without honor, had for years insisted that yellow fever was transmitted by the mosquito.

The product of the joining of these minds is well known and has been immortalized in words and picture as the victory over "yellow jack." Immediately Gorgas took up the war on yellow fever, but this time with precise knowledge to guide him. The breeding places of the Aedes mosquito were eradicated. The work began in March, 1901. There were only five deaths that summer in Havana, and none in 1902, 1903, and 1904. In 1905, there was a mild outbreak, quickly brought under control. Since then yellow fever has lost its sting in Havana. Research had once again penetrated one of Nature's secrets cleverly hidden for ages. This is not the place to relate how Gorgas went on to Panama and, by the same methods, conquered the jungle and made possible the construction of the Panama Canal.

The eradication of a mosquito from a large tropical country like Brazil is not utterly impossible: in fact, the Brazilian government and the International Health Board of the Rockefeller Foundation can proudly point to the fact that because of work begun in 1926 no aegypti-transmitted yellow fever has been seen in Brazil since 1937. To accomplish this result required determination, constant vigilance, and strict control. These are a few of the regulations of decree #21, 434 of the Brazilian government:

Article I. Personnel of the yellow fever service shall visit weekly . . . . all premises, inhabited or otherwise. . . .

Article II. Physicians of the service . . . . shall have immediate free entry at all times to all places.

Article X. The mosquito-proof closing of all water containers of every description is obligatory.

Article XVI. Only basements which may easily be inspected and will absolutely not collect water shall be allowed.

Article XVIII. The pavement of open areas and of walks shall be level without any depression, and have a sufficient incline for the water to drain away readily.

Article XX. Ornamental work, façades, caves, stone carving roofs, and awnings of buildings shall be constructed in such a manner that they cannot collect water.

Article XXIV. Metal roofs shall not be constructed of sheet metal that can be easily bent and may therefore collect water in the depression thus formed.

Article XXVIII. Drinking water receptacles for animals . . . . shall be so designed that they can be quickly and completely emptied.

Article XXIX. Flower vases, jars, jardiniers, and other ornamental articles in cemeteries shall never be allowed to contain water.

Article XXXV. Plants, which by the arrangement of their foliage are able to collect water . . . . shall at the discretion of the service be destroyed.

Article XXXVI. The use of unsplit bamboo for the construction of enclosures, or as fence, is prohibited.

Article L. The use of old automobile tires as fenders on the sides of watercraft shall be permitted only when the holes made in them are not less than an inch and a half in diameter and not more than twenty centimeters apart, so that they cannot retain water.

Call this regimentation if you wish, but, after all, government is the banding together of people for the common good. Which is worse—to build an ornamental roof in a specific way or to die of yellow fever? Yellow

fever has not yet been completely conquered, but the terrifying threat of the major epidemics of the nineteenth century has been banished.

The Beginnings of Medical Internationalism. At about the same time that Dr. Jewell sought cooperation of the health officers of the eastern port cities of the United States, the governments in the Mediterranean Basin looked about for some integration of sanitary regulations. The opening of the Suez Canal was responsible for the first real international sanitary code, subscribed to by fourteen governments in 1892. In 1893 ten European states agreed to notify one another of the outbreak of cholera and provide measures to control this disease.

In the Western Hemisphere in 1887 Uruguay, Brazil, and Argentina signed a sanitary code covering their water frontiers in relation to cholera, yellow fever, and plague. A second attempt at international cooperation in 1904 was subscribed to by Argentina, Brazil, Paraguay, and Uruguay. Finally, the sanitary convention in 1905, ratified by fourteen countries, marked the beginning of the present Pan-American sanitary code, signed at Havana November 14, 1924, and enforced by nineteen governments. The objects of this code are stated in Article I:

1. The prevention of the international spread of communicable infections of human beings.
2. The promotion of cooperative measures for the prevention of the introduction and spread of disease into and from the territories of the signatory governments.
3. The standardization of the collection of morbidity and mortality statistics by the signatory governments.
4. The stimulation of the mutual interchange of information which may be of value in improving the public health and combating the diseases of man.
5. The standardization of the measures employed at places of entry for the prevention of the introduction and spread of the communicable diseases of man so that greater protection against them shall be achieved and unnecessary hindrance to international commerce and communication eliminated.

The signatory powers to the Versailles Treaty agreed to "take steps in matters of international concern for the prevention and control of disease." This was the first time an important political treaty gave consideration to health. In accordance with this broad principle, three organizations were established: a health committee of twelve members, an advisory council, and a health section of the secretariat of the League of Nations. The actual practical work of the League has been along two lines: creation of a service of epidemiologic intelligence and public health statistics in Geneva and in Singapore; and the constitution of expert commissions to study specific problems. Madsen summarizes the accomplishments of the malaria commission as follows: ". . . establishing methods of investigation and so determining the extent and degree of gravity of malaria in the world; organization and training of malariologists who have in turn organized the antimalarial services in their respective countries; laying down methods and principles of treatment and, so far as possible, standardizing methods of work; setting on foot an extensive program of coordinated research . . . in various malarial countries . . .; and the committee has always been at the disposal of malarial countries for purposes of investigation and to assist them in organizing their malarial services." There have been commissions on tuberculosis, syphilis, rabies, leishmaniasis, leprosy, rural hygiene, typhoid fever, physical education, biological standardization, and nutrition.

Although the League of Nations had some noteworthy failures in the field of international political relations, collaboration between nations in health work was a success. The protection against pestilential diseases was stronger in 1939 than in 1920, largely because of international collaboration. The gains in the interim of peace must not be lost in the shambles of World War II.

The Threat in a Modern World. The threat of world-wide visitations of pandemic disease are still in existence. Only twenty-six years ago influenza spread like a prairie fire over the greater part of the earth. In the United States alone there were no fewer than 548,000 deaths. The epidemic of yellow fever in Rio de Janeiro in 1928, with 435 deaths, has been referred to.

New inventions and advances in transportation and communication bring their special problems. The airplane has brought the disease-ridden tropics within a few days' or hours' journey of our country. Let us re-

turn to the steamer *Guttenberg,* which took fifty-five days to cross from Hamburg to New Orleans. If anyone went aboard in Germany during the incubation period of a disease he would have become ill before arrival in the United States. Today a person may be bitten by a mosquito or sandfly in India, China, Africa, or South America and be in the U. S. many days before there are symptoms of illness. Actually, the health authorities keep in touch with every passenger who comes into this country from the tropics for fourteen days after his arrival. Shortly after the airplane lands, the commissioner of health in the home town of each passenger is notified by telegram and asked to report any illness in this person during the stated period.

An even greater threat than the importation of a sick human being is the insect "stowaway." A mosquito carrying the organisms of malaria in a salivary gland may secrete itself in a corner of an airplane and move from one continent to another. This actually happened a few years ago when the gambial mosquito crossed the South Atlantic from Africa to the hump of Brazil. This kind of mosquito above all others delights in biting man, and therefore is especially dangerous if it transmits disease. In Brazil this mosquito did transmit malaria. After a vigorous campaign the Brazilian government and the International Health Board of the Rockefeller Foundation have succeeded in eradicating the gambial mosquito from Brazil. A new type of disinfection and quarantine has been designed to cope with air transportation.

Looking at medical internationalism in terms of dollars and cents, we are told that our postwar economy must include a tripling of exports. The attainment of full export trade would be easier if the millions to whom we are to sell were healthy enough to work and earn enough to buy our goods. Some years ago the average cut of sugar cane per native on a certain tropical plantation was about one ton per day. Swamps were drained, mosquitoes were eradicated, and the average cut rose to about two tons per day. The cost was more than returned to the owner in a few years, but, more important, this little spot on earth became a better place to live in. We are not our brother's keeper but we are all our brother's helper.

The Dawn of a New Day. Despite all that has happened in the past, and all that might happen in the future, many see the dawn of a new day.

During a war the pressure for new discoveries is greatly increased and the old maxim "necessity is the mother of invention" is once more confirmed. Our ideas of the world and its problems are sharply focused and crystallized. The past is truly only prologue.

The supreme test of whether or not modern medical science had outgrown its swaddling clothes came shortly after Pearl Harbor. The United States Army was confronted by the largest and the most complex problem of health protection that has ever been faced by a military force of any nation. Army and Navy posts circled the globe; troops were stationed in every conceivable environment from the land of perpetual snow to the steaming jungles of India, Burma, and the Islands of the Southwest Pacific. Could medical science conquer disease under unfavorable circumstances in a few months in a manner comparable to what had taken years in the mild north-temperate climates of Europe and North America? The answer is written in the accomplishments of the Army Medical Corps, the Naval Medical Corps, and the United States Public Health Service during the past three years.

In November, 1942, American troops invaded North Africa. Among the natives there were 125,000 cases of epidemic typhus fever. Vaccination, adequate sanitation, and the now-famous DDT were used; the number of cases of epidemic typhus among our soldiers can be counted on the fingers of one hand. With few exceptions the experience with other insect-borne diseases has been the same. General Simmons, in April 1943, stated: "No authentic reports have been received of cases of yellow fever, plague, sleeping sickness, or relapsing fever" in American troops. A notable exception has been, and is, malaria. However, the reports are most encouraging in contrast with what might have been. For the decade before the war the annual admission rate per 1000 enlisted men for malaria varied from 1.5 to 5. Despite the heavy concentration of troops in the Fourth Service Command (that is, in the

South) the admission rate in 1942 was only 0.6 in the continental United States. Although the figures for troops in tropical and subtropical regions at first glance are high, the natives in these same regions show a far greater incidence. General Simmons gives figures for five unspecified theaters of operation as follows: 47, 53.5, 106, 173, and 38. The highest hospital admission rate for all disease in any theater of operation is 1091 as compared with 755 for the continental United States. Who would have been so foolish ten years ago as to predict that hundreds of thousands of young American men could be taken to every part of the world with a maximal increase of only 45 percent in hospital admissions for all disease.

It is possible that military discipline is sufficient to protect the men in the Army. What of civilians? With the same methods can medical science meet a major epidemic in a population that has been trampled under the hoofs of the Four Horsemen? The answer is,Yes! and the best example is the epidemic of typhus fever in Naples in the winter of 1943–44.

In the spring of 1943 there were a few cases of typhus in Naples, but no one paid much attention to them. Through the summer an occasional person fell ill with typhus. This was the danger sign. Whenever an epidemic disease of winter persists through a summer an explosion the next winter may be anticipated. The allied Armies took Naples in the fall of 1943. The ragged, underfed population, living under adverse conditions for many months, was an ideal soil for typhus. In December the weekly figures for cases of, and deaths from, typhus steadily mounted. By Christmas it was evident that something had to be done. A group from the headquarters of the U. S. Typhus Commission in the Near East was sent to Naples.

Epidemic typhus is a disease caused by a small parasite, called a Rickettsia, and transmitted from man to man by the bite of the body louse. It is a lousy disease, both literally and figuratively. A logical approach to prevention is, therefore, the eradication of all body lice. Lice leave an infested man soon after his death, and hence the immediate neighborhood where a death has occurred is highly dangerous. Fortunately, DDT had just become commercially available. The U. S. Typhus Commission went to work, and the folk of Naples were scientifically and systematically deloused, giving attention to the three groups: inhabitants of a district where a death had taken place, contacts of living patients, and the population at large. Vaccine was used where indicated. By February the epidemic was over. This means little to anyone who does not know something of the natural history of an epidemic of louse-borne typhus. From too much experience we know that an epidemic with the background of the 1944 Naples explosion would have been likely to continue on a plateau from February to May and subside only during the early summer. Few doubt that preventive medicine cut short the life of epidemic typhus in Naples.

Medicine and the physician have in their hands the most precious thing in the world—human life. In the name of humanity, if not in the name of our own health and economic stability, America must participate in a world unit of health, a part of a greater organization to include economics, finance, trade, social relations, and the many other aspects of modern life. Only then will man attain the full stature for which he went through the renaissance, the industrial revolution, the French and the American political revolutions, and two World Wars.

DESERT CHANGE: A STUDY OF THE BOULDER DAM AREA

By ROBERT M. GLENDINNING

ASSOCIATE PROFESSOR, DEPARTMENT OF GEOGRAPHY, UNIVERSITY OF CALIFORNIA AT LOS ANGELES

THE Boulder Dam area represents a significant portion of the southwestern dry lands of the United States. Moreover, it is a section of the dry country in which the occupance factors have been changing with inordinate swiftness, especially since 1930, and one in which they have progressed to the point of relatively broad geographic importance. This article proposes to describe the changing scene, using the past as a marking-gauge and carrying "the present" up to "the year of Pearl Harbor," 1941. The exigencies of war prevent discussion of factors involved in the immediate present, including several types of military camps, large areas commandeered for military training purposes, the completion and operation of one of the world's larger magnesium plants, new road construction, increased travel, and population increase. Future studies are contemplated, and it will remain for them to analyze the effects of war-impact on the area. Fortunately such a break in the study is logical inasmuch as the present conflict is having

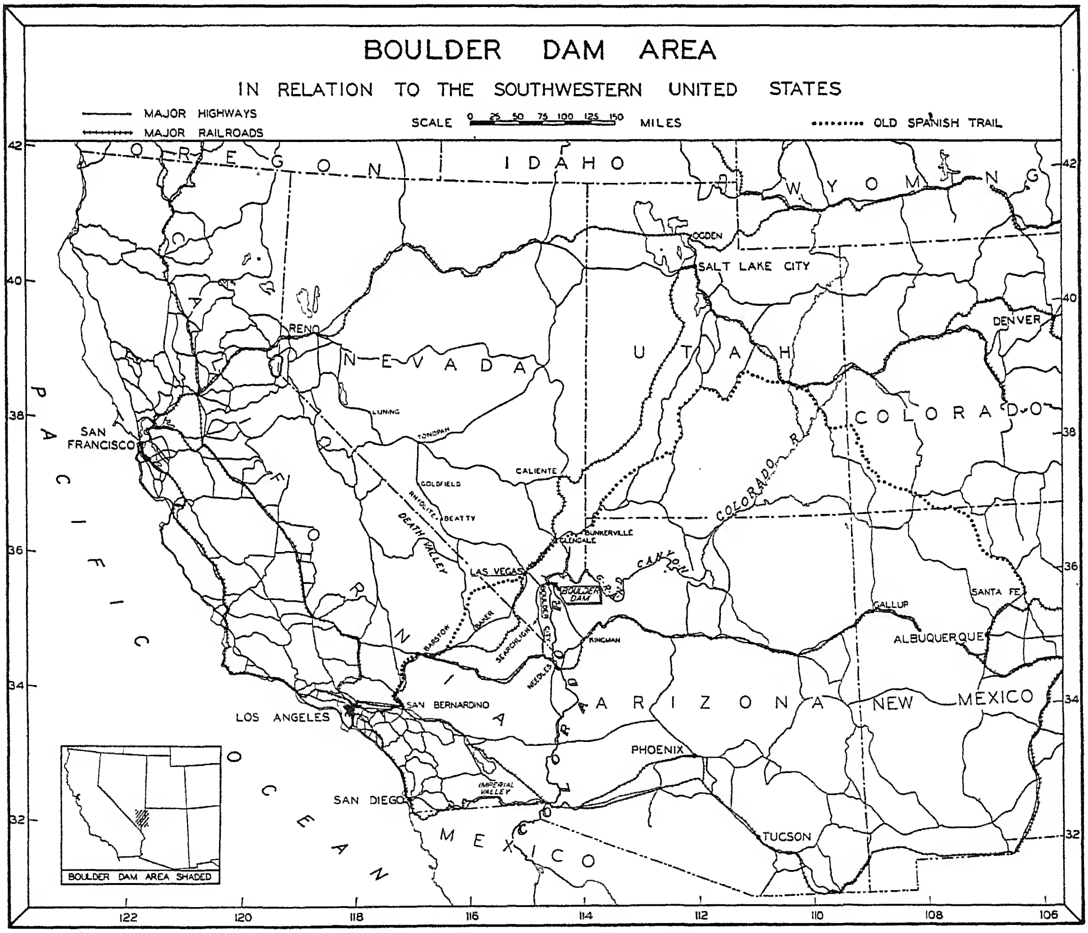


FIG. 1. GENERAL LOCATION MAP

NOTE THE POSITION OF BOULDER DAM WITH REFERENCE TO POPULATION CENTERS, MAJOR HIGHWAYS, ETC.

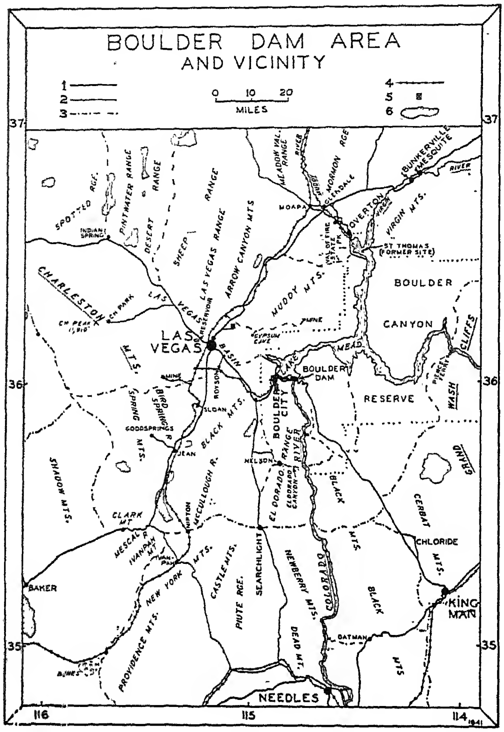


FIG. 2. LOCATION MAP[1]
SHOWING THE CENTRAL POSITION OF BOULDER DAM AND LAS VEGAS AND THE CIRCULATORY SYSTEM OF THE AREA. RARELY USED DESERT ROADS ARE NOT SHOWN.

many direct and far-reaching effects on the Boulder Dam area, effects which cannot, in any event, be properly comprehended or projected at this time nor, probably, for several years after the cessation of hostilities.

The area occupies a large section of extreme southeastern Nevada and reaches into northwestern Arizona (Fig. 1). It has a north-south extent of some 90 miles, from the head of the Virgin river arm of the Lake Mead reservoir to the latitudes of the famous old mining town of Searchlight. In east-west spread it stretches from the lower end of the Grand Canyon and the bold escarpment of the Grand Wash Cliffs, on the east, across the reservoir waters and the alluvium-floored Las Vegas basin to the high ramparts of the Charleston Mountains, a distance slightly in excess of 100 miles (Figs. 2, 3).

[1] The key at the head of the map is as follows: 1. First-class highways (modern, wide, all-weather); 2. Second-class roads (usually narrow, all-weather); 3. Third-class roads (narrow, impassable at times); 4. Railroads; 5. Airports; 6. Playas ("dry lakes").

Being a desert region in every sense of the word, the most visibly conspicuous features in the broader geographical scene are the landforms (Figs. 3, 4, 5). Rugged, scantily clothed mountains, marked by pronounced north-south trend, comprise the bulk of the view (Fig. 3). On the east is the layered edge of the Colorado plateau (the Grand Wash Cliffs) and on the west the steep and jagged wall of the Charleston Mountains. Set down in between the north-south trending mountains are numerous elongated, trench-like valleys and a few less-attenuated basins (Fig. 11). Parched and windswept, these lower sections are being slowly filled and dammed by the ever-encroaching alluvial fans and, as indicators of the extreme desiccation, are dotted with many salt flats or "dry-lakes" (Figs. 4, 6). Cutting through higher and lower levels alike, and at the

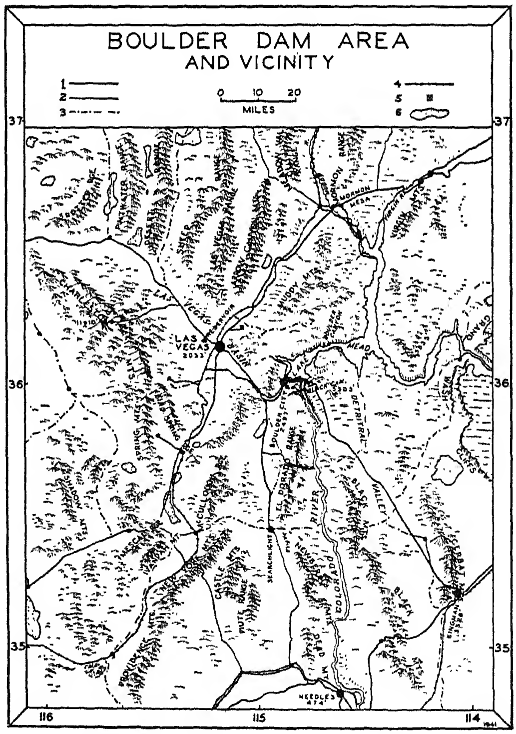
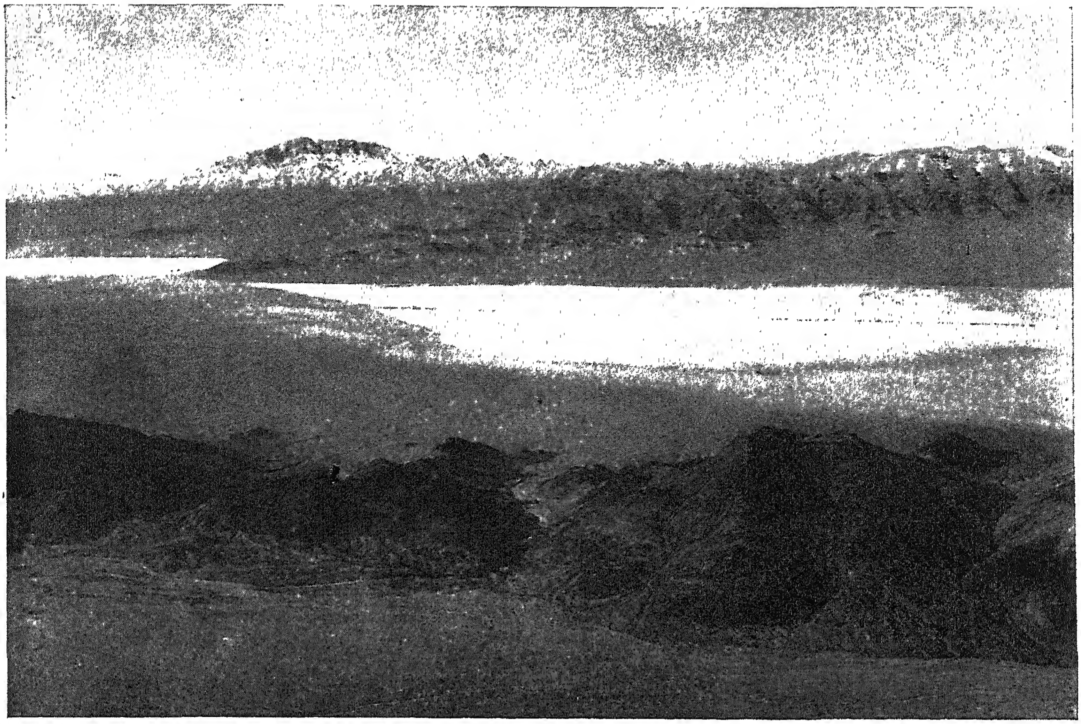


FIG. 3. PHYSIOGRAPHIC MAP[1]
NOTE THE NUMBER AND CONSPICUOUS NATURE OF THE HILLS AND MOUNTAINS WHICH BORDER AND ENCOMPASS THE DESERT BASINS. THE NORTH-SOUTH "STRIKE" IS READILY APPARENT, AS IS THE WASHBOARD EFFECT OF THE TOPOGRAPHIC COMPLEX. THE GRAND WASH CLIFFS, MARKING THE LAYERED AND TURRETED EDGE OF THE COLORADO PLATEAU, APPEAR IN THE MIDDLE OF THE MAP'S EASTERN MARGIN.



Fairchild Aerial Surveys photo

FIG. 4. A PORTION OF THE BOULDER DAM AREA SOUTH OF THE LAS VEGAS BASIN.
A LAND OF PLAYAS, ALLUVIAL FAN FILL, AND BASIN-ENCLOSING MOUNTAINS (SEE FIG. 2 FOR LOCATION). THE PLAYAS ARE ROACH DRY LAKE, TO THE RIGHT, AND IVANPAH DRY LAKE, TO THE LEFT. THE DARK LINE WHICH BISECTS ROACH DRY LAKE IS A PART OF THE LOS ANGELES-SALT LAKE HIGHWAY. THE VIEW IS WESTERLY.

same time partially ignoring the present "landform grain," is the great elbowed slash of the Colorado gorge, the river itself lying as a meager thread far below the majority of the basin floors.

Considering the ruggedness of the relief, the degree of moisture deficiency, and the blinding heat of the long summer period, it is difficult to think of this "washboard of hell" as a habitable land. Yet man has been in the scene, regardless of numbers and importance, for some eight millenia and, most significantly, now seems well on his way to a more and more permanent maintenance of his establishments.

The first dwellers in the apparently unchanging physical setting were the small, obscure, and exceedingly primitive Indian groups of about 8000 years ago. Only relatively less obscure and few were the more recent Indian populations represented, in order, by the Basketmakers and the Pueblos. Finally in the Indian picture are the Paiutes, who are still in the region; they, however, represent a much more primitive group than the forerunning Pueblos. None of the Indian groups accomplished any truly significant modification of the landscape, although it is probable that early hunting and collecting had some effect on the plant and animal life. In addition, the few and small communities of the Pueblo peoples represented a beginning, especially in the Virgin river area, for sedentary agriculture, carried on under irrigation, by closely knit groups. The Paiute, now a laborer on ranch or railroad, or an inhabitant of the Indian Reservation at Moapa, still plays an insignificant role in the area.

The story of the first penetration of the region by the white man is not completely known. It seems that about 1774 some mining was carried on by Spaniards in the southern part of the area, as, for example, in Eldorado Canyon (Fig. 2). This, however, did not represent a permanent in-

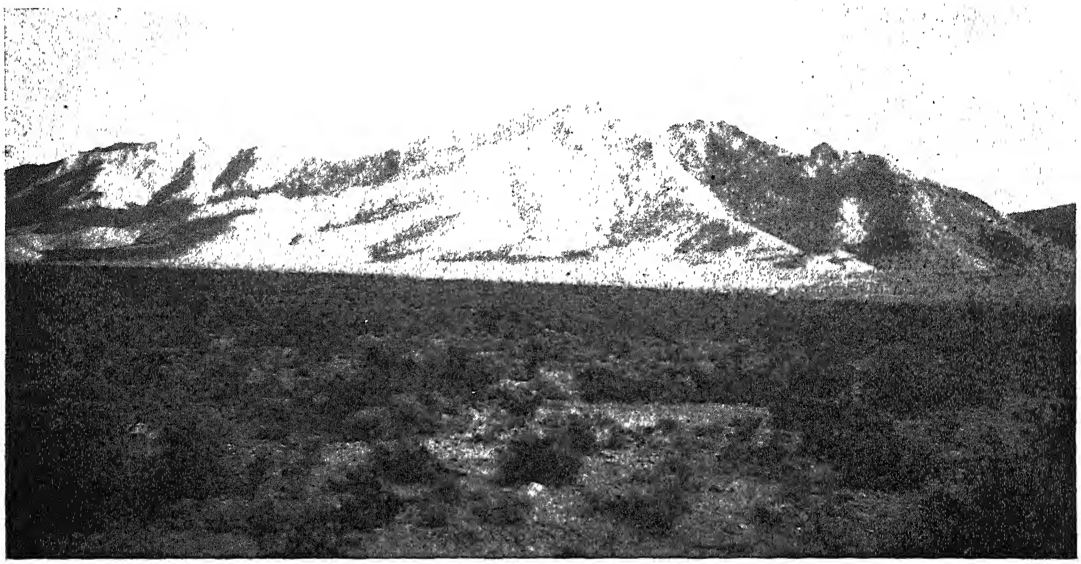


FIG. 5. TYPICAL DESERT VIEW
A CLOSE-UP PHOTOGRAPH SHOWING CHARACTERISTIC ALLUVIAL FAN SLOPE, COVERED WITH DESERT BRUSH AND RUBBLE, AND THE SHARP BREAK BETWEEN DESERT FLOOR AND RUGGED AND ESSENTIALLY BARREN MOUNTAINS.

habitation nor did it have any significant consequences. Before that, Cardenas, one of Coronado's Captains, had come upon the Grand Canyon from the east in the year 1541, but he did not continue westward into what has become the Boulder Dam area. In 1776 Escalante's explorations northeast of the region led to information on the route to California from the direction of Salt Lake and to the knowledge of available water at the springs of Las Vegas. Traders, chiefly from Sante Fe, soon followed, and a definite and permanent establishment of the Old Spanish Trail (Fig. 1) through the region became an accomplished fact by 1830. In 1844 Lieutenant Fremont, after crossing the northern Mohave Desert and skirting the southern end of Death Valley (Fig. 1), picked up the Old Spanish Trail to traverse the area, camping at Las Vegas before continuing in the direction of Salt Lake. Such were the beginnings of what have proved to be the forerunners of significant changes, and even though permanent settlements were not established by these white traders, miners, and explorers, the rudiments of a communication and transport system were nevertheless laid down as connecting threads from waterhole to waterhole. It was still a land to get through as quickly as possible—a rugged, thirsty land seemingly suited only to the Paiute, whom Fremont referred to as "humanity in its lowest form and most elementary state."

For some years the Boulder Dam area continued in the role of a transit region without, as far as the white man was concerned, any permanent settlements. In 1847 Mormon parties from Salt Lake City traveled through the area via the Old Spanish Trail to California, and this, backed up by further use and the maintenance of posts or stations, led to the designation of this part of the route as the Mormon Road. Thus began the Mormon impress which had an important part in the changing scene, especially in the more northerly portion of the area.

The year 1855 marks the beginnings of many of the elements which exist today. A trading-post oasis, protected from the pilfering Paiute by a stockade, was established at Las Vegas (Figs. 1, 2, 3), where, as the name implies (Las Vegas meaning "The Meadows"), water was available in sufficiently large amounts. This Las Vegas Mission, as the Mormons called the settlement, introduced irrigated agriculture, as carried on by the white man, and attempted to teach the Paiutes to "raise corn, wheat, potatoes, squash, and melons."

Mining and smelting of lead were instituted on a small scale some 18 miles south of the Mission, and thus after more than three-quarters of a century mining, begun and abandoned by the Spaniards, re-entered the scene. The entire project—agriculture, mining, and the education of the Paiutes—was far from successful, and in 1857 the settlement was abandoned. Its failure was speeded by the growing trouble between the Mormons and the U. S. Government which resulted in the recall of settlers to the Salt Lake area. However, the buildings remained to be used by traders and by the carriers of the Overland Mail, and during the Civil War infantry and cavalry were stationed there to guard the route to Southern California. Soon after the withdrawal of the Mormons the land and water rights were largely taken up as a succession of non-Mormon ranches—ranches which are in operation today, some of them in the ultra-modern form known as the "dude ranch." The mining activities started by the Mormons grew slowly in the surrounding desolate country to culminate in the general, but short-lived, mining boom of the early 1900's. Many of the "boom towns," such as Beatty and Rhyolite (Fig. 1), were located at varying, but considerable, distances from Las Vegas; however, Las Vegas was the general service center for many of them, even when they lay outside the "Boulder Dam area," and, hence, "Vegas" grew as they prospered. As a definitely and permanently established town Las Vegas came into existence in 1905. Just as it owed its earlier being to a line of transport, likewise this new phase of its existence, related still to water supply, ties in with the coming of a more modern transportation development, in this case the railroad. The San Pedro, Los Angeles, and Salt Lake Railroad (now part of Union Pacific) purchased a town site, and a true town, replete with town lots, streets, and water systems, sprang up from the desert shrub. The rail line from Salt Lake was completed in 1905, and the Las Vegas and Tonopah Railroad, tapping the country to the northwest, appeared a year later. In 1909 Las Vegas assumed its present role of county seat for Clark County. Two years later, by act of the legislature, it became a city (Fig. 7). But again, as in the days of the Mormon Mission, these transformations were confronted with many difficulties. In 1910 a torrential rain, flooding through Muddy River Valley to the north, wiped out more than 100 miles of railroad south of Caliente (Figs. 1, 2). Service was not re-established until five months later. Hard times, reflecting the importance of the railroad, ensued, and Las Vegas was for a while practically on the West's long roster of "ghost towns." Mining and stock raising in the surrounding region likewise had their ups and downs over the ensuing years. The Las Vegas and Tonopah Railroad was abandoned in 1918. Today its roadbed is still utilized, but now as a base for a modern highway. Then, finally, when it appeared that removal of the Union Pacific railroad shops from Las Vegas would again seriously undermine the economic base of the city, the Boulder Dam project, with all its concomitants, burst on the area to give the city a new and different lease on life. The signing of the first construction contracts in 1930 marks a new era in the desert transformation.

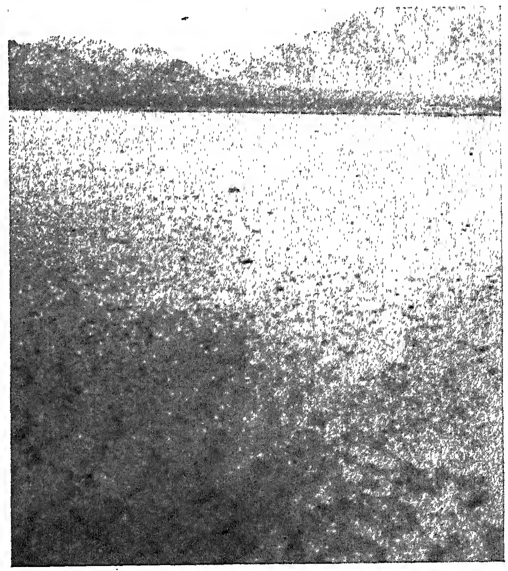


FIG. 6. MESQUITE DRY LAKE
A CLOSE-UP OF A TYPICAL PLAYA SET BELOW THE GENTLE SLOPE OF THE ALLUVIAL PIEDMONT AND FAR BELOW THE MOUNTAIN RIM. NOTE EXTREME FLATNESS OF THE MUD-CRACKED FLOOR OF THE PLAYA.

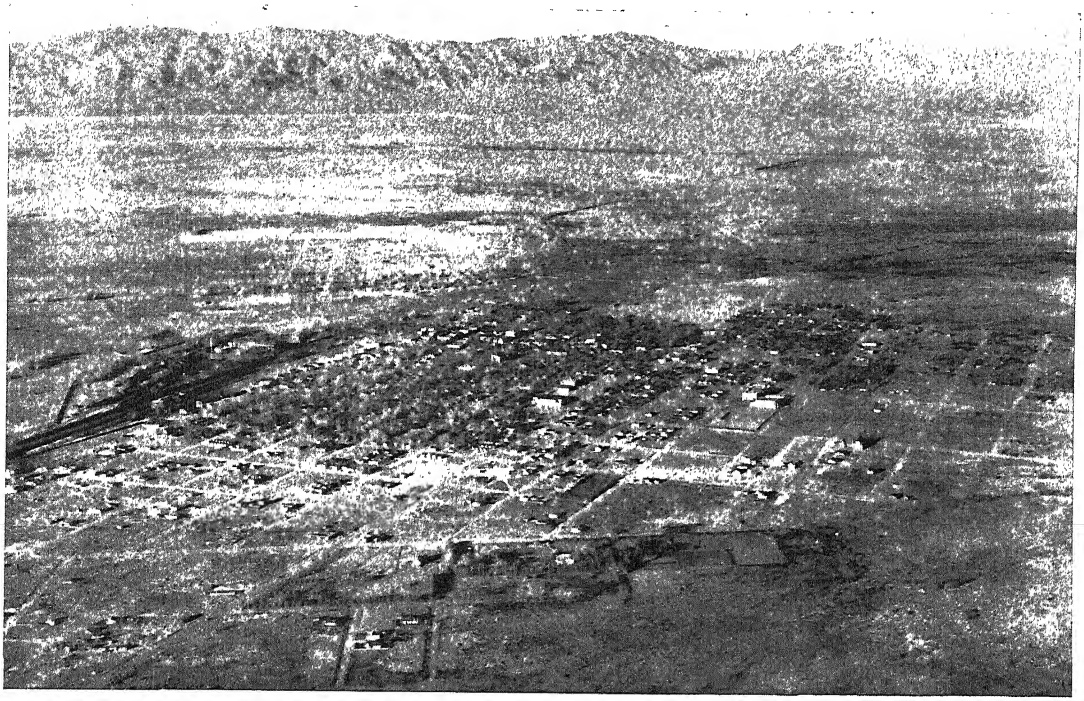
While all this was happening to the Las

Vegas portion, approximately the same sorts of things were taking place in some other parts of the region. Mormon settlements were established in the Virgin river and Muddy river valleys, to the north, along or near the line of the Mormon Road. As in the case of the Las Vegas Mission, the emphasis was on agriculture, and here also abandonment followed by later resettlement occurred. Today these settled sections form "strip oases" along the Virgin and the Muddy, producing export crops in the form of melons, vegetables, turkeys, etc., which travel by truck over broad paved highways to both near and distant markets. The Moapa Valley (the lower portion of the Muddy river valley), unlike the Virgin river valley, is served by a railroad tentacle from the main line of the Union Pacific (Fig. 2). Here again Boulder Dam finally enters the picture as a modifier, for the reservoir created by its construction has now covered the lower sections of these agricultural areas and has forced the abandonment of the Mormon villege of St. Thomas (Fig. 2).

The above-noted elements in the changing scene, as at and around Las Vegas and in the Virgin and Muddy river valleys, took place west and northwest of the Colorado gorge. The eastern and southeastern portion of what is now the Boulder Dam area was more isolated and less known, especially the territory stretching eastward to the Grand Canyon country (Figs. 1, 3). However, in the gorge itself there was an interesting, and at least temporarily important, development which paralleled in time the happenings already described. In 1857 a small steamboat ascended the Colorado River to approximately what is now the site of Boulder Dam (Figs. 1, 3). Two years later the Mormons of the Moapa Valley established a landing, known as Fort Callville, on the west bank of the river about due east of Las Vegas. This marked the up-river limit of navigation as carried on for several years—carried on even with ports as far removed as San Francisco. A report of 1868 indicated a trade beween the navigable section of the Colorado and the port of San Francisco of some $1,500,000 for a six-month period. In 1869 Major Powell made his famous journey down the Colorado through the Grand Canyon and on into the gorge section of the Boulder Dam area, and Powell's reports did much to stimulate interest in the region. But, in spite of these activities, the gorge section was left almost unchanged until Boulder Dam came.

Thus, among the man-induced changes as found in the region up to approximately the year 1930, the part played by the Indian proved a relatively insignificant one, while that of the white man assumed considerable importance. Inconspicuous Indian trails had become well-marked trails, trails became roads, and some roads have become highways which are segments of transcontinental routes (Fig. 1). Foot travel changed to horseback travel, horseback travel gave way partially to team and wagon, the stagecoach appeared on the scene, to be followed, at about the turn of the century, by the railroad; the railroad still hangs on but it has lost some of its strength as its services are taken over more and more by the modern desert vehicle, the automobile. It is difficult to overemphasize the part played by the automobile. Whether for pleasure or business, it has become king of transport, both intra- and interregionally. With the automobile have also come tourists and with them money, and money from outside sources often means the difference between life and death for many a desert area. The effects of the automobile, as reflected so importantly in this area by the tourist trade, were being felt shortly before the coming of Boulder Dam, but it has been the dam itself which has "boomed" the trade and caused persons to tarry long enough to spend truly significant sums of money in the region.

Along with the evolution of the routes and the carriers went an evolution in settlement. Waterholes largely dictated the settlement sites and, hence, to a degree the general alignment of the routes. Indian hut and pueblo ruins gave way to stockaded posts and spots and strips of oasis agriculture; mining camps, often soon to be "ghost towns," sprang up in the most unpredictable positions; a scattering of ranches, their number curbed by aridity as reflected in scarce water and sparse feed for stock, lost themselves in the broad desert sweep; and finally, the posts



Fairchild Aerial Surveys photo, 1932

FIG. 7. LAS VEGAS, NEVADA

THE LINE EXTENDING FROM CENTER LEFT TOWARD THE UPPER RIGHT CORNER IS PART OF THE MAIN RAILWAY BETWEEN LOS ANGELES AND SALT LAKE CITY. THE CHARLESTON MOUNTAINS APPEAR IN THE LEFT BACKGROUND.

became permanent towns and villages and one of them, Las Vegas, took on the aspect of a city. All of this took place before the construction of Boulder Dam. Granted this, it is still true that the dam has caused an acceleration in cases where progress was being made and has stopped or slowed down the forces of retrogression in others. These things cannot be accurately measured, but certain it is that obliteration of Boulder Dam would largely destroy the present economic base. To grasp the real import of this it is necessary to see what changes the dam has actually brought.

The coming of Boulder Dam might be said to be purely fortuitous. Its advent represented no need or demand within the area but rather the needs and demands of other regions, most of which are far removed (Fig. 1). The lower Colorado Valley flooded; certain down-river sections wanted a steady, rather than a spasmodic, supply of water for irrigation; the ever-growing Los Angeles area desired more water and more cheap hydroelectric power; so, finally, a vast project as set up by the Swing-Johnson Bill came into being on paper in 1928. The spring of 1931 saw the first excavation, water storage began in the late winter of 1935, the first hydroelectric power was generated in the fall of 1936, and the summer of 1941 witnessed the world's largest reservoir filled to capacity. Meanwhile many things have happened to modify the desert region which lies contiguous to the dam and the reservoir —things which would never have happened were it not for the fact that sufficient control of the Colorado's waters forced the choice of the site for the dam far upstream into the gorged section of the valley, hence, far removed from the areas which supposedly were the major beneficiaries of the Boulder Canyon Project Act.

When reference is made to the changes brought about by Boulder Dam, it is often forgotten that the dam represents no mean transformation in and of itself. Wedged across the upstream section of the Black Canyon gorge of the Colorado (Figs. 8, 9), it rises higher from its base than any other

dam in the world. With a volume exceeding that of the Great Pyramid, it reaches upwards 726 feet, is 660 feet thick at the base, and tapers to 45 feet thick at the crest. Yet, despite these dimensions, it is dwarfed by the still higher walls of Black Canyon and by such nearby features as Fortification Hill.

The major physical feature resulting from the construction of Boulder Dam is the Lake Mead reservoir (Figs. 3, 8, 10), which, paradoxically, is now the world's largest artificial lake existing in one of the drier sections of the earth. Unlike the dam, Lake Mead is not dwarfed by the contiguous natural features. (For reasons of security the publication of aerial photographs to reinforce this point is not allowed at this time). Here, again, some figures are necessary to portray the degree of transformation that has occurred. Lake Mead is about 115 miles long and varies in width from a few hundred feet in the gorge sections to 7 or 8 miles in the more open portions; its depth at the dam is nearly 600 feet and even upstream as far as the Virgin river reservoir-arm (the largest northern extension) it is more than 500 feet deep; in area it represents some 227 square miles of water surface. At capacity Lake Mead holds enough water to cover the entire State of Connecticut to a depth of 10 feet and enough to supply New York City for twenty years; and all this in a region where a single small spring has often meant the difference between life and death, a region that receives an average annual precipitation of but 5 or 6 inches and which in some years may receive less than 2 inches. The Lake Mead reservoir controls three-quarters of the run-off of the Colorado basin and can contain at one time a full two-year's flow of the river. Upstream from the head of the reservoir the Colorado still runs uncontrolled but as it enters the narrow, walled head of the reservoir its muddy flow is checked and the water changes from the brown of the canyon torrent to the clear blue of the lake. Underneath the lake surface the silty flow continues as "another kind of river" flowing slowly and muddily along the old channel of the Colorado. Estimates vary widely, but it seems probable that it will take at least 200 years for this "mud-river" to fill the reservoir to the point where its efficiency is destroyed, and before that time it is entirely possible that ways will be found to lead at least part of this mud-stream through lower conduits in the dam. Downstream from the dam, where formerly the river carried an average silt load of 300 tons per minute through Black Canyon, the controlled stream now flows clear. In fact it has so little load at present that it is beginning to scour its former deposits and thus cut a new gorge into a more ancient one.

FIG. 8. BLACK CANYON AND PART OF LAKE MEAD
LOOKING UPSTREAM FROM THE CREST OF BOULDER DAM, FORTIFICATION HILL IS SEEN IN FAR BACKGROUND.

Accompanying the clarity of the waters directly below the dam is the establishment of uniformly cool water temperatures, for the released water is drawn not from the surface of Lake Mead but from depths of 150 to 300 feet. As a result the Colorado is now a trout stream for some distance downriver from the base of the dam. Above the dam, the reservoir has been stocked with millions of fish (bass, crappie, catfish, perch) and, in addition, its surface constitutes a tremendous wildfowl refuge located along one of the major migration lines. Thus, as part of the transformation of the area, the building of the dam has resulted in the covering of more than 200 square miles of desert surface with water, has changed the behavior of water and silt movement, radically modified the conditions of aquatic life, and, in addition, furnished a water supply for the nearby new settlement of Boulder City (Figs. 2, 12).

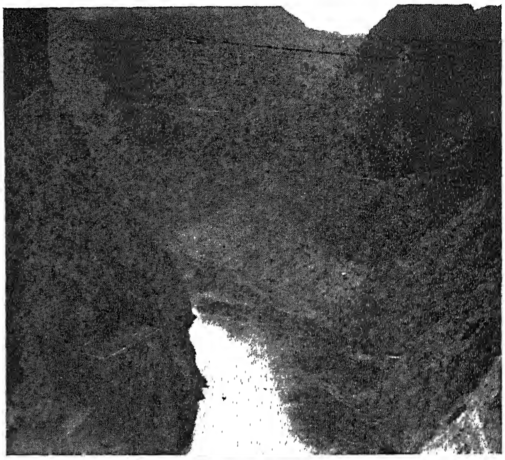


FIG. 9. BLACK CANYON AND THE COLORADO LOOKING DOWNSTREAM FROM THE CREST OF BOULDER DAM. THE DEPTH OF THE GORGE CAN BEST BE APPRECIATED BY REMEMBERING THAT THE CREST OF THE DAM IS ITSELF FAR BELOW THE TOP OF THE CANYON.

Another important element in the evolution of the country at, and in the vicinity of, the dam is the creation, or in some cases the modification, of transportation lines. To provide a flow-line for materials for the building of Boulder Dam and Boulder City a new railroad was constructed from the main line of the Union Pacific. This line begins at Boulder Junction just south of Las Vegas, extends southeasterly across the Las Vegas basin, pushes through the basin rim via Railroad Pass (Fig. 11), and then turns easterly to the railroad yards at Boulder City, from which point it makes a winding descent of more than 1000 feet into the western side of Black Canyon at the dam-site (Fig. 2). Somewhat counterbalancing the creation of this approximately 35-mile tentacle, the flooding of the Virgin river arm of the reservoir, on the north, has caused the removal of about five miles of track from the spur line that serves the Moapa Valley. Another phase of change in relation to the railroad is an increase in passenger traffic on the main line of the Union Pacific through Las Vegas because of rail-tourists visiting Boulder Dam and Lake Mead. This does not, however, affect the spur-line to the dam, mentioned above, inasmuch as such passengers are transferred to busses at Las Vegas. Likewise, busses handle the same type of traffic flow from the main line of the Sante Fe at Kingman, Arizona, even though Kingman is some 80 miles southeast from the dam and is, in any strict sense, outside of the Boulder Dam area (Figs. 1, 2).

Of much greater importance than the effect on railroads is the effect on roads. Already existing roads have seen a tremendous spurt in automobile travel, and this is especially true of the transcontinental highway that passes through Las Vegas on its way from Salt Lake City to Los Angeles (Fig. 1). This road is the modern equivalent of the Old Spanish Trail whose course it at least approximates. Also, at about the time that the activities associated with the Boulder Dam construction began, Death Valley, to the west (Fig. 1), was coming into real prominence as a resort area, and these two factors working together helped to cause the modernization of, and the increased flow of traffic on, the Las Vegas-Death Valley road. Further information on the role of Death Valley is given by the author in the July 1940 issue of *Economic Geography*.

Even more important has been the creation of new roads where none previously existed. Most pertinent in this connection is the truly modern highway that now connects Las Vegas and Kingman. Certainly here is a change in the picture that no one would have even thought of a few years ago. With a rugged, parched, and nearly uninhabited

land split in twain by the practically impassable gorge of the Colorado a more or less direct-line highway between southeastern Nevada and northwestern Arizona seemed quite out of the question (Fig. 3). Then, suddenly, came the need for a first-class road from Las Vegas to what was soon to be Boulder City and Boulder Dam. When the dam had bridged the canyon, the highway was continued, making use along part of the route of sections of poor ranch and mine roads in Detrital Valley (Fig. 3), and now a broad paved highway leads directly into Kingman. This has the important result of cross-linking two of the major routes of western travel: the Sante Fe-Los Angeles highway and the Salt Lake-Los Angeles highway. In addition, it also ties in as part of a wide circuit around the entire Grand Canyon (Fig. 1). As in the case of the railroads, there has been a minor deletion of a few miles of lesser roads as a result of the filling of the reservoir in the lower Virgin and Moapa Valleys and in a few other places as, for example, the northern end of Detrital Wash. But unlike reservoirs that have been created in rather densely settled humid lands, Lake Mead has not appreciably disrupted the existing road system; rather, as pointed out, the entire project has had the opposite effect.

Another kind of road has also been added to the circulatory system of the region, in this instance the water road as opposed to the land road. Because of Lake Mead it is now possible to travel by boat literally into the lower gorge of the Grand Canyon and into other sections which were hardly less accessible a few years ago. As a result, vast areas are now open to the scientist and the tourist, where before 1930 a mere handful of men had been able to penetrate, and some had penetrated never to return. It is as if a portion of the Lost World had been suddenly placed on the doorstep of civilization. To fulfill the ever-growing demand for lake travel a year-around boat service is operated from the Hemenway Wash embayment of Lake Mead (just northwest of the dam) with local trips to the upstream face of the dam and longer trips to the Virgin River section and the Grand Canyon, the latter representing a round trip distance of about 200 miles. Beside the regular boat service there are already (1941) nearly 300 privately owned small craft in use on Lake Mead.

Even in the roads of the air there has been some transformation which correlates with the coming of Boulder Dam and Lake Mead. The Las Vegas airport has seen an increase in both private and nonprivate plane traffic, a portion of which is attributable to factors associated with the dam and the lake, and at Boulder City a newly created airport serves private planes as well as furnishing daily service via commercial air lines.

Thus, on land , water, and in the air, lines

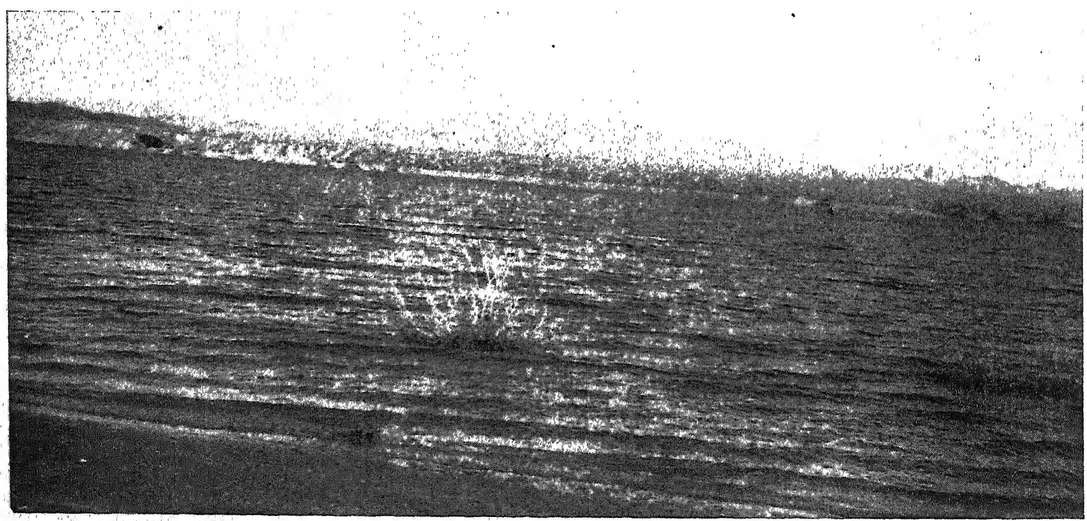


FIG. 10. AN ARM OF LAKE MEAD NORTH OF BOULDER CITY
MILLIONS OF GALLONS OF FRESH WATER IN THE STRANGE ARID SCENE. THE VIEW IS TOWARD THE NORTHEAST.

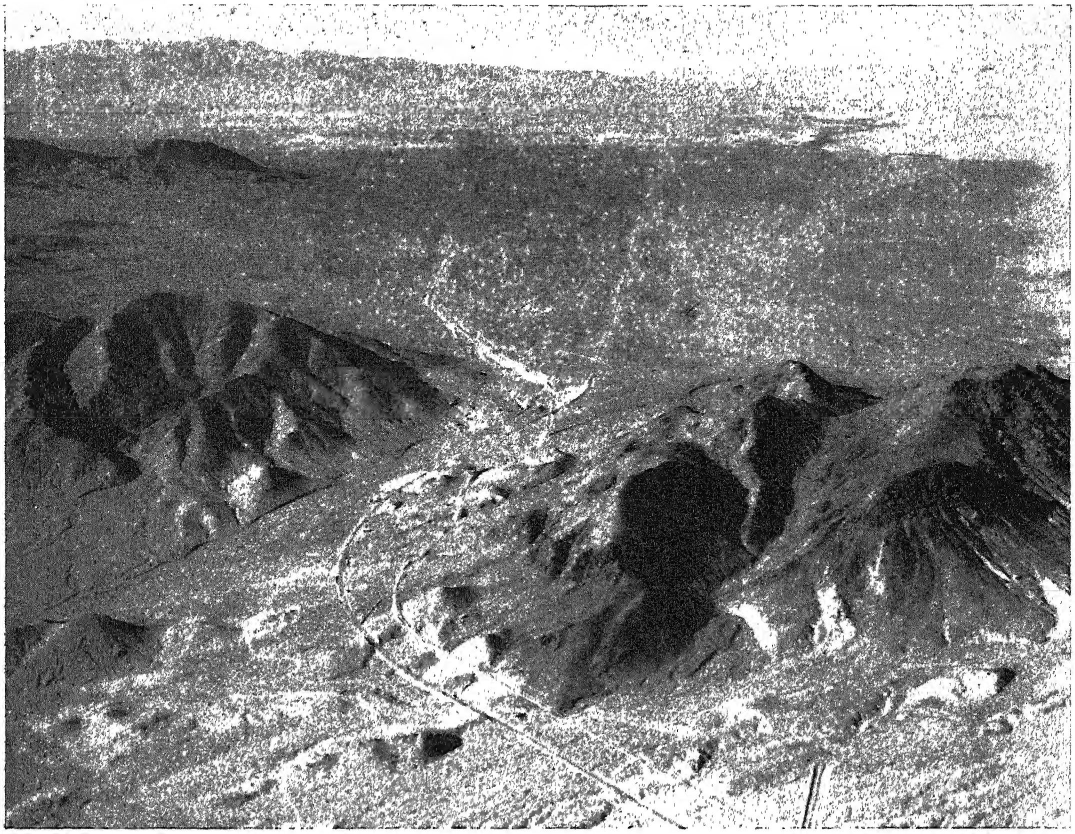


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FIG. 11. RAILROAD PASS AND THE LAS VEGAS BASIN

LOOKING NORTHWEST ALONG THE STRAIGHT LINE OF THE HIGHWAY, LAS VEGAS IS LOST IN THE DESERT HAZE AT THE END OF THE ROAD. THE PASS IS THE ONLY EASY ROUTE FROM LAS VEGAS BASIN TO BOULDER CITY (SEE FIG. 3 FOR LOCATION). THE MORE CURVING LINE WHICH CROSSES THE HIGHWAY AT THE PASS CREST IS THE GOVERNMENT-OWNED RAILWAY WHICH BEGINS JUST SOUTH OF LAS VEGAS AND ENDS AT BOULDER DAM ITSELF. NOTE, IN CENTER LEFT AND IN IMMEDIATE FOREGROUND, THE FLOOD PREVENTION WORKS FOR THE PROTECTION OF THE RAIL LINE FROM THE "FLASH-FLOODS" SO CHARACTERISTIC OF THE DESERT LANDS. TO BRING THIS PICTURE UP TO DATE THE LARGE BUILDINGS AND THE MODEL HOUSING OF BASIC MAGNESIUM INC. SHOULD APPEAR BEYOND THE CENTER OF THE PICTURE ALONG THE HIGHWAY AND ON TO WHERE THE RAILROAD BENDS TOWARD THE HIGHWAY.

of movement of persons and goods have been affected, or created, to provide a modern part of the changing desert complex. These routes, whether roads, railroads, and so on, are dead things, yet they aid in the maintenance of life to the degree that they serve as veins and arteries which connect settlement with settlement, settlements which in the Boulder Dam area range from isolated ranches and mines to Las Vegas and Boulder City.

Just as the coming of Boulder Dam has played a vital part in the present-day expressions of lines of transport, likewise it has brought recent and important changes in regard to settlements and population. Perhaps the most conspicuous element in the evolving settlement picture was the "overnight" establishment of a full-fledged town in a barren, rocky, and isolated section that normally would be incapable of supporting even a handful of persons. This settlement, the planned town of Boulder City, came into being in 1932 to serve the needs of the thousands of persons working on Boulder Dam. It is located about 7 miles southwest of the dam on the higher and more level country west of the Black Canyon Gorge, where summer temperatures are somewhat cooler and where the terrain allows the establishment of

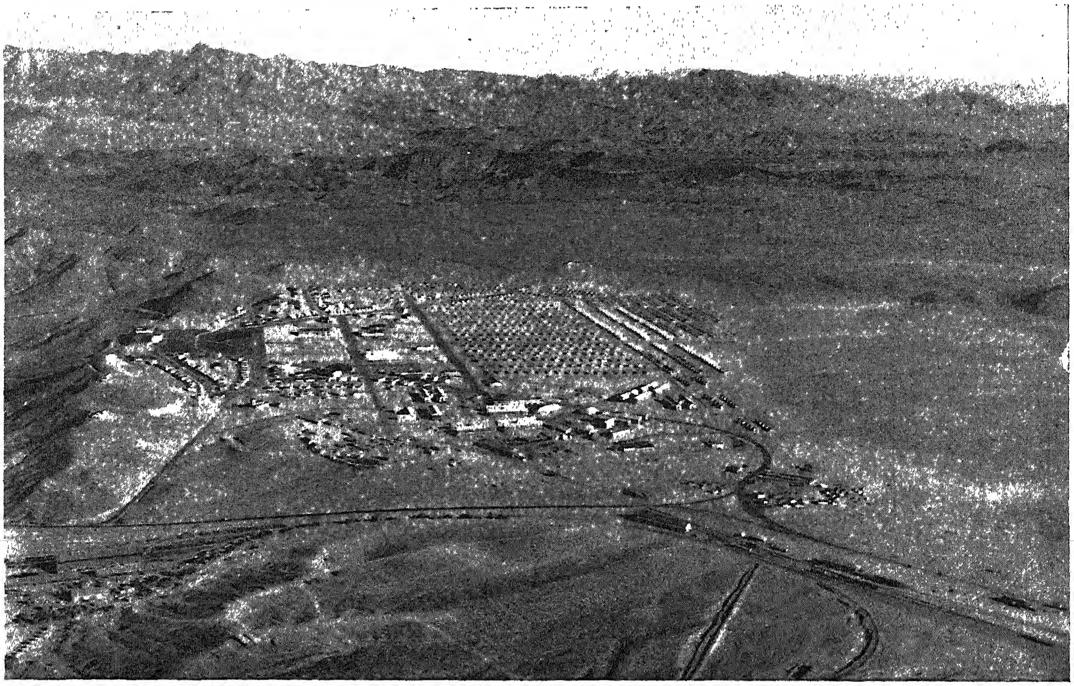

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FIG. 12. BOULDER CITY, NEVADA

LOOKING EASTERLY ACROSS THE BLACK CANYON GORGE IN WHICH BOULDER DAM IS LOCATED. THE DARKER ROCK FORMATIONS MARK THE TREND OF THE GORGE. THIS VIEW, TAKEN IN 1932, SHOWS HOW THE PLANNED GOVERNMENT TOWN HAS SPRUNG FROM THE DESERT WASTE. THEN A "CONSTRUCTION TOWN," IT IS NOW A PERMANENT AND ENLARGED CITY ADDING GOVERNMENT AND TOURIST-CENTER FUNCTIONS TO THOSE OF AN ORDINARY CITY.

a regular plan not too stringently controlled by the topography (Fig. 12). No longer a construction town, it has acquired a permanent status as the home of men who operate and maintain the dam, the powerhouses, and the entire reservoir property, and as such it has a nontransient population of about 3000 persons (1941). Being permanently established, and being where it is in reference to Boulder Dam, Lake Mead, and the modern transportation systems, it has also taken on the job of an important tourist and recreation center; thus, mixed in with what might be termed the "Government town" is the town of numerous restaurants, automobile service stations, tourist courts, hotels, stores, and all the other elements that go with a heavy flow of persons and automobiles along the highway. The heaviness of this flow, and hence its effect on Boulder City, may be given a necessary recognition by pointing out that in the 17 months preceding September, 1941, about one and one-fifth million persons passed through the town and nearly all of them contributed in some degree to its upkeep.

Indicative of still further, and likewise recent and rapid, transformation of the settlement picture is the coming of "another Boulder City." This town, called locally "Basic Magnesium," is in connection with the establishment of what is reputed to be the largest single magnesium plant in the world. Begun just previous to the participation of the United States in the present war, this magnesium plant represents a financial outlay of $133,000,000. Located just south of the main highway between Las Vegas and Boulder City, about ten miles southeast on the outskirts of "Vegas" itself, this town even in its initial development represented a settlement of several thousand persons (see latter part of explanation which accompanies Figure 11). Drawing on Boulder Dam power and reaching, in this instance, far northwest to the vicinity of Luning, Nevada, for ore supplies (Fig. 1), this development has added a new, and again virtually un-

predictable, modification of desert brush and rubble.

Supplemental to these major elements in the matter of *new* settlements are several minor ones which should not be ignored. Along the highways leading out of Las Vegas, especially along the route to Boulder Dam, there has been a mushrooming of small buildings and clusters of buildings serving as restaurants, gasoline stations, taverns, tourist courts, etc. Likewise, and even considerably removed from Las Vegas and Boulder City, similar new services have appeared here and there along the main traveled roads as the dam has encouraged heavier and heavier tourist flow. Also, under the control of the National Park Service, carefully planned and supervised small settlements are being established here and there on the Lake Mead shore, both near the dam and near the extreme head of the reservoir, as at Hemenway Wash and Pierce's Ferry. In some cases, as near the dam, a variety of functions are represented, including boating, bathing, camping, fishing, and housing. In other cases they are merely isolated fishing camps as, for example, the one at the end of the road next to the junction of the Muddy and Virgin river arms of the reservoir.

Of the settlements extant at the birth of the Boulder Dam project, Las Vegas has been most affected; its modification being second only to the outright creation of Boulder City. In fact, as stated above, the project actually saved Las Vegas from a serious decline. To be sure, other factors have been operating simultaneously. Las Vegas is now a divorce, marriage, and gambling center; also it serves as a base for recreational use of parts of the high Charleston Mountains near Charleston Peak (Figs. 2, 3). However, all these are more or less directly connected to Boulder Dam and Lake Mead—granted a combination of factors, it is, nevertheless, a combination in which Boulder Dam is not only the key ingredient but a catalytic ingredient as well. There are certain indicators which support this contention. For example, the number of tourists visiting the area in 1930, the year before construction of the dam began, was slightly more than one-tenth of a million, whereas in 1940 the number was about two-thirds of a million. This great number is now (as of September, 1941) reflected in Las Vegas by 45 auto courts, 40 hotels and rooming houses, 25 garages, and 28 service stations. Major payrolls in the area also reflect the situation. They are per month for Clark County (1940): mines \$60,000, railroad \$90,000, City of Las Vegas \$100,000, and Boulder City \$200,000. When it is remembered that Las Vegas is the county seat, the chief commercial center, and the major recreational unit, the connection between it and the Boulder Dam payroll is fairly obvious. Regardless of the exact weighting to be given the effect of the dam of Las Vegas, it is significant to add that the city's population has approximately tripled since the dam came into the picture (1941 nontransient population was about 10,000). How much money has been provided this increased population of Las Vegas by the approximately 5,000,000 persons who have so far visited Boulder Dam is impossible to say; however, it amounts to many millions of dollars and represents the difference between a large and thriving desert center and one that is dwindling and nonprosperous.

A CENTURY AT THE SEASHORE

By JOEL W. HEDGPETH

TEXAS GAME, FISH AND OYSTER COMMISSION, ROCKPORT

UNTIL a hundred years ago the seashore, with all its myriads of nameless creatures, had been left to itself. It had remained, almost since the beginning of time, as Sir Edmund Gosse described it:

> The rocks between tide and tide were submarine gardens of a beauty that seemed often to be fabulous; and was positively delusive, since, if we delicately lifted the weed curtains of a windless pool, though we might for a moment see its sides and floor paven with living blossoms, ivory white, rosy-red, orange and amethyst, yet all that panoply would melt away, furled into hollow rock, if we so much as dropped a pebble in to disturb the magic dream.

To be sure, naturalists had known of this vast and lovely province of nature since the days of Aristotle, that first great seashore observer, and there had always been collectors of such conspicuous things as sea shells and starfishes as well as fishers of crabs and cockles for the market place, but in all those hundreds of years between Aristotle and the early years of Victoria's reign there had been no general pillage of the seashore.

Today, however, there is no longer peace at the seashore, except, paradoxically, in areas which have been restricted in wartime. The "unravished bride of quietness" has been ravished by those very ones who profess to love her most, the nature lovers and the zoology students. Armed with handsomely illustrated books, buckets, bottles, and enamel pans, they descend upon the shore left undefended by the recessed tide, and scrape its creatures from their rocky homes and dig them out of their crevice refuges. Reefs which have been particularly favored by the complicated circumstances of time, tide, and temperature, have suffered the most, especially if they are near cities, and it is the perennial lament of the teacher who guides another ravaging horde to a choice "collecting ground" that the beach is no longer as it used to be ten, fifteen, or twenty years ago. Perhaps there has been, he suggests, a shift in the ocean currents, and the rarer creatures have been obliged to seek some more favorable spot.

It may be unfair to accuse any one person of bringing this lamentable state of affairs to pass, but if we trace the development of seashore study, at least as an amateur pastime, to its beginnings, the most influential name we find is that of Philip Henry Gosse. There are others, notably Louis Agassiz, who discovered the seashore shortly after his arrival in America in 1846, who did their share in promoting the study of the seashore, but it is Gosse who seems to have inspired the greater part of the host of dilettante and amateur collectors who have invaded the beaches in the past hundred years.

As his son Edmund tells us in *Father and Son,* that inimitable psychological "study of two temperaments":

> The fairy paradise has been violated, the exquisite product of centuries of natural selection has been crushed under the rough paw of well-meaning, idle-minded curiosity. That my Father, himself so reverent, so conservative, had by the popularity of his books acquired the direct responsibility for a calamity that he had never anticipated, became clear enough to him before many years had passed, and cost him great chagrin.

Philip Henry Gosse, the man who, in the words of the Reverend Charles Kingsley, himself the author of *Glaucus,* an immensely popular seashore book, did "more for the study of marine zoology than any other . . . man," happened to take up his calling of popularizing the seashore by accident. It was in 1843, just after Sir James Ross had returned from his Antarctic voyage, and when the theories of Edward Forbes about the distribution of life in the sea were much in the minds of professional naturalists. Gosse, then 33 years old and author of a not too successful book, *The Canadian Naturalist,* was engaged in writing a general zoology for the Society for Promoting Christian Knowledge when an illustrator named Whimper suggested that a book about the ocean might find a ready sale. The project appealed to Gosse, and some time in the following year he completed the book, and it

appeared in 1845 while he was in Jamaica on a collecting trip.

This book, *The Ocean,* is the first of that long series of volumes, many of them lavishly illustrated, on the wonders and mysteries of the deep. It is a rare secondhand bookstore that does not have two or three of these in stock—the recent ones, prepared for this more sophisticated age, cannot compare with those older volumes, crammed with woodcuts and fascinating stories of everything from the unfathomable mysteries of the great abysses to the elusive Flying Dutchman. Sea serpents, of course, received extensive mention. But it is vain to attempt to describe one of these wonderful books. The eager reader is advised to seek them out in the dustier recesses of a secondhand bookstore.

Gosse not only set a new style in books but also changed his own life, for he was the first person to be influenced by his own book. The interest in marine zoology he had acquired while preparing *The Ocean* led him to further studies, and in a few years he published his *Naturalist's Rambles on the Devonshire Coast* (1853), which is an account of the manifold attractions of the seashore, beautifully illustrated by his own drawings. Since the appearance of this book it has become the tradition for writers on seashore life to be their own illustrators, but in precision of detail and brilliance of color Gosse's illustrations for the *Devonshire Coast* have seldom been equaled in any work, whether it be a scholarly monograph or popular handbook. The book not only established Gosse's reputation as an interpreter of the life of the seashore, which was eventually to earn him an F. R. S. to attach to his name, but also it almost immediately started a new fad in parlor amusements, the marine aquarium. Perhaps it was this fad which had as much to do with the ravishing of the seashore, which his son was to deplore fifty years later, as the more conventional gathering of crabs and sea shells for the collector's cabinet. In one season alone Gosse himself collected more than 4,000 specimens for exhibition in his aquaria at museums. Gosse was also the first, at least in England, to hold natural-history classes on the seashore.

Of course, this interest in seashore life was not entirely inspired by Gosse's books and activities. Edward Forbes, the Manx naturalist, also was a familiar name to amateur naturalists through such works as his charming *History of British Starfishes* (1841) and his posthumously published *Natural History of the European Seas* (1859). But we remember Forbes primarily because of his great mistake, his theory that animal life did not exist in the ocean below 300 fathoms. This error, together with his

PHILIP H. AND EDMUND GOSSE
FROM EDMUND GOSSE'S BOOK *Father and Son.*

pioneer classification of marine life by zones, stimulated dredging activity by professional and amateur alike off the shores of Britain, Norway, and the eastern United States.

Although it would be difficult to prove such a supposition, it seems probable that Forbes and Gosse were more responsible for that first great oceanographic expedition, the voyage of the *Challenger,* than posterity has seen fit to acknowledge. Certainly these two men, as diverse in character and personality as it is possible to imagine, stimulated the large followings of British natural-

EDWARD FORBES
FROM WILSON & GEIKIE'S LIFE OF EDWARD FORBES.

ists which were to encourage and support the *Challenger* Expedition.

Edward Forbes was not so fortunate in his biographer as was Philip Henry Gosse, and as a result his personality has been lost in the arid bypaths of the history of science.[1] Of Gosse's almost pathological Calvinism, his fantastic efforts to reconcile his own literal interpretation of God's Word with the fossil record, as expressed in that odd book *Omphalos, or The Geological Knot Untied,* which so shocked Charles Kingsley that he protested that if anything could make him doubt God's wisdom it was that book—of these things, and what they meant to a sensitive, growing boy, we have a beautifully written record.[2]

Edward Forbes, on the other hand, was a completely charming character. Indeed, he was one of the most delightful personalities that ever graced the solemn halls of science. During those years when Charles Darwin was patiently studying his barnacles every morning and Thomas Henry Huxley was cruising off Australia in H.M.S. *Rattlesnake,* Edward Forbes was the great name on the roster of British naturalists. When anything was afoot, it was Forbes who was consulted, and his approval was eagerly sought by the younger naturalists, including Huxley. Gosse, on the other hand, was a lonely and solitary man, especially after the cool reception—or lack of reception altogether—accorded to his *Omphalos.* Friendship, for a man so beset by his own thorny theology, was difficult at best, and even his intimacy with the genial and easy-going Forbes was a short-lived affair.

Forbes never forgot that he was born on the Isle of Man. If he did not believe in the Little People, he was at least very considerate of their feelings. Gnomes, leprechauns, mermaids, and nymphs frequented the margins of his student notebooks and occupied conspicuous places in the chapter headings and tailpieces of his monograph on starfishes. During scientific meetings his long fingers were always busy drawing caricatures of the speakers or writing accounts of the meeting in humorous verse—with dubious rhymes—to be read later at his own Red Lions Club. His tall, thin frame was accentuated by long hair which hung loosely below his ears, and his deep-set eyes and broad mouth betrayed his Celtic ancestry. Forbes died in 1854, shortly after he had gained the chair of natural history at Edinburgh for which he had waited so long and impatiently. Though he was busy in his chosen field for most of his forty years, Forbes left no extensive monument of books and papers behind him. His influence was felt primarily through personal contact and lectures. His oft-expressed theory that the distribution of living animals and plants could best be understood by comparison with related fossil forms entitles him to an honored but seldom acknowledged place as one of the founding fathers of paleontology.

Forbes was less fortunate than Gosse in his family. Gosse, like Henry Moseley, the mathematician of sea shells and designer of battleships, whose son was H. N. Moseley, naturalist of the *Challenger,* and whose grandson was H. G. J. Moseley, the brilliant physicist who was killed at Gallipoli, and like Thomas H. Huxley, founded one of those three generation dynasties which have contributed so much to British science and culture. For Gosse's son Edmund, in spite of his being raised in a home where Shakespeare was frowned upon as frivolous, be-

came one of "the most understanding and sympathetic interpreters of literature," and his grandson Philip is a recognized authority on radium as well as the author of several books about piracy. Forbes had two children, but neither the *Dictionary of National Biography* nor those concerned with the later misadventures of Mrs. Forbes—including the usually exhaustive William Roughead—have found them worth mentioning. For in 1858, four years after the death of Professor Forbes, his widow contracted a marriage which, to put it mildly, was unfortunate, and the subsequent turmoil may have been the prime factor in denying a line of Forbeses to British science. The man Mrs. Forbes had the misfortune to marry was one William Charles Yelverton, who, it seems, had already informally married himself to a Miss Longworth under circumstances of questionable legality. During the ten years controversy over this marriage, which included debates in the House of Lords, poor Mrs. Forbes and her two children became the innocent and forgotten bystanders.[3]

Contrary to the impression which might be gained from a glance at the shelves of a well-stocked library, there have been relatively few good books on the seashore and marine natural history since Gosse's day. There has been no dearth of hack works, many of them charming in an unconscious way, and of serviceable manuals for amateurs, but within the rigid limits of the genre Gosse has had no successful imitators until the past twenty years. Two of the best are *The Seas,* by F. S. Russell and C. M. Yonge, and D. P. Wilson's *Life of the Shore and Shallow Sea.* Both of these are by members of the staff of the Marine Biological Association Laboratory at Plymouth. These two books satisfy the three essentials for a book of this type: that the author should be a recognized authority, that he should illustrate the book—at least in part—himself, and that it should be written in language accessible to the layman. A further desideratum, suggested by the influence these books have had on seashore collecting, might be that they should inspire respect for life and discourage idle collecting while at the same time arousing interest. Unfortunately, such sentiments are rarely expressed.[4]

Although the Woods Hole Marine Biological Laboratory antedates that of Plymouth by several years and has established an honorable tradition of marine research in America, no member of its staff has prepared anything comparable with the two recent English books. Some explanation for this may lie in the very size of the United States, its diverse ocean fronts, and the reluctance of commercial publishers to venture on projects which have only local interest, but none of these possibilities is altogether valid since at least two fine books, both of them about the Long Island shore, have been published in recent years. These are William Crowder's *A Naturalist at the Seashore* (1928) and H. J. Shannon's *The Book of the Seashore* (1933). The authors are both accomplished illustrators but are essentially amateur naturalists.

Only on the Pacific coast has a book appeared which might be considered to stand in the high tradition of English popularization. This is *Between Pacific Tides,* by Edward F. Ricketts and Jack Calvin (1938). Though intended as a handbook for seashore animals of the Pacific coast, its readable style and approach to the problem by zones instead of zoological classification raise it far above the usual status of a handbook. This book has a sort of sequel, *Sea of Cortez,* by John Steinbeck and Edward F. Ricketts, which is something else again. Many have found its narrative half too philosophical for their tastes or have objected to the philosophy on its own merits.

But it is not these books alone which send students and dilettantes to the seashore to continue that pillage which Gosse did so much to inspire less than a hundred years ago. Indeed, such books are probably not consulted until interest has already been aroused in some other manner. This has been more true, perhaps, in America than in England, where, despite the paucity of good seashore books, the coasts of Massachusetts and California have been as effectively raided as that of Devon, and the primeval beauty of the seashore is no more. It is no less a personage than Louis Agassiz who must be blamed for a large share of

the impetus which sent well-meaning despoilers to the shores of the North American Continent. Like that of Forbes in England, the influence of Agassiz has made itself felt through teaching and the inspiration of teachers rather than through the writing of books, and the precept "Study nature, not books," which he posted on the wall of America's first seaside station at Penikese in 1873, is still fresh in the minds of American naturalists (now called biologists, or worse still, ecologists), professional and amateur alike.

They have indeed studied nature instead of books, although some of them seem to have conducted their studies solely for the purposes of contributing rather dull and detailed monographs, if not the writing of books. Looking over the first hundred years of the books—seaside companions and vade mecums, as few as they are, one is tempted to deny that there is any need for newer and better books about the fascinations of the seashore and its creatures. The writer, himself an ardent student of one type of littoral creature, whose numbers he has persistently reduced on every occasion he has visited the beach, and furthermore the author of some popular articles which may have done their own small part to promote that same enthusiasm he now deplores, fully realizes that he is not without sin. Yet up-to-date manuals of seashore life are badly needed, even at the peril of further ransacking of the tidepools. Of course, it might be argued that an obsolescent manual or quaintly outdated book—for example, perhaps, one of Gosse's—stimulates more intensive collecting, and that a complete manual might eliminate the necessity of excessive collecting in the hope of finding uncatalogued rarities.

There seems to be no way out of this dilemma; certainly G. K. Chesterton wrote no truer paradox than "The man who is most likely to ruin the place he loves is exactly the man who loves it with a reason."[5] Such a thought could not have occurred to Aristotle on a visit to the shore in those remote golden days before Everyman became his own biologist, carrying a bucket to the beach, for Aristotle, while not much of a poet himself, had a taste for Homer and loved the sea with emotion as well as reason. The unkindest slur against that great man's name is the fable that he ended his life by throwing himself into the sea because he could not understand the currents in the Strait of Euripus.

We cannot expect a generation of Aristotles, but it does not seem too much to hope for that our biologists, readers and writers of books alike, will go down to the shore, not in the spirit of treasure hunters, but as poets in spirit and as students of living things instead of specimens in bottles, even if the Homeric phrase does not ring in their ears as it surely did in those of Aristotle:

παρὰ θῖνα πολυφλοίσβοιο θαλάσσης.

NOTES

[1] In addition to *Father and Son*, Edmund Gosse wrote an earlier, more formal biography of his father, *The Life of Philip Henry Gosse* (1890). The only biography of Forbes is *Memoir of Edward Forbes, F.R.S.*, by George Wilson and Archibald Geikie (1861). It suffers from being the work of two hands, neither of them skilled in biography.

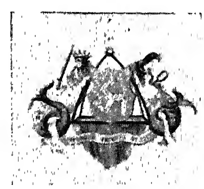
[2] It is not without its note of irony that Gosse, the author of *Omphalos*, a book which argued, among other things, that the fossils were created *in situ*, was also the originator—or most ardent proponent—of the theory that the sea serpent is a living *Plesiosaurus*.

[3] Cf. William Roughead, *The Evil That Men Do*, vol. 2. "The Law and Mrs. Yelverton," pp. 469–505 (Crime Club, 1929). Perhaps it should be mentioned that H. O. Forbes (1851–1932), author of *A Naturalist's Wanderings in the Eastern Archipelago*, was the son of the Reverend Alexander Forbes, and presumably no relation to Edward Forbes. Forbes's son was born in 1850, but we do not even have his name.

[4] The works of William Beebe, and Rachel L. Carson's *Under the Sea Wind* (1941), are not, strictly speaking, books about the seashore, nor are they illustrated by their authors. Miss Carson's book is further disqualified, by my standards, because it gives names to creatures after the manner of Thornton W. Burgess and is mired down in too many purple patches.

[5] G. K. Chesterton, *Orthodoxy*. Ch. V, "The Flag of the World."

The following vignette is from the title page of Forbes' *British Starfishes*.



In Triton's shell the echoing sea
is but the mirrored surge of sound
within the distant hallways of the ear,
and time the shadow of a thing unfound.

JOEL W. HEDGPETH

BRAZIL'S RESEARCH FOR INCREASED RUBBER PRODUCTION

By NORMAN BEKKEDAHL
CHEMIST, RUBBER SECTION, NATIONAL BUREAU OF STANDARDS, WASHINGTON, D. C.

In an effort to regain prominence as one of the principal rubber-producing nations of the world, Brazil, in the past few years, has been conducting research and making plans to increase her output of rubber. In the early days of natural rubber practically all of the world's supply came from her Amazon Valley. Unfortunately for that country, however, her natives at that time were making plenty of money by collecting latex from the wild rubber-trees of the jungle and processing it into the form of crude rubber, and thus took no interest in improving any of the processes or steps involved. High prices for the rubber and a lack of competition did not lead them to look into the future.

The British and the Dutch were quick to take advantage of this backwardness of the natives of the Amazon Valley. In the year 1876 Sir Henry Wickham gathered 70,000 seed of the Pará rubber tree, botanically known as *Hevea brasiliensis* Mull. Arg., from the banks of the Tapajós River, close to its outlet into the mighty Amazon. These seed, after germination in England, were transplanted in the Far East and became the beginning of all the rubber-growing industry of that part of the world which ultimately produced 97 percent of the world's supply of rubber.

In 1912 Brazil hit its high in production of rubber, over 40,000 long tons. This accounted roughly for 40 percent of the world's supply at that time. With the great increase in demand and supply which followed in later years, Brazil would have had to produce about ten times that quantity in order to hold the same percentage in 1938, the last normal year before the war. Instead she actually produced less than 15,000 tons. This decrease in production was caused largely by the fact that rubber produced from wild trees in Brazil by the primitive methods could not compete in price with rubber produced from the planted trees and the more modern processes developed and used in the Far East. The Ford Company started planting rubber trees in Brazil in 1929 but was unable to produce much rubber because of the unforeseen difficulties which will be described later in this article. The last few years, however, have seen an increase in the production of wild rubber in Brazil because of the higher price for natural rubber set by agreement. This price was high enough so that profits could again be realized even with the same old primitive processes used in the Valley over 50 years ago.

Brazilian authorities now realize that if Brazil is to become a large producer of rubber, she, also, must plant and cultivate trees instead of depending on the wild-trees of the jungle. If this is to be done there are many problems which must first be solved. It is true that most of the large amount of research performed on the growing and processing of rubber in the Far East will contribute considerably to the solution of problems of this hemisphere too, but because of different conditions much new work needs also to be done in Brazil.

The Hevea tree requires a warm, moist climate in order to thrive. In order to obtain a fairly good yield of latex, the tree must grow within about 10 or 15 degrees of the equator, either north or south. The Amazon Valley, which is the original home of the Pará rubber tree, therefore, has suitable climatic conditions. Over a very large area in the jungles of the Amazon Valley this tree grows wild but scattered. The number of the Hevea trees usually does not average more than two or three per acre.

In Brazil the Pará rubber tree is commonly known as the *seringueira,* and the native who taps the trees for latex and processes it into rubber is known as the *seringueiro.* This *seringueiro* must arise in the early morning hours each day of the week and make a tour of his path, or *estrada,* into the jungles, which has been routed so as to include as many rubber trees as is possible. From 50

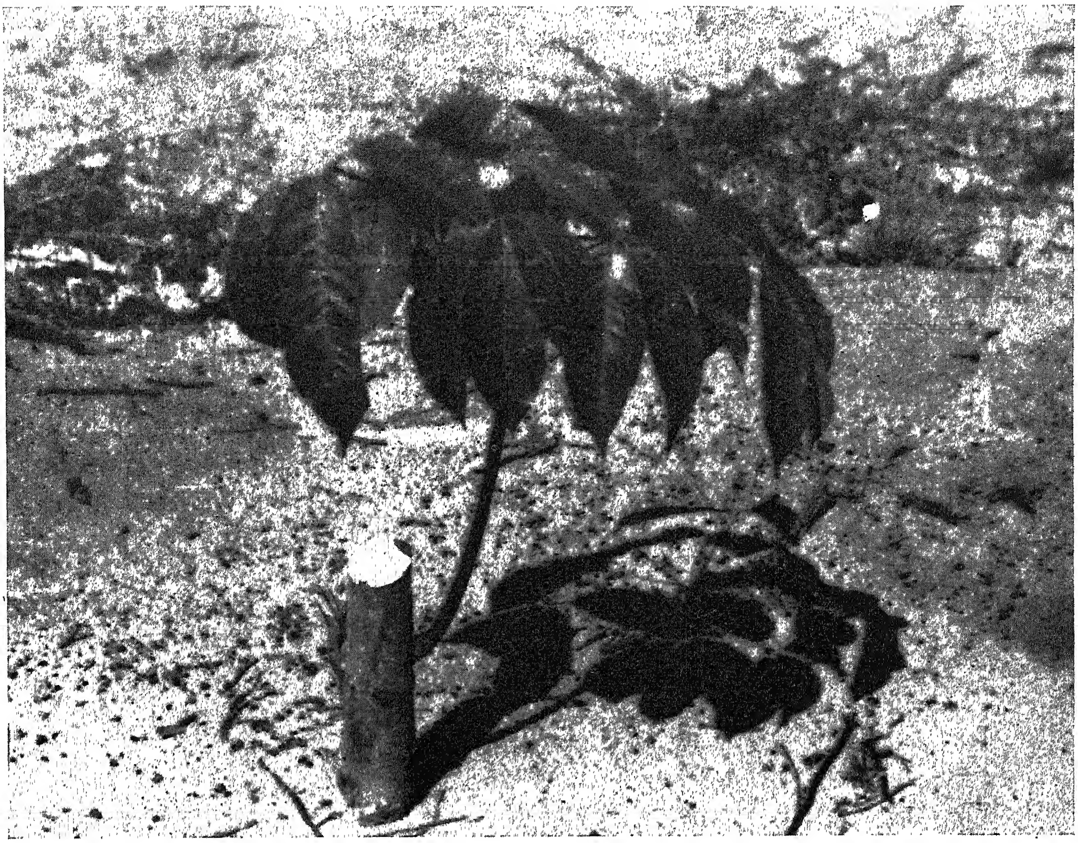
RUBBER SEEDLINGS
ONE END OF THE BOX IS TORN AWAY IN ORDER TO EXPOSE THE SPROUTED SEED AND THE ROOT SYSTEM.

to 100 trees is about all a *seringueiro* can take care of in one day. He makes several large, deep cuts into each tree by striking it with a large knife and then places a small cup under each cut in order to collect the latex which flows a drop at a time for several hours. After all his trees have been tapped in this manner he covers the same territory again, this time collecting the latex in a pail or other suitable container. The quantity of latex which he has obtained from all the trees of his *estrada* will probably not be more than one-half gallon, which will yield roughly only a pound and one-half of dry rubber.

All this tapping and collecting is done in the morning hours, and the afternoon is devoted to processing the latex into rubber. The oldest and still most common method used today in the Amazon Valley for separating the rubber from the latex is coagulation by means of smoke. The latex is poured over the end of a stick or paddle and is then held over a smoky fire which causes the latex to coagulate into a film of rubber. Then more latex is similarly added and this too is coagulated in the same manner. The process is continued until all the latex has been coagulated. The latex collected on subsequent days is also added until a large ball is formed. This large ball of wet rubber, which may weigh 100 pounds or more, is then removed from the paddle or stick and sold to a rubber buyer.

On the plantations of the Far East the processes of obtaining rubber from the trees are much easier. Instead of tramping through miles of thick jungle covering many acres of land in order to reach but a few trees, the tapper covers from 2 to 4 acres per day. Instead of tapping but 50 to 100 trees, he can easily take care of from 300 to 600 trees. He makes only one cut per tree by means of a special knife which he has been trained to use in a manner that will not damage the tree by cutting into the wood. In addition this type of cut yields more latex, and since it heals and the bark is renewed in a few years the same portion of the tree may be tapped again. The old system of tapping raises large growths on the tree and often kills it. The latex obtained on plantations is first strained in order to remove bark, sand, and other foreign material and is then placed in coagulating tanks where acetic or formic acid is added. On the following day the coagulum of rubber, which the acid has separated from the latex in a sponge-like slab, is passed between wringer-rolls and marking-rolls in order to squeeze out most of the water and to make the rubber into ribbed sheet form. The sheets are then placed inside a smokehouse for from 4 to 7 days, where they are dried and preserved by the smoke. These smoked sheets of rubber are much cleaner and drier than the rubber produced in form of balls by the old Brazilian method, and naturally demand a higher price. Much less work is involved in processing the rubber by plantation methods, and the production per man is much greater. Also on the newer plantations only improved and selected trees giving high yield of latex are planted.

It is quite possible that if the planting of rubber trees in Brazil is conducted in a scientific manner trees as good as or even better than those of the Far East may be produced. Brazil has the great advantage of a wider selection of starting material, which should permit development of a tree superior to any now known. In 1876 Sir Henry Wickham made his collection of *Hevea brasiliensis* seed from only one small area of the rubber producing region of the Amazon Valley and thus



A YOUNG RUBBER TREE SHORTLY AFTER BUDDING

THE BUD, WHICH WAS TAKEN FROM A HIGH-YIELDING RUBBER TREE, WILL FORM A NEW HIGH-YIELDING TRUNK.

did not secure material representing all the variations of that tree. In fact it is generally recognized that other regions of Brazil produce better strains. There is also a possibility that other Brazilian species of trees might produce even better quality rubber than Hevea.

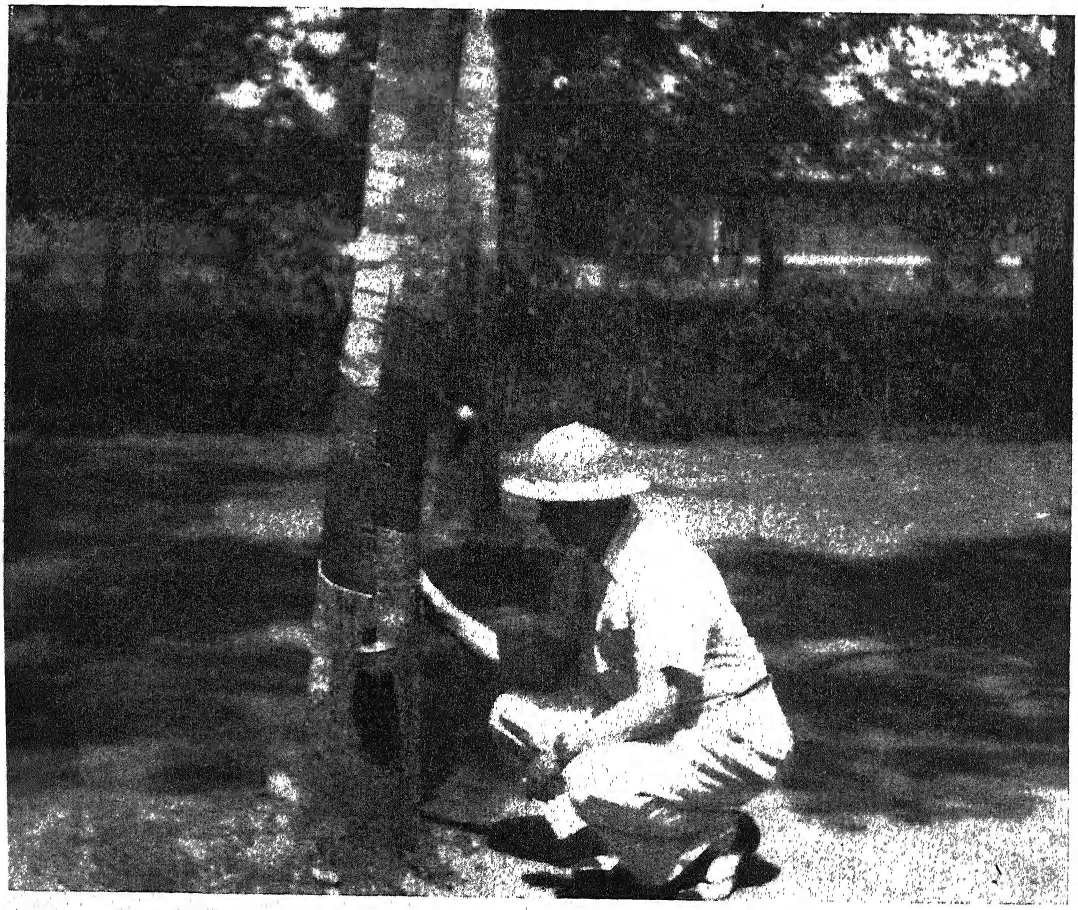
There are in the Amazon Valley several species of Hevea other than *Hevea brasiliensis* and many other trees and shrubs which yield a latex of one form or another. Most of these latexes produce a rubber-like material, varying greatly both in quality and quantity. Some of these trees and shrubs give up their latex more readily than do the others. Some of these latexes coagulate very easily, others with more difficulty. Some trees require many years after planting, while others only a short time, before they can be tapped commercially. Some trees grow easily, without requiring much care, while others are quite delicate. Some are more susceptible to diseases than others. In fact there are often great variations between different strains of the same species. Therefore it can be seen that what is needed is a thorough study of and comparison between the various rubber-producing plants, including a study of their susceptibility to diseases and how to control these diseases, a study to improve the yields of latexes and rubber, and a study to determine the different physical and chemical properties of the various kinds of rubber. The result of all these studies may prove that the solution is to produce various types of trees which yield rubbers having different characteristics.

Variations in the yield of latex among different trees from seed of the same tree may be very great, as much as tenfold and more. Studies were made on Hevea trees in the Far East, and budwood for grafting on the

poorer trees was selected from those which were known to produce a good yield of latex. By continuous budding and selection over a period of years, trees have been developed which produce much more latex than the average wild tree does.

Several plantations were started in tropical America at the beginning of the century, particularly in the British and Dutch Guianas and in Trinidad. These plantations were started on a rather large scale without any previous research or experimental work and ended in ruin owing to attacks upon the tree by a leaf-disease known as the South American leaf-blight, caused by the fungus *Dothidella ulei.* This blight had been known to exist on the rubber trees of the South American jungle, but separation of the wild rubber trees from one another by other types of trees and the heavy undergrowth of the jungle has limited spread of the fungus. However, when the trees are planted closer together, as in plantations, this disease reaches greater severity and the leaves of the attacked plants wilt and fall. Repeated defoliations from the blight cause the tree to decrease its yield in latex and finally to die. The trees are not "born" with the leaf-blight but acquire it through contact with the fungus, which is spread from infected trees by winds. The rubber trees of the Far East have not been affected by this blight since the disease has never appeared there. However, it is generally believed that if the blight once got started there, it would prove to be a pest and might even exterminate the present trees from that part of the world.

In order to prepare for the possible estab-



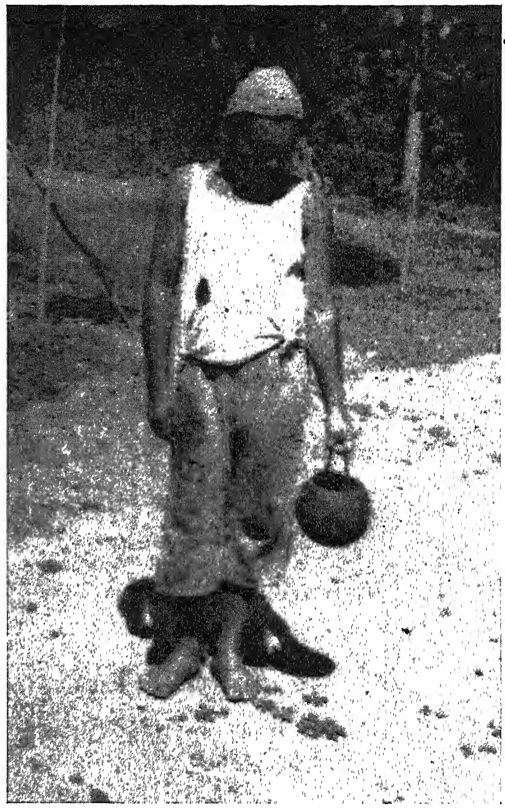
THE AUTHOR EXAMINING A RUBBER TREE

THIS TREE HAS BEEN TAPPED BY THE MODERN FULL-SPIRAL METHOD IN WHICH ABOUT A MILLIMETER OF BARK IS REMOVED AT EACH TAPPING. THE WHITE LATEX IN THE GROOVE RUNS DROPWISE INTO THE RECEIVING CUP.

lishment of rubber plantings in Brazil, a research institution was set up in 1941 at the mouth of the Amazon River near the city of Belém do Pará, and called the Instituto Agronômico do Norte (Agricultural Institute of the North). The object of this institution is to study all agricultural products of the Amazon Valley, rubber being one of the most important. It will therefore conduct research on botanical, pathological, entomological, physiological, technological, and economic problems connected with rubber. It also plans to establish an agricultural school in which will be taught subjects pertaining to agricultural science, with particular emphasis on those subjects important to the Amazon Valley. Its location at the mouth of the Amazon is ideal for a research center on natural rubber since it is surrounded by many different types of wild rubber trees, and since Belém do Pará is the port through which passes all the rubber produced in the Amazon Valley. In fact, the name Pará-rubber was derived from the name of this city.

The Instituto Agronômico do Norte, or I.A.N., began with 10,000 acres of land completely covered with dense jungle. At the present writing about 500 acres have been cleared. A campus of about 50 acres is the site for more than 50 buildings, which include those for administration, general chemistry, botany and pathology, rubber technology, school of technology, warehouses, shops, nurseries, hothouses, homes, etc. More than 100 acres of the cleared land are planted with rubber trees which are under scientific observation and experiment.

Since, as has already been mentioned, there are many different types and grades of rubber produced by the various species of trees in Brazil, one of the most important researches is the study of the physical and chemical properties of these various rubbers. Such a study requires a laboratory equipped with the type of apparatus and machinery employed in the research, testing, and development laboratories of the larger rubber manufacturing companies of the United States. This was realized by Dr. Felisberto C. De Camargo, who, immediately upon becoming director of the I.A.N., began making plans for such a laboratory. Since scientific



A TYPICAL SERINGUEIRO
THE CONTAINER, WHICH IS USED FOR COLLECTING THE LATEX, IS MADE BY HOLLOWING OUT A NATIVE FRUIT.

workers with the requisite training in rubber technology were not available in Brazil, it was considered desirable to seek the assistance of the United States, which was greatly interested in obtaining natural rubber from sources in its own hemisphere. Consequently, by means of negotiations between the governments of the two countries, an agreement was reached that temporary technical assistance would be furnished by the United States. This resulted in the loan of the author of this paper to the I.A.N from the Rubber Section of the National Bureau of Standards in Washington, D. C., and also of Mr. Fredrick L. Downs from the Rubber Laboratory of the American Steel and Wire Company in Worcester, Massachusetts. It was the duty of these two rubber technologists to select and purchase for the Brazilian Government all the equipment necessary for the Rubber Laboratory, to install it in a new building especially designed for the purpose,

COAGULATING RUBBER FROM LATEX
A SERINGUEIRO USES THE PÁ-AGRONÔMICO METHOD.

and to instruct and train a staff of Brazilian scientists and technicians so that they could take over the operation of the laboratory after the services of the North Americans had terminated. All the apparatus and machinery for the laboratory were purchased in the United States and, with the cooperation of the Rubber Development Corporation, shipped by boat to Belém. Practically all this transportation took place at various times during the year 1943 when ship-sinkings were very heavy due to enemy action, but fortunately not one single item was lost. In about June of 1944 the laboratory was completed, research was started, and analyses and tests of wild rubbers began to be reported.

The physical and chemical tests on the wild rubbers, although quite routine in themselves, not only define the properties of individual samples of rubber but also give data valuable for other purposes. From all these data which are being prepared at the I.A.N., together with similar data being obtained at the Rubber Section of the National Bureau of Standards, a method is being worked out by which all crude wild rubbers may better be evaluated and graded.

Not only is the laboratory suited for this routine testing but it is well-equipped for conducting research work on rubber. Its location near the rubber trees puts it in a position to do many types of experiments which cannot be performed in other laboratories. As an example, a rubber technician can actually go out into the "back yard" of his laboratory, tap a rubber tree, bring in the latex, and conduct experiments on the fresh unadulterated product of the tree. The latex from most rubber trees will putrify and coagulate in the course of a few hours unless some preservative has been added, which in many cases alters the results of the experiments. It is a great advantage to any laboratory when it has an unlimited quantity of pure, fresh, raw material with which to work.

The Rubber Laboratory of the I.A.N., in its short existence, has already performed several valuable experiments. It has perfected a method for the coagulation of the rubber from the latex of *Hancornia speciosa,* known in Brazil as the *mangabeira.* Many trees of this species are distributed in various parts of Brazil, but very few of them are tapped, partly because of the difficulty of coagulating the latex and partly because of the poor quality of the rubber. It is difficult to get a good coagulation of the *mangabeira* rubber from the latex by the usual smoking method. The coagulating agents usually used for the Hevea rubber, acetic or formic acids, do not function unless used in such high concentrations that the process is made impractical. The method which in the past has generally been used by the natives of Brazil to coagulate the *mangabeira* rubber is to add a solution of ordinary alum. This seemingly gives a nice coagulation, but leaves the resulting rubber with very poor aging qualities. A few months after production the rubber becomes very sticky and tacky, a property not very popular with manufacturers of rubber goods. The Rubber Laboratory has improved coagulation by adding to the latex, in place of alum, another latex from *Ficus anthelminthica,* called in Brazil the *caxinguba.* It was also discovered that very small quantities of ordinary hydro-

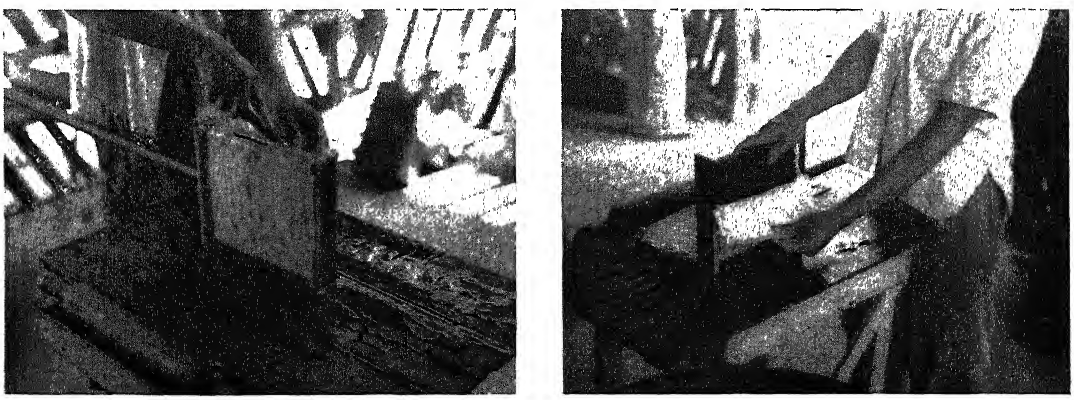
chloric acid form a satisfactory coagulum. Aging tests performed at the laboratory on rubber coagulated by various agents have shown that it is not the *mangabeira* rubber itself that naturally deteriorates rapidly, but that its poor aging qualities have been caused by the alum which had been used to coagulate it. Both accelerated aging tests and natural aging tests have shown that when *caxinguba* latex or hydrochloric acid are used as coagulants, the resulting *mangabeira* rubber is much superior to that which has been coagulated by alum.

The routine testing at the laboratory has shown that in some instances the practical rubber graders, who from necessity have been taught to make their tests on the rubber in the unvulcanized state without the aid of scientific equipment, do not always evaluate samples of rubber in the same order as do the scientific tests which are made both on the unvulcanized sample and the vulcanized product. Several types of rubber have been considered by the rubber graders as weak or unclassified, when the scientific tests have actually shown them to be of excellent quality, equivalent to the best grades of plantation rubber from the Far East. Thus in the grading of rubber alone, this laboratory located near the source of supply of rubber can be considered an asset to the rubber producers, to the manufacturers, and also to the consumers.

The Rubber Laboratory operates a small sheet-rubber factory located a few miles out in the jungle, where the rubber trees are more abundant. This factory is equipped to coagulate or process rubber by any of the various known methods. It processes an average of more than 100 liters of latex each day, and thus makes a very useful "pilot plant" for the laboratory to verify its experimental conclusions.

This rubber factory has studied various systems of coagulating and processing Hevea rubber and has developed a method which it recommends for use by the natives of the Amazon valley. It has been named the *Pá-Agronômico* method. *Pá* is the word for paddle in the Portuguese language. The term "*Agronômico*" is taken from the name of the institution, the I.A.N. This system of processing employs the old method of smoking the latex over a smoky fire but uses a specially designed wide paddle around which a band of rubber is coagulated. After from one to two liters of latex have been processed in this manner, the rubber is cut along one edge, peeled off in sheet form, run between rolls for better sheeting and marking, and then dried either in the shade, in a heated room, or in a smoke-house.

The processing of rubber from latex by the *Pá-Agronômico* method is a very slow process and is not recommended for use on plantations where large quantities of latex are obtained or on other establishments where trained help and coagulating acids or chemicals are available. But for the uneducated native or small-producer in the Amazon jungle, who generally has no knowledge of mixing acids or calculating their strengths,



STEPS IN THE PÁ–AGRONÔMICO METHOD

Left, CUTTING THE RUBBER ALONG ONE EDGE OF THE PADDLE; *right*, STRIPPING IT FROM THE PÁ-AGRONÔMICO.

the method has considerable advantage. It produces rubber in sheet form, which is more easily examined for dirt and other foreign matter than the ball rubber. The rubber sheets are dry and ready to go directly to the manufacturer, whereas the ball-rubber requires subsequent washing and drying. The best grades of *Pá-Agronômico* rubber in Brazil have been selling for the same price as the best smoked sheet.

Natives of the Amazon Valley having more than average ability and personality are sought by the I.A.N. to work in the rubber factory and are given a thorough training-course in the processing of rubber by the improved methods, including the tapping and care of the rubber trees. After they have become proficient they are sent to various substations of the I.A.N. or to other regions of the Amazon Valley where they may instruct their fellow-countrymen in the improved rubber-producing methods. The system of transferring information from one native to another is likely to be more successful than to send scientists into the field, since the natives usually resent being told by botanists, agronomists, chemists, and other "foreigners" how they should tap a tree and process the rubber. The native usually takes the attitude: "My father was a *seringueiro.* So was my grandfather. What right have you to tell me anything about my work?" The system of instruction adopted by the I.A.N. seems to be proving its worth. Sheets of *Pá-Agronômico* rubber are beginning to appear from different corners of the Amazon, but considerable time will still be necessary to cover such a vast region. As the cultivation of rubber becomes more general in the valley and the quantity of latex becomes too great to be handled by the older processing methods, the natives will be taught to use more efficient methods.

SQUEEZING WATER FROM THE RUBBER
THE COAGULUM IS ROLLED BY HAND BEFORE IT GOES THROUGH THE WRINGER CALLED SHEETING-ROLLS.

In the field of synthetic rubber, which has developed so rapidly in the United States within the last few years, the manufacturers have found that some types of synthetic rubber behave better than others for specific purposes. There is no single type which is better than all others for all purposes. Undoubtedly the same is true for the various types of natural rubber. In fact, before the war when all types of natural rubber were available, one well-known company which manufactures electrical cables always used *Up-River fine Pará,* a type of rubber produced in the upper Amazon Valley. The company claims that for this particular purpose the *Up-River fine Pará* is superior to any other rubber, including the best grade of plantation rubber of the Far East. Another well-known company which manufactures adhesives always insisted on using a rubber of different type from another section of Brazil. Thus it seems that with the many varieties of rubber that are at the present time produced from the different species of trees in Brazil, and with the unlimited number of new strains of trees that can be developed in the future, rubber products of a quality superior to that which we have known can be manufactured.

Brazil may have new species of rubber trees in the future which may prove to be more popular than *Hevea brasiliensis.* However, up to the present time the rubber from this tree has been studied more than any

other of the natural rubbers, its characteristics are better known, and it will undoubtedly play the most important role in natural rubber for at least a number of years. Thus, for the present at least, Brazil plans to plant *Hevea brasiliensis.*

The Ford Company began its plantation of *Hevea brasiliensis* in the valley of the Tapajós River in Brazil, and considerable loss was taken due to the south American leaf-disease. The company had grafted its Hevea seedlings with buds from clones obtained from various parts of the world and known to come from trees producing a high yield of latex. A very large percentage of these trees contracted the blight and threatened to die unless something was done. The Ford Company strained every effort to save its great investment. It began a series of various experiments to try to produce a tree resistant to the disease.

The greatest success came from crown-budding the high-yielding trees of the plantation with clones having high resistance to the disease. The Companhia Ford Industrial do Brasil (The Ford Rubber Plantation), the I.A.N. at Belém, and the U. S. Department of Agriculture have been working in cooperation both at Ford's plantation and at the I.A.N. on this method of producing a superior strain of the *Hevea brasiliensis.* The method involves the planting of a tree from seed which is known to give a strong and healthy root-system. After the tree is about one year old it is bud-grafted a few inches from the ground with the bud of a tree giving a high yield of latex. The old tree is cut off just above the newly-grafted bud, and the bud then grows and becomes the new tree. The result is a two-component tree, consisting of a root-system of the first and a stem and crown from the second. Since this crown is usually very susceptible to the leaf-disease, it must be sprayed with a fungicide at frequent intervals in order to keep the tree in good condition. The spraying method for control of the blight is effective and practical only when used in nurseries where the trees are small and close together. After this budded tree has grown for about a year and has attained a height of from 10 to 12 feet, it is top-budded (or crown-budded) about 6 to 7 feet from the ground with a clone known

SHEETING THE RUBBER

to have resistance to the blight. After this bud has "taken," the old crown is completely removed just above the newly-grafted bud. This procedure produces a three-component tree, built up from three different strains. After this new type of tree was developed, the question became, "What effect will the new crown have on the yield of latex from the tree?" Fortunately the answer was in the direction that the experimenters had hoped. The yield of latex, which is taken from the trunk of the tree, remained high, and the new crown maintained its resistance to the leaf-blight.

In all these first experiments on the double-budding system of producing a new tree, only strains of *Hevea brasiliensis* were used. The double-budding was naturally not expected to have any effect on the quality of the rubber since the tree was still purely of the one and same species. Since an extremely small percentage of *Hevea brasiliensis* trees are resistant to the leaf-disease, it would be convenient to top-bud with other resistant species also, such as with *Hevea benthamiana* or *Hevea spruciana.* These latter trees, however, are generally considered by rubber graders to produce a rubber which they classify as weak and inferior. For experimental purposes the Ford Company made a number of such buddings several years ago, and the trees have recently reached the tapping stage. Samples of rubber produced from these trees, together with samples produced from similar trees top-budded with

RUBBER FACTORY OF THE INSTITUTO AGRONÔMICO DO NORTE
IN THE MIDST OF THE JUNGLE, EXPERIMENTS ARE CONDUCTED ON THE PROCESSING OF LATEX INTO RUBBER.

resistant strains of *Hevea brasiliensis,* were tested in the Rubber Laboratory of the I.A.N. and it was found that the good qualities of the *Hevea brasiliensis* rubber were not lost by crowning the trees with the other species.

It is the belief at the I.A.N. that Brazil can now begin the cultivation of rubber and depend on the double-budding system to produce a good, strong, disease-resistant tree giving a good yield of latex and a rubber of good quality. This system is more costly than the single-budding method used in the Orient and other disease-free areas and will require about an extra year before the trees get into production. However, the plantings of Brazil will then consist entirely of trees budded for high yield. In this respect they would be superior to most of the present ones of the Far East, where only a rather small percentage of the rubber trees are of high-yielding types. The restriction scheme which has been practiced in the Far East over a number of years has prevented the rubber-growers from making large conversions to the higher-yielding clones.

In the Far East there are rubber plantations of all sizes, some consisting of thousands of acres. In addition there are thousands of families who have but a few acres of land each, part of which is planted in rubber trees, the rest being used to grow food-crops. It has been noticed that this latter type of rubber-grower can sell rubber at a lower price than the large plantations and can keep his rubber moving on to the market during periods of overproduction and low prices. Because of this stability the I.A.N. is recommending small one-family units for Brazil instead of large plantations. For ease of development it is planned to place a large number of these small units in the same locality.

The I.A.N. has for several years been producing budwood and budded stumps in its nurseries for future plantings. It now has sufficient quantity on hand to begin planting on a rather large scale in three widely differ-

ent localities. The object of planting in different regions is to try to spread rubber planting enthusiasm to different parts of the Valley and to determine the effects of different climates and soils on the trees. In this plan the Brazilian government will finance the planting of these regions. It will then assign the planted land to natives who show good intentions to continue the cultivation, each land-holder receiving about five acres on which are planted 400 or more rubber trees. Some of the land to be given him will be left free from trees and is intended for the farmer to grow food-crops sufficient for his family. He will be dependent to a large extent on the food-crops for support until the trees begin yielding rubber, which usually requires about 6 years. Until the rubber trees come into full bearing the government will give him a small salary and in addition a certain number of points for time served in cultivating the trees. After a given number of points have been accumulated, the land will be turned over to him. If he gives up his work before this time, he will lose his points and the opportunity to grow rubber.

Under this system a rubber-grower should collect and process about 2 tons or more of rubber per year from his few acres. At fifteen cents a pound for his rubber this would give him a minimum annual gross income of $600.00. This may seem to be a very small income to us, but it is actually very great as compared to what he has been receiving when producing in the neighborhood of one-tenth that quantity of rubber from the wild trees of the jungle. The work with the rubber would not occupy all of his time, so he would have ample opportunity to work in his vegetable and fruit gardens, to raise hogs, chickens, etc., and to fish. His necessary expenses would be small and consequently he could live on a much higher scale than he ever has in the past.

It is difficult to predict what the price of rubber will be after the world again returns to normal conditions. For several years previous to the war, natural rubber sold for about 20 cents per pound. At this price many rubber-growers of the Far East made large profits, and on a competitive market they can and probably will reduce the price of rubber considerably. The manufacturers of synthetic rubber are predicting that their production costs will be as low as 15 cents per pound after the war. If this prediction is correct, natural rubber may sell for approximately the same price. Consequently the quotation of 15 cents per pound for the natural rubber used in the calculation for the income of the Brazilian native rubber-grower seems reasonable.

Before this rubber program comes into its own, there are many obstacles which must be overcome. The first and most important is the unsatisfactory condition of health and sanitation in the Valley. Insect life thrives extremely well in such a climate, and unless steps are taken to improve sanitation and to eliminate the malaria-carrying mosquito and other harmful organisms, it will be difficult to induce outsiders to come into the Valley. The available manpower must be greatly increased in order to take care of the large rubber production program. Schools should be established to teach plantation methods, rubber technology, etc., to the natives of North Brazil. Industries operated by trained native technicians should be relatively stable. Imported technicians who are not accustomed to the tropics are less likely to remain indefinitely in the rubber-growing areas.

In conclusion, then, it might be stated that with a good system of rubber research and technical schools established in the Amazon Valley, Brazil should be able to restore rubber production as a farming enterprise and compete favorably in quality and price with both the natural rubber of the Far Eastern producers and with the synthetic rubber of the United States manufacturers.

SCIENCE AND PREJUDICE*

By GENE WELTFISH
INSTRUCTOR, DEPARTMENT OF ANTHROPOLOGY, COLUMBIA UNIVERSITY

In a sense, the title of this paper is anomalous. For science, being an instrumentality or tool, is neither prejudiced nor unprejudiced. It is therefore rather to the scientist that my remarks are directed.

The popular conception of science is that it is in the main disinterested. This commonly implies that since the scientist practices his techniques not in his own interest, but in the interest of somebody or something else, he can be free of emotional involvement in the outcome and is therefore unprejudiced. The history of science, however, is replete with instances that belie this supposition.

One of the difficulties with this popular notion is that it is derived from considering science as a species of gospel or revelation, divorced from the scientist. The fact that the scientist is a mere human being and that his science is merely an instrument for seeking the truth is little understood.

Much is to be said for the devotion of the scientist in perfecting his methods and techniques. On the whole, he does a good job in accordance with whatever facilities may be available at any given time; and whatever differences of opinion scientists may have on technical matters, their standards of objectivity in this field are very high. But this preoccupation often overshadows the scientist's consideration of the purposes for which his investigation is being carried out, and he often neglects to apply to the formulation of purposes and uses the same dispassionate standards that he applies to his technical methods. It is in this respect that science today is challenged.

In order to make the case concrete, I shall relate a story from the history of anthropology. During the war of 1870 a bombshell landed on the roof of the Natural History Museum in Paris. The director, the eminent anthropologist, de Quatrefages, was moved to remark that the Germans were congenital barbarians and had no appreciation of culture. He further implemented his remarks by writing a pamphlet called "The Prussian Race" (*La Race prussienne,* 1871), in which he stated that the Germans were not Teutonic at all, but were descended from the Finns, who were to be classed with the Lapps—all alien Mongolian intruders into Europe. A more muddy, irresponsible, and unscientific statement would be hard to find.

The German anthropologist, Virchow, replied by instituting an extensive government census of the color of the hair and eyes of six million German school children. In the process, many new refinements were developed for describing these physical features. But how this demonstrated their lack of descent from the Finns, and in turn from the Lapps, and in turn from the Mongols, no one troubled to ask. As long as the technical measurements were all right, all was well. It was also altogether too much to expect that someone would ask the question: If Germans are descended from Finns, Lapps, and Mongols, does *that* make them barbarians today? Are not all of us ultimately descended from barbarians?

In the meantime a rumor began to circulate among the peasant population in Posen that all Catholic children with black hair and blue eyes were to be sent out of the country, some said to Russia; others said that the king of Prussia had been playing cards with the sultan of Turkey, and had staked and lost 40,000 fair-haired, blue-eyed children; and there were Moors traveling about in covered carts to collect them; furthermore, the schoolmasters were helping them, for they were to get five dollars a head for every child they handed over. One can hardly feel that Virchow's reason for his investigation was much more valid than this.

As a result of the rumor parents kept their children away from school and hid them, and in the streets of the market town the children would cling to their parents in terror. The

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affair ended when a local school teacher told the parents that it was only children with blue hair and green eyes that were wanted.

In his *History of Anthropology,* in which this case is described, A. C. Haddon remarks that the results were of great service to the Science of Anthropometry.

In spite of its general comic aspect, this story has a number of serious implications. It reflects some basic attitudes of the scientist that carry over into the present day. Although Virchow and de Quatrefages were among the world's most eminent scientists, they did not hesitate to use their elaborate scientific techniques to implement a spiteful personal animosity, and to make assertions that were incapable of proof. Moreover, Virchow, in setting up a census involving six million children, felt no need to explain his purpose to their parents, even though they felt sufficient anxiety to invent a weird reason for his action. There was also a whole corps of scientific workers who likewise found it unnecessary to inform the public what the investigation was about, and there was also the school teacher who tried to allay the parents' fears by a fantastic description of the children that were reported to be wanted.

And, finally, the British anthropologist, Haddon, writing in the year of Hitler's rise to power, evaluates the incident by the effects it had upon the development of measuring techniques, without saying anything about the falsity of the initial premises and of what it added to the doctrine of racism. This is a typical attitude even today and reflects considerable discredit on the fundamental morality of the scientist.

Now let us transport ourselves through time to this war and to our own era. It was in Germany, in 1942, that Scientist X was given a problem to work on. He was to perfect a method of human extermination that would render the bodies easily disposable and the whole affair was to be quick and efficient. Scientist X went to work with admirable method and efficiency and produced a gas whose effects would be best achieved if the exterminees took a shower beforehand. Scientist Y planned an efficient incinerator, etc. He need have no qualms as the exterminees were, of course, worthless human civilians, mainly Jews and agitators; or perhaps he didn't ask—simply went to work with his customary scientific accuracy; his scientific integrity was beyond reproach; he neither made blunders nor sabotaged his technique.

It is not hard to see how the attitudes in incident A can shade into incident B. Would it ever be possible for a Nazi regime to flourish if scientists did not take orders and cooperate? There is no doubt that many of the scientists acted under duress—just as the Russian and Czech prisoners were forced to enter into actual combat along with the regular German army. However, there should be a dishonor roll of scientists who collaborated willingly, such as Alexis Carrel, who last autumn was apprehended by the French people.

I am certainly not trying to imply that scientists are to blame for this war. I am only trying to demonstrate that scientists, like the rest of our contemporary population, are sadly lacking in the fundamental values that would have made such a war as this im-

THE SCIENTIST'S OATH

I pledge that I will use my knowledge for the good of humanity and against the destructive forces of the world and the ruthless intent of men; and that I will work together with my fellow scientists of whatever nation, creed, or color, for these, our common ends.

possible. But unlike the rest of the people, the scientist can escape responsibility for his lack of values by the all-too-familiar ascent into the ivory tower—which is simply another way of saying that he can escape into the intricacies of his techniques and methods.

Some years ago, while attending commencement exercises, I heard the medical graduates recite the Hippocratic Oath. I was impressed by its solemnity and also by the fact that this was the only group of graduates that was asked to express any public responsibility in the exercise of its future profession. We well know that not all physicians are strictly faithful to the implied ideals of the code. But it is also a fact that such practices, when exposed, are subject to considerable public censure.

The scientist, on the other hand, does not have the same kind of check upon his actions. On the contrary, his freedom from public responsibility is condoned and considered to be his right. This, despite the fact that science has far-reaching social consequences—considerably greater than the actions of the individual physician.

In the face of many facts, I maintain that the disinterestedness of the scientist is largely mythical—that it amounts to a lack of evaluation of purposes, and that as a consequence, the scientist can readily become the creator of havoc and destruction. I further maintain that such a robot scientist is a greater menace to humanity than the robot bomb. In our reconstructed world graduating scientists should take cognizance of their responsibilities for the social consequences resulting from their use of scientific techniques. To make them conscious of their obligations I propose that each new group of graduates take a solemn oath, for example:

I pledge that I will use my knowledge for the good of humanity and against the destructive forces of the world and the ruthless intent of men; and that I will work together with my fellow scientists of whatever country, creed, or color, for these, our common ends.

DR. GENE WELTFISH

Gene Weltfish, Ph.D., is probably most widely known as coauthor with Professor Ruth Benedict of a pamphlet entitled *Races of Mankind,* which was published in 1943 by the Public Affairs Committee to combat race prejudice. Since 1936 Miss Weltfish has been an instructor in the Department of Anthropology of Columbia University. Our heroine has been a New Yorker since her birth in 1902. She was educated at Barnard College and took her doctorate in anthropology at Columbia. An atypical New Yorker, she knew that the Indians do not live in Indiana; from 1928 to 1935 she lived among a number of American Indian Tribes in Oklahoma, New Mexico, Arizona, Louisiana, and North Dakota; investigated their arts, manufactures, poetry, language, social and economic life; and made a number of collections for the American Museum of Natural History and the Reading Public Museum in Pennsylvania. She arranged materials for exhibit in several museums. Her activities of recent years are explained by her social conscience. She writes: "About six years ago I became convinced by the course of world events that the scientist was unjustified in maintaining an 'ivory tower' isolation and since that time I have been working on the race problem and the popularization of anthropological material in the schools and other fields. I think the scientist should do something about society *before* the buzz-bombs fall on his laboratory." So we find Dr. Weltfish lecturing in the East and Middle West for churches, schools, and community organizations; serving on the editorial board of the *Journal of Social Issues,* on the executive committee of the A.A.Sc.W., as Executive Secretary of the One World Citizens League, and as Honorary Chairman of the Citizens Committee of the Upper West Side. We hope that our readers will take her Scientist's Oath to heart and "give the little lady a hand."

SCIENCE AND DIETARY WISDOM

By SAMUEL BRODY

DEPARTMENT OF DAIRY HUSBANDRY, UNIVERSITY OF MISSOURI, COLUMBIA

The destiny of a people depends on the nature of its diet.—A. BRILLAT-SAVARIN.

THE haunting, seductive radio voices on behalf of various food products, vitamins, and laxatives sensitize scientists as well as laymen to the problem of dietary selection. Food rationing, educational efforts of various agencies in behalf of the "basic seven" foods, victory gardens, and related dietary problems add to the general food consciousness. Sociologists, nutritionists, public health workers, and physicians are brooding over the Stiebeling, Kruse, and related surveys, which indicate by comparison with nutritional standards that most of our citizenry is ill-nourished. While only 10 to 15 percent show clinical symptoms, 50 to 70 percent are said to have subclinical nutritional disorders, to suffer from decaying teeth, bleeding gums, digestive disturbances, sore tongue, fissured mucous membranes, and various nervous and psychic manifestations such as vague aches and pains, fatigue, insomnia, and apprehensive fears. Conclusions from these surveys are substantiated by Selective Service reports indicating that 30 to 40 percent of our men in the prime of life are unfit for military training. It is said that the majority of those disqualified have preventable nutritional defects. Soil-nutritionists contribute to this somber picture by suggesting a parallel between the deterioration of beast and man in our present dust-bowls and the decline of past civilizations owing to soil depletion. Then, too, there is a growing humanitarian concern about the world food scarcity which this country is asked to relieve because "the world cannot long exist in peace if half of its people are well-fed and the rest ill-fed" (Mattill).

The writer is particularly interested in the effects of growing science and technology on human wisdom. Some of the general aspects of such effects have been discussed in the September 1944 issue of this magazine. The present essay extends this general discussion to a particular problem—the relation of rapidly growing science and technology to dietary choice advantageous to long-range survival. Such selection of food leading to optimal health and longevity of individual and species is here called *dietary wisdom.*

IT IS generally known that dietary selection in modern man is not always motivated by biologic wisdom or homeostatic mechanisms as it is in subhuman species or in primitive man; instead biologically irrelevant traditions, fashions, prejudices, prestige symbolisms, and advertising often govern choice of food. Curiously enough, some of the most nutritious foods have the lowest prestige values. For instance, the most nutritious parts of grain are the germ, middling, and bran, which are generally removed in milling and "shunted into the mouths of chickens, cattle, and hogs" (Carlson). The most nutritious parts of the animal carcass are often the visceral organs, many of which are discarded as offal. Much of the skim milk is fed to chickens and hogs and even dumped into sewers, although it is the most nutritious part of milk as it contains all the milk proteins, minerals, water-soluble vitamins, the uniquely important milk sugar, and half of the original milk energy. The failure to utilize all the skim milk and whey for human consumption results in unbelievably large food losses, said to be enough to supply the protein needs of perhaps 35 million persons, riboflavin needs of 65 million, and calcium needs of 80 million (Krauss); the protein loss in skim milk is said to be equivalent to that in 20 million beef steers (Rose).

Yellow corn is nutritionally superior to white corn, but the poor Southern whites prefer the white varieties because yellow corn is "nigger food." Sharecroppers near river banks avoid eating river fish because "fish is eaten only by them river rats" (Bennett), referring to the "river people" whom the sharecroppers consider as social inferiors. The peanut is an excellent food, but it is said

not to be eaten by man where it is grown most luxuriantly because pigs eat it there; it is "pig feed." On the other hand, many canned and packaged foods are highly esteemed by the rural poor because of their high social prestige value; they are "city foods" and brightly packaged. White flour, starch, and sugar have high social prestige value also because they used to be expensive and, therefore, were consumed only by the rich. Their present popularity is due to such factors as traditional prestige, taste to which most consumers have been conditioned, superior keeping qualities, and preconceived notions concerning the inherent desirability of "sweetness," "purity," "whiteness," in food. A major reason for the popularity of these denatured foods is, perhaps, a merchandising one. Their sale and consumption are promoted because they are particularly suitable for large-scale manufacture, storage, and distribution.

For reasons of religious and related traditional symbolisms pork, shellfish, lobsters, and crabs are not *kosher* to orthodox Jews; for similar reasons, Moslems do not eat pork, Hindus do not eat beef, and Bantus do not eat fowl. White North Americans do not eat the meat of horses, dogs, cats, rats, alligators, snakes, sails, and insects, although some peoples eat such animals.

Fish eggs and fish in various forms—pickled, smoked, dried—are dietary staples in much of Europe and Asia, but relatively little is consumed in this country, although, on a dry basis, fish has approximately the same nutritional value as meat and is obtained by the fishing industry at a manpower cost of about a tenth that of meat. After the oil is removed, most of the commercial ocean catch in this country is used for animal feed and fertilizer.

These examples indicate that foods are often chosen not because of their nutritional value or availability but because of uniquely human conditioning to symbolisms or prejudices which do not complicate the lives of subhuman species and because of merchandising factors which make it more convenient or more profitable to market certain food products.

Dietary selection has become particularly confused by the extremely rapid development of technology of food processing. Our dietary-selective mechanisms were not evolved for wise discriminations between recently developed processed foods, especially when artificially flavored, colored, and promoted by high-pressure advertising. For these reasons the rapid development of science and technology does not necessarily improve the nutritional condition of the average consumer. Indeed, Dr. Russell Wilder, an eminent dietary authority in a famous clinic, expressed the belief that malnutrition is more serious now than it has been at any time in our history just because of the perfection of certain food technologies (as the manufacturing and wide consumption of patent flour, starch, sugar and sugar products). The science of nutrition has furnished powerful tools to the physician skilled in dietary therapeutics but it has aggravated the nutritional hazards of the average man. It has been said that God provides the remedy before creating a disease. This is true of the two-sided effect of rapidly-growing nutritional science.

It may be recalled that nutrition, like reproduction, is not a new thing under the sun; that it existed before the appearance of a brain. Therefore, under natural conditions, dietary selection is a process that is largely automatic even in the highest organisms. For instance, there is no dietary choice during the period of most significant growth and development; namely, during the prenatal and infantile periods. During prenatal life the embryo and fetus must subsist on the nutrients provided by the egg or uterus; during the infantile period, the mammal must subsist on milk and the bird on "crop milk" or equivalent nutriment.

Following infancy, the strict carnivore normally subsists on a given animal species that it was evolved to consume. The herbivore has a wider range of choice, and it has instinctive preferences for nutritious and aversions to injurious plants. Herbivores also exhibit such discriminatory phenomena as search for salt licks to supplement the salts in which herbage is relatively poor. During pregnancy and lactation when there is increased need for minerals, especially for calcium and phosphate, or when there is

dietary deficiency in the mineral content of food plants owing to poor soil, herbivores automatically search for calcium- and phosphorus-rich bone, limestone, and earth. In search for these special nutrients, the animal may be confused and mistake poisonous lead-containing paint, for example, for lime-containing whitewash, just as man may be confused between the choice of artificially colored or flavored unwholesome food and a naturally colored and flavored wholesome food. Pregnant women, likewise, often develop special dietary cravings, but they are usually confused concerning their nutritional needs by the conditioning of their tastes to processed foods and related substances.

Omnivores have the widest feeding range and the greatest discriminatory powers in dietary selection. For instance, Richter observed that rats maintain their nutritional condition relatively normal despite experimental designs to upset normality. They maintain a normal sodium salt level despite great increase in salt loss following extirpation of the adrenal glands, provided that salt is furnished in a separate container; they maintain a normal calcium level despite great increase in calcium loss following the extirpation of the parathyroid glands, provided that calcium is furnished in a separate container. The loss of sodium salt or calcium salt is reflected by heightened hunger and appetite for the corresponding nutrient so that the surgically induced losses are compensated by extra nutrient intake. Indeed, Richter suggested that taste thresholds may serve as delicate indices of nutritional deficiencies and of disease of metabolism-regulating glands. Rats thus show excellent dietary wisdom in selecting the needed nutrients if given opportunity to do so by the cafeteria feeding style and if not confused by conditioned habits or by synthetic flavors or odors. It is emphasized that the force of strongly conditioned habit—in rat or man—is often greater than the force of inherited behavior patterns. Man is a product of environment as well as of heredity.

Another homeostatic mechanism compensating for some dietary deficiency is the hypertrophy of the gland regulating the metabolism of the given deficient nutrient. Critical iodine and calcium deficiency leads to hypertrophy, respectively, of the thyroid and parathyroid glands; excessive carbohydrate consumption leads to hypertrophy of the islets of Langerhans that produce extra insulin required for the extra carbohydrate metabolism. The glandular hypertrophy compensates in part for the dietary deficiency or excess.

What was said about endocrine influence on the adjustive choice of foods or nutrients often holds for vitamins and minerals. For instance, withholding thiamine from the diet shifts the rat's dietary preference from carbohydrates and proteins, the oxidation of which involves thiamine, to fat, the oxidation of which does not involve thiamine. When thiamine is added, the rat's preference is reversed to carbohydrate and protein.

Much of what was said about the rat's ability to choose foods wisely holds for other omnivores, including unconditioned pigs (Evvard), chicks (Funk), and children (Davis, Sweet). As regards herbivores, Dove reported that rabbits show excellent discrimination in choosing the most nutritious herbage, and Nevans observed that dairy cattle choose wisely of their natural herbage diet but that their judgment is blurred with respect to concentrates (processed) feeds for which their discriminatory mechanisms were not evolved.

One is tempted to cite in conclusion the remarkable dietary wisdom in the honeybee. Its social life is conducted on a caste system consisting of one queen and many worker bees. The large fertile queen is genetically identical with the worker bee; she attains her royal status by dietary means alone, by being fed "royal jelly"—a secretion produced by the nurse worker bees—in the larval stage, while the larvae of the future worker bees are not so fed. This is demonstrated by the fact that commercial queen raisers produce queens from any larva by simply feeding it royal jelly from hatching. In this connection one wonders how the better diets usually enjoyed by the children of the well-to-do differentiate them from underprivileged children not so well fed. R. F. Harrell (Teachers College, Columbia University) reported increased learning ability in orphanage children receiving a vitamin supplement.

The worker bee differs greatly from the queen bee, but perhaps relatively no more than the human cretin differs from the average normal human being—and it appears that both types of differences are produced by dietary chance. The congenital human cretin in iodine-deficient regions is apparently the product of a dietary chance circumstance—severe iodine and perhaps thyroxin deficiency in the mother during pregnancy. Other congenital abnormalities may likewise be due to dietary deficiencies in the mother during pregnancy. For instance, congenital blindness, missing kidneys, missing limbs, cleft palate, hare lip, and other abnormalities were apparently produced in calves (Moore), pigs (Hale; Zilva, Golding, Drummond, and Coward), and rats (Warkany) by withholding vitamin A (and also vitamin B_2 in rats) from the pregnant mother's diet. Richardson and Hogan observed about a dozen cases of hydrocephalus—characterized by a great skull with little brain—in new-born rats from mothers fed a "synthetic diet" complete in all the *known* dietary constituents. Deficiency of some *unknown* essential dietary factor may account for this abnormality because it was not observed on rations which contained *crude* vitamin carriers. May not some related disorders, such as some types of spastic paralysis, result from dietary deficiency during pregnancy? Faulty teeth structure is often attributed to deficiencies in dietary minerals and vitamins D and C in the pregnant and lactating mother. Sherman observed that under the given conditions white bread sufficed to nourish a first generation of rats but not successive generations. Similar cumulative nutritional deterioration may occur in human society.

Where is the dividing line between automatic and conscious dietary wisdom? The enlargement of the thyroids and parathyroids under conditions of iodine and calcium deficiency is clearly automatic. But how much conscious selection is there in the rat's greater intake of table salt following adrenalectomy or greater intake of calcium salt and less intake of phosphate salt following parathyroidectomy? Or how explain the rat's preference for fat and aversion to carbohydrate and protein on a diet deficient in vitamin B? Richter reported that cutting the rat's taste nerves abolishes his ability to make the beneficial dietary selections. The taste buds are the rat's essential "dietary wisdom" components. What are the corresponding dietary wisdom components in man, whose taste buds are often unreliable guides to food value because of conditioning by tradition and confusion by food processing and advertising? Should man adopt the science of nutrition rather than taste and symbolisms as the safer guide for dietary selection? If so, what is the status of the science of nutrition and how reliable is it as a nutritional guide?

The founding of the science of nutrition is attributed to Lavoisier, who, together with Laplace, observed, about 165 years ago, that animal heat was derived from the oxidation of the body's fuels as candle heat is derived from the oxidation of candle fuel. He, therefore, concluded about 1777 that life—and nutrition is its basic process—is a chemical function: "*La vie est une fonction chimique.*"

It is true, as Lavoisier concluded, that the energy for maintaining the body machinery is derived from oxidation of body fuels. But there are uniquely biological factors in animal oxidation not encountered in a burning candle. For instance, the oxidation—the burning—of a candle is accomplished directly by raising the temperature of its wick end far above that compatible with life. The fires of life, on the other hand, glow at a very much lower temperature than that of a candle, and the oxidation is not directly by combination of the fuel with oxygen but stepwise by way of a long chain of intermediate biochemical reactions, each of which is controlled by one of an interrelated system of biocatalysts; that is, enzymes and hormones of various categories.

These reactions, unlike those in a candle, are reversible. For instance, the oxidation of body sugar—glucose—is not a simple combination of sugar with oxygen to form water and carbon dioxide as it occurs on burning sugar outside the body. First, much of the body sugar exists in starchlike glycogen. When the glucose in the blood declines below a certain level, the glycogen is depolymerized; not by simple hydrolysis, however, as starch is hydrolyzed *in vitro,* but by combi-

nation with phosphoric acid to form the Cori-ester, glucose-1-phosphate. The formation of this ester, like that of the many subsequent intermediate compounds, is under the influence of a specific enzyme—in this case under the influence of the enzyme phosphorylase—and this reaction is reversible. Many other intermediate compounds, including hexose diphosphate, triose phosphate, glycerophosphoric acid, phosphoglyceric acid, pyruvic acid, lactic acid, methyl glyoxal, glycuronic acid, glyceraldehyde, acetaldehyde, dihydroxyacetone, are similarly formed under the influence of specific enzymes during oxidation. Deficiency of a specific biocatalyst for the oxidation of the corresponding intermediate metabolic product may block the oxidation process at the given reaction stage with the accumulation of the given intermediate product and thus lead to dietary deficiency intoxications and disease, such as beriberi, pellagra, and alcoholism.

The biocatalysts, or enzymes, are composed of many components, including vitamins, such as thiamine, certain inorganic elements, such as iron, and proteins or amino acids. For instance, the enzyme carboxylase, which catalyzes the oxidation of pyruvic acid (one of the intermediate compounds in carbohydrate and protein oxidation), is a diphosphothiamine-magnesium-protein. The newer science of nutrition, which emphasizes the vitamins, minerals, amino acids, enzymes and coenzymes, and hormones, is mostly a product of the present century.

A historical review of the vitamins could begin with any one of many investigations and investigators. Following 1880, when rice polishing methods were perfected and polished-rice consumption became widespread in Japan and in the East Indies, beriberi developed there in epidemic proportions. This stimulated Eijkmann in Batavia, Takaki in Japan, and others to investigate the cause of this disease. Eijkmann, in 1897, fed polished rice to chickens, which promptly developed the nervous disease polyneuritis, similar to beriberi in man subsisting on polished rice. He explained the polyneuritis and beriberi, in harmony with the then prevailing general concept, by postulating that disease-inducing toxin was produced in the intestine of polished-rice consumers. Shortly thereafter a new concept appeared—that disease may be produced not only by the presence of harmful factors, poisons, and germs but also by the absence of indispensable factors. For instance, in 1905 Pekelharing, on the basis of observations on mice fed purified protein, fat, and carbohydrates, concluded that "there is a substance which even in very small quantities is of paramount importance to nutrition." In 1906 and 1912 Hopkins, on the basis of observations on rats fed purified protein, fat, and carbohydrates, concluded that the then known purified nutrients were deficient in "accessory food substances," which were present in milk. Osborne and Mendel reported similar results about 1911. Incidentally, the 1929 Nobel Prize in medicine was awarded to Eijkmann and Hopkins for these pioneer dietary experiments.

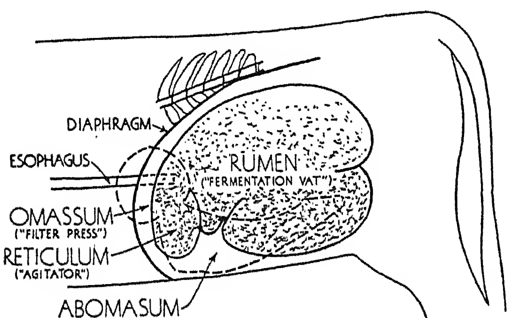
In 1911 Funk, then a 27-year-old Polish student at the Lister Institute in London, boldly formulated the theory that polyneuritis, beriberi, pellagra, rickets, and scurvy result from deficiency in the diet of specific chemical entities which he named vitamins. Funk, indeed, prepared a potent extract from rice polishings which cured polyneuritis in chickens fed polished rice. This extract is now known to have been rich in thiamine, nicotinic acid (identified by Funk who believed but did not prove that it was a dietary essential), and in other B-complex vitamins and minerals.

During the same period, E. B. Vedder and R. R. Williams in the Philippines were attempting to cure beriberi with extracts from rice polishings—attempts which led to the triumphant thiamine synthesis in 1936 by one of these workers, Williams, together with Cline.

In the meantime, beginning about 1907–09 (results published in 1911–15) Osborne and Mendel at Yale University and McCollum and associates at the University of Wisconsin employed purified nutrients in rat-feeding experiments, the results of which confirmed the preceding investigations that the then known purified nutrients were nutritionally inadequate and led to the discovery in milk of the fat-soluble vitamin A (McCollum and Davis, 1913; Osborne and Mendel, 1913) and a water-soluble vitamin B (McCollum and

Davis, 1915), now known as the B-complex vitamins. The essential nature of certain amino acids in the diet was likewise established during this period (Wilcock and Hopkins, 1907; Osborne and Mendel, 1911, and subsequent papers). The extremely widespread research interest in the "newer knowledge" of nutrition began with this very recent period.

The newer nutritional researches by thousands of investigators led to the classification of nutrients into four broad categories: substances yielding energy (including carbohydrates, fats, and proteins), amino acids (lumped under the heading "protein"),



—*Modified from A. F. Schalk and R. S. Amadon, N. Dak. Agr. Exp. Sta. Bull. 216, 1928.*

FIG. 1. SYNTHESIS IN VIVO

THE MICROBIOLOGICAL SYNTHESIS OF B-COMPLEX VITAMINS AND AMINO ACIDS IN THE RUMEN OF CATTLE MAKES THIS SPECIES INDEPENDENT OF DIETARY B-COMPLEX VITAMINS AND AMINO ACIDS AND ADDS TO ITS REMARKABLE EFFICIENCY AS A PRODUCER OF ONE OF THE MOST COMPLETE FOODS, COW'S MILK.

minerals, and vitamins. These four categories are known to include a half-hundred-odd distinct nutrients — including 22 known amino acids, 13 known mineral elements, 15 isolated vitamins, and an essential unsaturated fatty acid (linoleic acid, or linolenic acid, or arachidonic acid). New dietary entities, particularly B-complex vitamins and minerals, are being continuously discovered, which means that our dietary knowledge is incomplete, that we are not able to prepare a nutritionally complete "synthetic diet."

An important recent development is that species differ in their dietary vitamin needs. For instance, vitamin C is apparently a dietary essential only for primates and guinea pigs. The other species investigated seem normally to synthesize this vitamin within the body, although not always to the optimal level. For the latter vitamin C is a *nutritional* essential, not a *dietary* essential; it is a hormone or enzyme component.

The vitamins of the B-complex, including thiamine, riboflavin, niacin, pyridoxine, pantothenic acid, choline, biotin, the "folic acid" complex (including vitamin B_c, etc.), inositol, *p*-aminobenzoic acid, appear to be identical with the microbiologist's "growth factors," mostly members of "bios." About 1860 Pasteur observed that yeast required some factor for growth which Wildier in 1901 called "bios," later found by many investigators to include about a dozen distinct "growth factors." The recently-discovered B-complex vitamins are identical with the long-known microbiologist's bios factors, or growth factors, and are constituents of or associated with oxidation catalysts or enzyme systems in organisms ranging from bacteria to man. This means that there is an identity in oxidative mechanisms and nutritional (but not dietary) need in diverse organisms, ranging from bacteria to man. This identity of oxidative mechanisms in diversity of organisms is of great biological, chemical, and philosophical significance.

While the B-complex vitamins and amino acids enter into the nutritive processes of all organisms, some can synthesize them either directly as all green plants and most microorganisms do, or indirectly, as cattle do. Normally, cattle are not dependent on dietary B-complex vitamins and amino acids, not because they can synthesize them but because cattle—and apparently other ruminants—have in the rumen chamber of their stomach (Fig. 1) what is virtually a fermentation vat, with flourishing populations of microorganisms which synthesize the B-complex vitamins, especially in the presence of soluble carbohydrates. These microorganisms and their synthetic products are, in turn, used as food by the host organism. However, even some nonruminants, especially those with a large functional cecum, harbor microorganisms which synthesize to some extent B vitamins and amino acids. Even some human beings have good biosynthetic intestinal flora (Najjar and Holt). The nutritional condition of the animal is thus depen-

dent directly on the diet and indirectly on the nutrients synthesized by the microorganisms in the alimentary tract. Methods will probably be developed to make the rat or even man relatively independent of dietary B-complex vitamins and of some amino acids by developing foods which would stimulate maximal development of the most desirable biosynthetic microorganisms in their alimentary tracts.

The ideal diet, then, is one which includes not only nutrients needed by the animal but also nutrients needed by the symbiotic microorganisms in the alimentary tract. It is generally known that a high milk diet promotes a different intestinal flora than, for example, a high egg or meat or vegetable diet, with corresponding differences in biosynthetic activities. Dogs, for instance, are independent of dietary nicotinic acid, provided they are on a high milk diet which promotes niacin synthesis in the alimentary tract (Elvehjem). The lactose and, perhaps, to a less extent the butterfat in the milk favor the intestinal organisms that synthesize niacin, riboflavin, and other vitamins. The old idea of Metchnikoff concerning the profound influence of intestinal microorganisms on health and longevity may be substantiated, but more in the sense of nutrient biosynthesis than toxin inhibition.

The function of oil-soluble vitamins—A, D, E, K—differ in being, perhaps, more specialized than the water-soluble B-vitamins and not universally present in living organisms. Vitamin K is liberally synthesized by intestinal microorganisms so that, normally, it is not a dietary essential. The vitamin A precursor carotene is synthesized plentifully by most colored plants, which are the normal sources of vitamin A for herbivores and omnivores. Carnivores obtain vitamin A mostly from the livers of their victims, and omnivores may similarly obtain it. Vitamin E is also widely distributed in higher plants especially in the germ oil of wheat and other grains. Such microorganisms as yeast, rich in the B-vitamins, are probably devoid of and do not need the oil-soluble vitamins. As examples of the functions of the specialized oil-soluble vitamins: vitamin D increases calcium absorption and accelerates bone construction, and vitamin A is an essential constituent of the light-sensitive part of the eye, especially in night vision. Early vitamin A deficiency may be recognized by the slowness of adjustment of vision on entering a darkened room. However, vitamin A may also become a limiting factor in growth, reproduction, integrity of epithelial tissues (which serve as a barrier to infection), and development of teeth.

As already explained, plants and many microorganisms, and ruminants by way of rumen microorganisms, synthesize amino acids from such simple inorganic nitrogen compounds as urea, nitrates, and ammonium compounds. But rats and man do not, normally, synthesize what are called "essential amino acids." Of the 22 known dietary amino acids, 9 or 10 are dietary essentials for the rat—and probably for man—that is, they are physiologically needed but they are not synthesized by the body or by microorganisms in the alimentary canal in sufficient amounts. Of the remaining, 3 or 4 are partly dietary essential (they are synthesized, but too slowly for adequacy) and 9 are dietary nonessentials (they are synthesized in adequate amounts in the body proper or by microorganisms in the gut).

The nutritional value of a protein to nonruminants with simple stomachs, such as man, thus depends not only on its digestibility but also on its content of essential and partly essential amino acids. Since different proteins contain different proportions of the essential amino acids, proteins from various sources may supplement each other's deficiencies. For instance, cereals are poor in the amino acids lysin and tryptophane, while milk, egg, and meat are relatively rich in these amino acids. Hence, while different cereals do not correct each other's deficiencies, milk, egg, meat, and to a less extent, yeast, and some legume seeds, especially soybeans, do correct the amino acid deficiencies of cereals.

Of the thirteen mineral nutrients known to be essential (Ca, P, Mg, Na, K, Cl, S, Fe, Cu, Mn, Zn, I, Co) some are used for structural purposes, such as calcium and phosphate salts for bone; others are constituents of enzymes or hormones, such as iron in hemoglobin and cytochrome, magnesium in carboxylase, zinc in carbonic anhydrase, and

iodine in thyroxine; still others, including sodium, potassium, magnesium, and calcium, are involved in the regulation of physiological conditions, such as osmotic pressure, cell permeability and membrane potentials, colloidal condition, irritability of nerves, and contractibility of muscle. There is no dividing line between these functions. For instance, the phosphate radical serves as a structural constituent of bone and also of enzymes.

DIETARY deficiency of vitamins, minerals, or amino acids which function as essential parts in oxidative catalysts may be reflected in a wide variety of symptoms ranging from subclinical dermatitis and vaguely felt shifting pains to serious nervous and psychic disturbances as observed in advanced pellagrins and alcoholics. Pellagra may, indeed, begin with dermatitis, progress to insanity, and end with death. As previously noted, Eijkmann's classic experiments in feeding polished rice to chickens resulted in a severe nervous disturbance, polyneuritis. Why is the nervous system particularly affected by deficiency of vitamins and trace elements?

The basic fact is, of course, that nervous tissue is unable to function or even to maintain its integrity without continuous energy supply. This may be simply demonstrated by shutting off the oxygen supply and, therefore, the energy supply since energy is obtained by oxidation. The oxygen supply may be shut off suddenly by simple choking or by centrifugal drainage of the blood from the brain as currently experienced in fliers' "black out" or by inhaling carbon monoxide or hydrogen cyanide which combine with an oxygen carrier, hemoglobin in the first case and cytochrome in the second case. The extreme rapidity of onset of a "black out" is generally known. The effect of carbon monoxide, and especially hydrogen cyanide, inhalation is of the same order of rapidity (diving animals, such as whales, have special mechanisms for oxygen storage). The available oxygen may be reduced more slowly by dietary deficiencies, for example, by deficiency of iron, which is a constituent of hemoglobin and cytochrome, or by deficiency of thiamine, a constituent of the oxidizing enzyme carboxylase. The nervous and psychic symptoms associated with severe iron or thiamine deficiency are, then, similar in principle to those associated with decrease in oxygen supply by other methods, but the effects develop more slowly. Moreover, as previously noted, oxidation in the body occurs stepwise, and each step is catalyzed by a specific enzyme. For instance, if thiamine is lacking, the enzyme carboxylase is lacking, and the oxidation is blocked at the pyruvic acid stage. Consequently, there is not only a reduction in energy supply but also an accumulation of, and intoxication by, pyruvic acid and apparently other products of intermediate metabolism.

Under such conditions Zimmerman and others observed myelin and axon degeneration in the peripheral nerves and spinal cord and hemorrhage in the brain nuclei. The resulting disease may be polyneuritis in birds; beriberi, Wernicke's syndrome or Korsakoff's syndrome (otherwise known as alcoholic polyneuritis and whiskey paralysis) in man; Chastek's paralysis in foxes. Beriberi occurs among polished-rice consumers since the thiamine which is in the polishing is discarded in the milling process. Wernicke's and Korsakoff's syndrome may occur in chronic alcoholics, since alcohol is devoid of thiamine, and excessive alcohol consumption depresses the appetite for normal thiamine-containing food. Excessive consumption of alcohol or, for that matter, excessive consumption of any energy-rich but vitamin- and mineral-poor food may lead to dietary-deficiency diseases. Chastek's paralysis in foxes follows the feeding of raw fish which contains a thiamine-destroying enzyme.

The nervous system is the most sensitive to the absence of carbohydrate-oxidation catalysts not only because of its dependence on continuous energy supply but also because, as observed by Himwich, other tissues can use fat as energy source but the brain cannot.

Similar statements might be made concerning the effects of other mineral and vitamin deficiencies, but the function of thiamine is typical of the behavior of most other clinically important vitamins and minerals which function in oxidation processes. Indeed, the dietary-deficiency disease pellagra, often characterized by severe nervous and psychic

as well as dermal disturbances, is believed to be due to deficiency of several vitamins and perhaps some minerals and essential amino acids, although nicotinic acid is apparently the major deficiency.

Incidentally, psychic disturbances, such as mental confusion and disorientation may be similar, regardless of the causative factor. For instance, similar symptoms may be seen in severe anemia, poisoning, deficiency of thiamine, nicotinic acid, or other oxidative vitamins, severe infection, circulatory failure, and even in some cases of purely "functional" mental disorders. Man, characterized by a high level of neuropsychologic integration, naturally responds sensitively by psychic reactions to many causative agents, including nutritional deficiencies, of which deficiency of thiamine and nicotinic acid, as seen in beriberi and pellagra, are best known and most dramatic.

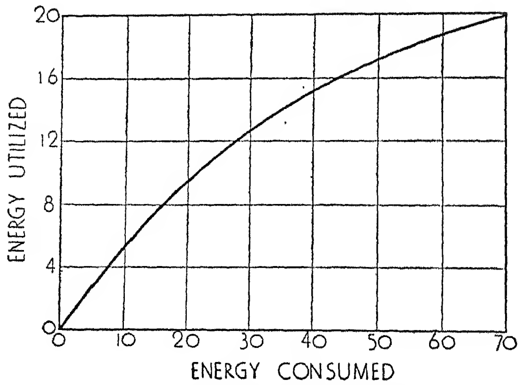
Vitamin A deficiency may also lead to disturbances of the nervous system but sometimes for different reasons. Wolbach and Bessey observed that vitamin A deficiency in the young retards the growth rate of the bony skeleton but not of the nervous system. The result is that the would-be normal growth of the nervous system is cramped by the retarded growth of the bony frame, with consequent damage to the nervous tissue. This may explain how animals may be born eyeless when their pregnant mothers are on a diet deficient in vitamin A. Under condition of vitamin A deficiency Moore observed constriction of the optic foramen in calves with consequent constriction of the optic nerve and blindness, and as previously noted, Warkany and others observed similar congenital malformations in other species.

The above discussion illustrates the fact that different dietary deficiencies and different basic causes may result in similar clinical symptoms; and depending on conditions, the same dietary deficiency may result in different symptoms. For instance, perosis, a disease of chickens, may be developed by any one of five dietary deficiencies: manganese, biotin, choline, and, according to unpublished observations by Hogan and Richardson, perhaps also by deficiency of vitamin B_c and nicotinic acid. On the other hand, deficiency of vitamin E may cause widely different effects, ranging from impairment of fertility to muscular dystrophy and, in chicks, to a serious disease of the nervous system, particularly the brain. The same may be said about deficiency of other vitamins: vitamin A deficiency may lead to night blindness, conjunctivitis, or to total blindness (severe xerophthalmia or keratomalacia is common in South India) and it may cause sterility (degeneration of the germinal epithelium); pantothenic acid deficiency may lead to diseases ranging from dermatitis to cessation of growth; choline deficiency may lead to many disturbances including perosis in chicks, impairment of lactation, fatty liver, and hemorrhages of kidneys in the rat.

As previously noted, the effects of vitamin deficiency in man are known only for 6 or 7 of the 15 isolated vitamins: A, D, thiamine, riboflavin, ascorbic acid, nicotinic acid, and biotin. This problem is, moreover, confused by sparing, synergistic, supplementary, and antagonistic interrelations so that a change in the intake of one nutrient has a variable influence, depending on circumstances, on the effects of the others. For instance, dietary thiamine requirement is dependent on bacterial synthesis in the gut, which is, in turn, dependent on the fat and riboflavin content in the diet. The dietary manganese requirement is related to the thiamine intake. Pantothenic acid is similarly interrelated with riboflavin.

To cite endocrine interrelations, an overactive thyroid accelerates enormously the breakdown of all nutrients so that hyperthyroid individuals tend to suffer from all dietary deficiencies, including vitamins and minerals. On the other hand, vitamins A and C are said to counteract the effects of an overactive thyroid and of excessive thyroxin. The parathyroid hormone, vitamin D, and dietary calcium are also interrelated: animals without parathyroids may live normally but only on a very high calcium and vitamin D diet. A dramatic effect of a vitamin deficiency on endocrine function follows the elimination of vitamin E from the diet of the male rat; the germinal epithelium degenerates with consequent permanent sterility.

Another factor which confuses nutritional experiments is the effect of the quantity of food intake on food need. The less the food



—Based on a compilation of data by Forbes, by Mitchell, and by Wiegner on cattle and on rabbits.

FIG. 2. EFFICIENCY OF FOOD USE

THE FOOD UTILIZATION PER UNIT FOOD INCREMENT DECREASES WITH INCREASING FOOD INTAKE AS INDICATED BY THE WAY THIS CURVE FLATTENS OUT WITH INCREASING FOOD CONSUMPTION. IN OTHER WORDS, THE LESS THE FOOD INTAKE (WITHIN LIMITS) THE MORE EFFICIENTLY THE FOOD IS UTILIZED. FOR HUMAN NUTRITION THE CURVE IS NOT NECESSARILY THE SAME.

intake the less, within limits, the food needed. This interrelation is extremely important in permitting survival under adverse conditions, such as now prevail in the famine-ravaged war areas. The mechanism of this interrelation is relatively simple as regards dietary energy. Decrease in dietary energy is followed by loss in body weight, and the lighter the body the less is the energy required for its maintenance. Then, too, as illustrated in Figure 2, the less the food intake the higher its utilization efficiency. A striking illustration thereof in human nutrition in relation to dietary energy need was furnished by Benedict during the first World War when he reported that decreasing a man's body weight by about 15 percent may enable him to maintain the lighter body on nearly half the original dietary energy intake. Another illustration, by Mitchell and others, relates to utilization efficiency of dietary calcium. For instance, a child on a liberal calcium diet utilizes, perhaps, 30 percent of the calcium intake while one on a very low calcium diet may utilize 90 percent of the calcium intake; an adult on a liberal calcium diet needs about 10 mg. of calcium per kilo. of body weight, but one on a very low calcium diet may attain calcium equilibrium on 3 or 4 mg. of calcium per kilo. of body weight. The higher calcium utilization on a low calcium diet may be due to using over and over the calcium "waste," as occurs with the re-utilization of iron on a low iron diet. There is, however, a limit of re-using body waste. For instance, if the dietary energy or protein is reduced below a certain critical value, the body protein may be used for fuel, the blood protein may be reduced to an abnormally low level, and the serious condition called "famine edema" or "war edema" may develop.

An overt method of re-using body wastes for dietary purposes, one which gives a lot of trouble in vitamin research, is illustrated by experimental animals on deficient diets; they then resort very skillfully to coprophagy in an effort to supplement the deficient diet, despite the experimenter's ingenious efforts to prevent such dietary supplementation from feces. (Feces are normally rich in vitamins and amino acids synthesized by intestinal microorganisms and derived from the body proper, also in minerals, because calcium, iron, and many other salts are normally excreted by the body into the bowel. Indeed, cattle manure is used as a tonic for chickens and swine presumably because of its high vitamin and mineral content, and it is generally known that on many stock farms swine follow the cattle and chickens follow the swine in friendly symbiosis.)

The homeostatic mechanisms which serve to adapt the animal to dietary scarcity are useful from the survival viewpoint, although they may involve serious dangers to health. One may survive at least for a while and reproduce while suffering seriously from many dietary disabilities, ranging from loose teeth and constipation to insanity. The ideal, however, is the most advantageous long-range survival, what Sherman calls "buoyant health," which may often be attained by simple dietary means.

ARE there really simple dietary means for eliminating malnutrition on a national scale? If so, why is there such a high incidence of malnutrition? Are we wanting in the good will and patriotism required for adopting a simple means for abolishing malnutrition? These are problems of capital religious and national importance in peace as well as in

war. The answer thereto is like the one given in the author's essay (SM, September 1944) concerning maladjustments associated with the rapid development of science and technology. One may, perhaps, suggest again that since scientists are in a sense responsible for disturbed conditions associated with scientific and technological progress, they should properly assume leadership to promote the best use of their specialized knowledge, to humanize it and give it a social orientation, and to attempt to "make reason and the will of God prevail" (Matthew Arnold).

A simple solution comes to mind for our national malnutritional problem, which results from excessive consumption of denatured foods. The idea is not novel. Grocery stores sell inexpensive dietary-complete dog foods. Why not similarly inexpensive dietary-complete human foods?

It is suggested that a nationally available attractive and inexpensive milk-bread properly fortified with vitamins and minerals and perhaps garnished with such a food as raisins would eliminate malnutrition on a national scale. The reasons for suggesting milk-bread as a medium for a nutritionally complete food are: first, wheat and milk are the least expensive and most abundant foods of plant and animal origin—they provide the greatest amount of food per unit of agricultural resource in this country; second, whole wheat and whole milk in certain proportions constitute an optimal diet for rats (Sherman), and it is reasonable to assume that a wheat-milk bread, if fortified with vitamin C and other nutrients required in relatively greater proportions, would also be an optimal diet for man; finally, everyone eats bread anyway, especially the underprivileged who are the largest bread consumers and who are the special concern of research in national dietary welfare. Since the wealthy are said to obtain only about 15 percent of their dietary energy from wheat, it is less essential that their bread or cake be of good nutritional quality because they get their superior proteins, vitamins, and minerals from more expensive foods, such as milk, butter, cheese, meat, egg, fresh vegetables, and fruits. The underprivileged, on the other hand, omit the expensive "protective" items and concentrate on bread, starch, and sugar, which contribute, perhaps, 75 percent of the dietary energy. Members of the middle class are said to obtain over 50 percent of their dietary energy from white flour, starch, and sugar; that is, from foods which are seriously deficient in vitamins, minerals, and amino acids.

Other conditions being equal, whole-wheat flour and whole milk would be best for the proposed dietary-complete bread. They are natural foods in the sense that man has consumed them as they are in the course of his evolution. Nutritionists admit that they are not yet able to replace naturally evolved foods, such as milk and whole grain, by "synthetic" diets. The concept of "purity" has its place, but from the nutritional viewpoint such pure foods as sugar, starch, and patent flour are greatly inferior to whole-wheat flour with all its bran, middling, and germ "impurities." As Carlson has said, "nutritional safety lies in consuming, so far as possible, foods in their natural states." It is significant that the bread of Russian soldiers is made of whole-grain flour.

There is, moreover, every reason for believing that whole wheat is better utilized and is more valuable nutritionally than are the several wheat constituents when consumed separately and that, likewise, whole milk has a higher nutritive value than the sum of its constituents consumed separately. Furthermore, the nutritive value of whole wheat and whole milk when consumed together is greater than the sum of the nutritive values of each, milk and wheat, consumed separately. The various constituents are said to *supplement* each other, each supplying what the other lacks, thus enabling more efficient utilization of both.

White patent flour is inferior to whole-wheat flour because the milling process removes virtually all the vitamins, trace elements, and fiber. The parts discarded in milling—the germ, middling, and bran—carry with them most of the B-complex vitamins, minerals, and biologically superior amino acids, which are usually fed to cattle. As previously explained, cattle do not need dietary B-complex vitamins and amino acids and they obtain minerals from their basic roughage ration. Thus, the vitamins, amino acids, and minerals are taken from man who needs them and fed to cattle that do not need

them. This is certainly not a good illustration of human dietary wisdom. The following are examples of losses incurred in the patent milling process: 100 percent each of carotene, manganese, fiber, vitamin E; 85 percent each of thiamine, nicotinic acid, iron; 80 percent of ash; 75 percent each of copper, riboflavin, phosphorus, potassium; 50 percent each of calcium, pyridoxine, pantothenic acid, fat; 13 percent of the protein, and the amino acid relations of the remaining protein are seriously disturbed. There are, of course, many other losses.

The tenacious hold of the white flour products is due in part, as already explained, to its traditional or symbolic high prestige value; in part to its superior *culinary* qualities despite its very inferior *nutritional* properties; but mostly to its superior keeping qualities—it has no vitamins and minerals to support insect pests that infest whole-wheat flour, no oil to get rancid, and no protein-digesting enzymes.

It is, indeed, these superior keeping qualities of white flour and of other denatured cereal products that made possible the concentration of the milling industry into a few circumscribed areas that supply flour to the whole country. Whole-wheat flour manufacture, on the other hand, would have to be decentralized to the immediate neighborhoods of the bakeries, and the wheat grinding would have to be done shortly before baking. Advocating whole-wheat bread thus amounts to opposing a powerful industrial trend in a giant industry. This industrial trend is probably the underlying reason for the modern popularization of white bread and related cereal products by special sales techniques. This is a good illustration of how technical developments may exert unwholesome effects on general welfare. With the exception of oatmeal, which is virtually a whole grain, most of the packaged cereal products are nutritionally inferior to, although much more costly than, the original grains from which they were obtained.

"Enriched" white flour is merely flour from which two dozen odd nutrients are removed and to which only four nutrients—iron, thiamine, riboflavin, and niacin—are added to the level of the original grain. To characterize a seriously impoverished food as "enriched" is in a sense misleading. Even the short-time rat-feeding experiments by Morgan, Chick, Mitchell, McHenry, and others demonstrate that "enriched" white flour is no substitute for whole-wheat flour. The effects would have been much more striking if the rats had received this diet through several generations.

The white flour deficiencies are made good by the other foods in the expensive diets of the well-to-do, but not in the diets of the less privileged who lack the means for obtaining the protective foods. Many consumers, too many, attempt to make good the dietary deficiencies of their white flour and sugar diet by the use of expensive and perhaps harmful drugstore laxatives, vitamins, minerals, and so on. Even if we could—but we cannot—scientifically supplement the white-flour deficiencies with drugstore products, would it not be slightly absurd to remove the vitamins, minerals, and the laxative parts of the grain and then purchase them anew at many times the cost of the original constituents, especially in the case of those who cannot afford them? It is such considerations that suggest the great superiority of natural foods such as whole wheat and whole milk. Whole-wheat flour would supply energy, protein, B-complex vitamins, minerals, and bulk; milk would supply calcium, biologically superior amino acids, fat-soluble vitamins, and related substances deficient in wheat. No doubt there will be devised in the near future a stable ascorbic acid which when added to the mix would make the whole-wheat-whole-milk bread nutritionally complete for man. This bread could be further fortified with all other vitamins and minerals to such a level that when it supplies a third of the dietary calories, it would assure the full day's quota of the known vitamins and minerals, thus definitely eliminating, as far as humanly possible, nutritional deficiencies associated with the excessive consumption of white flour, sugar, and related highly purified—i.e., unnatural——forms of dietary energy.

Making available on a national scale the whole-wheat-whole-milk bread in the proportion of 80 percent whole-wheat flour to the equivalent of 20 percent whole-milk powder and fortified with a stable ascorbic acid and other vitamins as above described would be

the nutritional ideal. This ideal is not, however, likely to be adopted in the near future for the reasons given, especially because of the tendency for whole-wheat flour and powdered whole milk to spoil readily and because of the relatively high cost of whole milk, which is prized as a beverage and its butterfat as a spread. Since the whole-wheat-whole-milk bread ideal cannot, apparently, be achieved in the near future, the problem becomes one of finding a suitable substitute. The best substitute for whole milk is skim milk. While skim milk contains half of the energy of whole milk, it has all the water-soluble vitamins, minerals, protein, and lactose of milk. Consequently, *per unit energy,* skim milk is twice as rich as whole milk in the water-soluble vitamins and in the mineral and protein factors, and it is also fairly rich in choline. Thus, is it an ideal supplement to wheat flour in bread making.

Powdered skim milk is also very economical; prior to the war it sold at six cents a pound, not much above that of flour; so that replacing 2 ounces or 20 percent of the flour in a pound loaf of bread by skim-milk powder would not change appreciably the price of the loaf. Indeed, prior to the war bakers incorporated 6 percent of powdered skim milk in their "milk bread" without increasing its price. They did it partly because it greatly improved the yeast action with consequent production of a large loaf per unit weight of flour. Increasing the powdered milk to 20 percent of the flour improves the taste of the bread (to the writer's palate) but increases the density of the loaf because of decrease in gluten, which holds the air. The 20 percent milk-bread has more of the substantial solidity of "home-made bread" rather than the more aerial qualities of bakery bread.

If it is not practicable to use whole-wheat flour (I think it is; methods are being developed for permanent storage of perishable foods), it might be possible to use part whole-wheat flour and part white flour; or it might be possible to use flour containing 85 percent of the kernel instead of the 72 percent as is customary in white flour; or the usual white flour could be used, but the parts discarded in milling could be replaced by physiologically-equivalent supplements. For instance, about a half-billion pounds of wheat and corn germ are produced yearly and fed to livestock; similarly, large quantities of powdered brewer's yeast are produced as by-product of the brewing industry as well as directly for the drug trade. One or more of these could be added to the extent of 2 percent of the flour. Other products such as soy bean meal, linseed meal, peanut meal, tender dehydrated grass (cerophyll), middlings, or bran could be added in various amounts to the flour.

The problem is not one for an individual to solve. The National Research Council might appoint a special committee representing the milling industry, the dairy industry, the bakery industry, the consumer, the public health profession (it is a public health problem), the medical profession (it is a medical problem), the Surgeon-General (it is a military problem), the advertising profession (it is a publicity problem), as well as professional nutritionists, to develop a nutritionally complete bread to furnish all the needed "protective nutrients" and "bulkage" when the bread is consumed to yield a third of the dietary energy.

The national adoption of the proposed nutritionally complete bread, perhaps nationally subsidized and nationally advertised, might virtually eliminate the malnutritional toll, just as the incidence of infectious diseases has been materially reduced by national health measures and as the incidence of goiter has been reduced by adding iodide to table salt on a national scale. The prayer "Give us this day our daily bread" will then become scientifically as well as religiously meaningful.

(*For story of Britain's National Bread, see next page.*)

BRITAIN'S NATIONAL BREAD IN BALANCED NUTRITION

The history of wheat bread in Britain during the past 75 years since the roller milling process came into general use has been a tale of increasing whiteness. Before the introduction of the roller mill, fine wheat flour of a creamy color had been obtained by repeated stone grinding, which reduced the wheat germ and inner portions of the pericarp to such a fine powder that they were not removed by the subsequent bolting process.

In the modern roller mill the germ is detached from the grain and flattened out on the rollers so that it is easily removed when the flour is bolted; the outer layers of the endosperm are also discarded with the bran. The use of chemical bleaching agents has contributed also to the whiteness of the flour and to some extent to its loss of nutritive value.

Until the end of 1939, flours for breadmaking were usually milled in Britain from blended wheats grown in different parts of the world and mixed so as to yield white flour almost constant in its baking properties. As much as 75 percent of the whole grain was obtainable in the form of straight-run white flour, though the usual prewar degree of extraction was 70 to 73 percent. For economy's sake this was increased to the full 75 percent shortly after the outbreak of World War II.

When, in 1940, it became necessary to conserve every available food resource, the Ministry of Food and the Medical Research Council considered seriously the problem of bread and flour in order that the highest possible yield of nutrients for man and animals might be obtained. From published analyses of milling products for mineral and vitamin content and from studies of the digestibility and nutritive value of protein and carbohydrate in breads made from flours of different degrees of extraction it was deduced that a flour representing somewhat more than 80 percent of the grain would contain satisfactory amounts of the most important nutrients. The results of experimental tests by British millers showed that a flour of 85 percent extraction could be obtained which would produce a satisfactory loaf of pale complexion and of high food value.

The Medical Research Council recommended the new flour and showed the advantages of its increased supply of vitamins and minerals. It was specified that the National Wheat Meal should contain 85 percent of the weight of the cleaned wheat and include all the germ and some fine bran.

Of the vitamins present in wheat the most important are those of the B group and vitamin E. As the latter is mainly present in the germ, use of bread made from this flour containing all the germ of the wheat may well be a significant factor in the general health of the people of Britain.

Extensive analyses for content of B vitamins have been made by chemical, microbiological, and biological methods on flours of different degrees of extraction. Tests on growth of young rats indicate substantial gain in vitamin content by increasing the degree of extraction of the wheat from 73 to 85 percent. The mineral content of 85 percent extraction flour is superior to that of 73 percent extraction flour with respect to calcium, phosphorus, and iron. In the former more of the phosphorus is in the form of phytic acid which binds calcium and renders it less easily absorbed, but this disadvantage was overcome by addition of extra calcium carbonate to the flour. The protein content of 85 percent extraction flour is usually greater than that of white flour and somewhat less than that of whole meal. The coefficients of digestibility of the proteins showed that national flour was about 3 percent less digestible than white flour, but this was compensated by its slightly higher content of proteins and their enhanced nutritive value.

Apart from its function in the wartime diet as source of calories and carbohydrate, the national loaf, as compared with white bread, has made a small extra contribution of protein and provided an important addition of B vitamins in the national dietary. It has been calculated that the normal adult requires about 1 mg. of vitamin B_1 daily; this would be supplied by 300 grams of national flour, roughly equivalent to a 1-pound loaf. The average daily consumption of bread in Britain is estimated at about $\frac{3}{4}$ pound a head, which would make a most important contribution of vitamin B_1 to the wartime diet.

The daily human requirement of riboflavin probably lies between 1.5 and 2.0 mg. A daily consumption of $\frac{3}{4}$ pound national bread would provide about 0.52 mg. The chief sources of riboflavin in the diet are cheese, milk, eggs, and meat, all of which are at present in short supply and are strictly rationed. Their probable daily contribution of riboflavin has been calculated to be approximately 0.6 mg. Here again the national loaf is important.

The decision of the Ministry of Food in October 1944 to lower the degree of extraction of the national flour to 82.5 percent and later, in January 1945, to 80 percent is regarded by nutrition experts with anxiety. Although it is possible that the vitamin B_1 content may be kept at a reasonably high level by a recent modification of the milling process, which insures the inclusion in the flour of the scutellum portion of the wheat germ that is specially rich in this vitamin, it is probable that there will be a serious fall in the riboflavin content. There is also the possibility that losses of other unrecognized dietary factors may also have occurred in the lower extraction flour.

In Britain the national loaf made from flour of 85 percent extraction has proved itself a staff of life of high nutritional value and general acceptability during four years of restricted wartime diet. During this period the good health of the people has been almost miraculously maintained, and apprehensions that gastric ulcers and other such conditions might be exacerbated by the browner bread have not been realized. On these grounds alone the use of bread of this type is to be commended, but consideration of the food shortage in the liberated countries of Europe where from necessity, even the bread is a strictly rationed article of diet provides additional reasons, both economic and humanitarian, for the most thrifty use of the wheat available.—Harriette Chick and Alice Copping, Lister Institute.

KEEPING AMERICA FIRST IN THE AIR*

THE NACA AND AERONAUTICAL RESEARCH

By JOHN F. VICTORY
SECRETARY, NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS

THE overwhelming influence of German air power during the first year of the war in Europe awakened the American people to a great danger. With the invasion of France and the Low Countries the President sounded the burglar alarm with his call for 100,000 airplanes, and American industry responded with the miracle of aircraft production. No single measure has had more far-reaching effect in changing the whole course of the war. Subsequently, in the battle of Britain there was a demonstration of the relative value of superior numbers on the German side versus superior performance by Britain's aircraft. That test made it clear that the airplanes that America was destined to produce in great quantity must have superior performance. The dollar cost of the aircraft program was alone sufficient to require enormous expansion of the work of the National Advisory Committee for Aeronautics (NACA) in order to assure a sound return upon the investment. But in time of war money loses significance alongside the imperative military requirements for superior airplanes. Although the Congress, too, can seemingly work miracles in its field, it cannot legislate superior performance into an American airplane. But it can set science to work. It did so, and that is where the NACA comes into the picture.

The NACA was established by the Congress thirty years ago "to supervise and direct the scientific study of the problems of flight with a view to their practical solution." It is a committee of fifteen appointed by the President and serving as such without compensation. The membership comprises General Arnold and Major General Powers of the Army Air Forces; Vice Admiral Fitch and Rear Admiral Richardson; two officials of the Civil Aeronautics Administration; the heads of the Bureau of Standards, the Weather Bureau, and the Smithsonian Institution; and six technically qualified experts from private life.

Out of NACA research has developed the engineering basis for a rapidly advancing technology. Consequently, when the war started, America had a healthy nucleus of a strong, competitive aircraft industry. Had it not been so, we might now be studying German and learning how to do business with Hitler.

There is no natural law known that to-day fixes a limit upon either the speed or the size of aircraft. All types of American airplanes in production to-day, and many foreign types, make use of fundamental design data from the laboratories of the NACA. Let me emphasize, however, that in the development of America's air power the NACA has been only one member of the firm, a silent partner, so to speak, of the military services and of the aircraft industry. The over-all progress is the result of the organized effort of millions of Americans involving many organizations, governmental and private, including not only the military and manufacturing establishments directly concerned with aeronautics, but many supporting agencies and industries. Developments in aeronautics have been extremely rare for which any single organization or individual, in or out of the Government, deserves all the credit.

The airplane and the tank were introduced in World War I, but it remained for the present war to develop their dominant role. In like manner, we may expect that new weapons recently introduced in war may be but the forerunners of a whole new line of weapons that may dominate the future. Aviation is entering an era of revolutionary change resulting largely from the development of new methods of propulsion. The new propulsive systems open up extraordinary high-speed possibilities which must be studied and evaluated.

* From *The Torch*, July, 1945. Copyright by the International Association of Torch Clubs, Inc.

The country can be grateful to the Congress that had the vision to establish the NACA as a separate agency to advance aeronautical science, although that Congress was quite cautious about how it appropriated the taxpayers' money. It started the NACA with an appropriation of "$5,000 a year for five years, or so much thereof as may be necessary." It was not clear whether the phrase "so much thereof as may be necessary" related to the $5,000 or to the five years. However, with continued far-sighted support of the Congress, the NACA has grown from that modest start to an organization of 6,500 employees, with an annual operating budget of $26,000,000 and with major research stations at the Army's Langley Field, Virginia; the Navy's Moffett Field, California; and the Cleveland Municipal Airport, Cleveland, Ohio.

Experience through two great wars has enabled the NACA to blaze new trails in aeronautical research and to lay the foundation in America for the new science of aeronautics. Under the leadership of able chairmen from Dr. Durand in World War I to Dr. Hunsaker and including the illustrious names of Charles D. Walcott, Joseph S. Ames, and Vannevar Bush, the NACA, with the invaluable assistance of Dr. George W. Lewis as Director of Aeronautical Research, has worked as one with the Army, the Navy, and the industry to gain for America definite leadership in aircraft development.

America's leadership first became generally recognized in the late twenties. I recall that in the early thirties there was a meeting in the United States of the Federation Aeronautique Internationale. Discussions by European leaders after visiting the NACA laboratories at Langley Field were to the effect that America was at that time ten years ahead of any other nation in the development of aeronautics. I asked them to explain. They replied that if progress were to cease in the United States, and Europe were to continue at its own pace, it would take any European nation ten years to catch up.

Then came Hitler and the resurgence of German militarism. Setting about quietly at first to build a superior air force, Hitler authorized Goering and Udet to do all things necessary to build for Germany the strongest air force in the world. Realizing that the struggle for supremacy in the air must start in the research laboratory, Germany expanded and multiplied its research facilities until, at the time of the Pact of Munich in 1938, the German aeronautical research establishment had grown to five times the size of that of the United States. By that time Germany had also built its great air force and was then the strongest power in the air.

In 1937 the NACA anticipated the oncoming emergency and began to expand its research facilities. It is fortunate that these additional facilities were constructed in time to increase the effectiveness of the whole aircraft program in this war. Let us see how that has been done. The problems are too numerous to recount in detail, but a few examples can be given.

THE AIRCRAFT program required enormous quantities of aviation gasoline and lubricating oil, involving first of all a determination of the kind of petroleum stocks that could be used to produce high octane gasoline in the quantities needed.

In cooperation with the oil industry's laboratories and with the Army and Navy, the NACA was able to bring forth research data upon which decisions were based for the early expansion of the aircraft petroleum industry. The shortage of natural rubber made it necessary quickly to reach decisions as to the petroleum stocks to be used, and NACA research served as a guide by indicating how different products would perform in an aircraft engine.

The chief tool for aerodynamic research is the wind tunnel, which is, generally speaking, an apparatus or structure for measuring the various characteristics of airplanes under simulated flight conditions. Tests are made at the throat, or smallest section, of the tunnel where the air velocity is greatest. Throat sizes range from 4 by 18 inches to 40 by 80 feet, the latter being the largest in the world, and large enough to test a full size airplane. The NACA has in operation about thirty wind tunnels, used for different purposes; e.g., to investigate stability and control, to increase lift and reduce drag of wings, to

improve streamlining in general, to develop engine installations, to study propeller efficiencies, to make airplanes controllable when spinning, to prevent ice formation in flight, to improve operating characteristics at high altitudes, and to explore the possibilities and obtain fundamental data in the entirely new field of flight at speeds above the velocity of sound—roughly 750 miles per hour at sea level.

Speed is still the most important single characteristic of an airplane. Through the ages the speed of man's travel has increased slowly up to the time of the conquering of the air. Since then improvement has been rapid. One of the greatest developments in the evolution of the aerodynamic, or streamline, form has been the NACA high-speed wing. This wing makes possible under flight conditions as much reduction in wing drag, or resistance to forward motion, as all previous research had accomplished in this respect since the original Wright airplane first flew in 1903.

The first significant application of the NACA high-speed wing was in the P–51 Mustang. That airplane was voted by Europe's outstanding aeronautical experts as the best fighter airplane on either side in the European war. Since Pearl Harbor the NACA has investigated, on request of the Army alone, 42 types of bombers, 50 pursuit types, and about 40 miscellaneous types, and a comparable number for the Navy. As an example of what the NACA does on a new type of airplane, let me mention some other aerodynamic characteristics of the P–51 which are also the product of NACA research: air scoops to provide combustion and cooling air, which greatly reduce the drag of air intake openings as formerly used on airplanes; filleting, that is, the fairing at the junction of the wing and the fuselage; the windshield and canopy shape; NACA flush-type rivets; and improved controllability in dives. The NACA also provided the basic data for the design of exhaust stacks on the P–51 in order to get additional speed by the use of the exhaust as a form of jet propulsion.

The early versions of the Mustang had a 1,000-horsepower engine. The design of the airplane was such that no fundamental bar to speed-increase existed, so that when engines approximating twice the power became available, they were used. The resulting large increase in speed caused serious rumbling and strong vibrations around the radiator scoop. The causes were investigated by the NACA in a wind tunnel at 600 miles per hour. By application to the air scoop of the same principles that were used in the high-speed wing design the rumbling and vibration were cured. This simple improvement permitted a full realization of the speed possibilities of the Mustang design.

The basic principles of the NACA high-speed wing are gradually coming into general use on American airplanes where high speed is desired. The Bell P–59 Aircomet, the Bell P–63 Kingcobra, the Douglas A–26 Invader, and the Lockheed P–80 Shooting Star are mentioned as a few examples. The Shooting Star, a jet-propelled airplane recently announced by the Army, the Lockheed Company, and the General Electric Company, was investigated in model form in a wind tunnel at over 500 miles per hour to determine its high-speed control characteristics and to obtain an estimate of its performance. It was confirmed by those tests that the NACA high-speed wing, which the Lockheed Company had chosen, would permit the P–80, with its new jet power plant, to fly at a higher speed than any previous airplane. Extensive flight tests were made by the NACA Flight Research staff to correlate the performance data obtained in the wind tunnel and in flight and to make full-scale load measurements by recording the air pressures on the wings and tail surfaces during various high-speed maneuvers. This information is of exceptional value to designers because it is the first ever obtained at such high flight speeds. The NACA test pilot who conducted the flight tests on the P–80 flew, in those tests, faster than any other human being, without, however, bothering to establish an accredited world speed record.

As airplane speeds approach the velocity of sound new problems are encountered because the character of the air-flow over the wing, in obedience to natural law, changes abruptly at that speed and causes an im-

portant loss of lift and serious increase in drag.

Although the speed of any airplane can be increased through the simple use of more power, this method has serious limitations because constantly enlarging the power plant would increase weight and fuel consumption and reduce useful load. The major objective of aerodynamic research, therefore, is to increase speed by reducing the drag of the whole structure. The ability steadily to achieve progress in this direction has distinguished aviation from other forms of transportation. For example, to increase the speed of an ocean liner five knots would require twice the horsepower. To increase materially the speed of railroad trains would require not only more power but greatly improved road-beds and rolling stock. Likewise, to increase materially the speed of automobiles would not only take more gasoline and tires and cause more rapid depreciation and more accidents but would also require improved superhighways. On the other hand, the onward march of aeronautical science shows increases in speed being attained with increased efficiency. For commercial aircraft this will mean increased speed at less cost per passenger mile. And the more we learn to improve the performance of the airplane, the greater and more practicable of accomplishment seems to become the need for further improvement. Of course somewhere in the course of airplane development there will come into the equation some limiting factor of diminishing returns, which will impose a practical limit on speed and size. But, if we judge the future from the past we shall, as limits are approached, merely raise our sights a little higher and press for some further scientific development that will permit us to carry on.

As an example, the NACA has demonstrated by recent research that flight at supersonic speeds, that is, above the velocity of sound, is possible with new types of power plants and is conducting further research to make it an actuality.

Despite such development, large increases in drag will still occur at speeds between present flight speeds and supersonic speeds and represent one factor in hindering the immediate development of higher speed airplanes. It is essential that the drag of high-speed airplanes be made as small as possible. This problem is being intensively studied in high-speed wind tunnels and in flight, and considerable progress has already been made in the development of improved configurations not only for wings but also for fuselages, tail surfaces, cowlings, air intakes, windshields, and canopies.

The huge passenger airliners of the future may be powered by gas turbines of 5,000 to 10,000 horsepower per engine, using low-grade, nonvolatile fuel. The gas turbine has many fundamental advantages over the reciprocating engine. It has, for example, fewer moving parts, is considerably lighter in weight and smoother in operation, and its relative efficiency is superior at high altitudes and at high speeds.

The NACA is pushing research on the metallurgical and aerodynamic problems involved in the development of light, powerful compressors. Aircraft turbines will also require research to develop materials that will keep their strength at high operating temperatures.

The idea of a gas turbine has been known for two thousand years and has been used for at least a score of years in other forms of transportation and in stationary power plants. Why has it only recently attracted such attention that it is hailed as *the* engine of the immediate future for aircraft? One reason is that, since 1939, the NACA has initiated and sponsored research by others in the development of new alloys having high strength at high temperatures.

German turbine development so far has been accomplished without using such high-temperature alloys, but this has required a complicated system of cooling which would not have been necessary had they had our high-temperature alloys. Another NACA contribution was the improvement through research of the efficiency of supercharger compressors from below 70 percent to 85 percent, and higher. These improvements enabled us, after a slow start, to gain the lead in this most important field.

The NACA initiated steps for the development of jet propulsion in this country. Several years ago the NACA formed a Special Committee on Jet Propulsion and got three

large firms interested in accepting membership; namely, General Electric, Westinghouse, and Allis-Chalmers. As a result, each of these firms developed designs of jet propulsion units, two of which are now in production. The third seemed better adapted for postwar development and therefore has not been pushed.

Parallel with that activity, General Arnold saw what the British were doing with the Whittle engine, and in conferences with our committee it was agreed to get an American firm to manufacture the Whittle engine. The Army then made a contract with General Electric resulting in the production of this jet propulsion engine, which was first used in the Bell P–59 airplane.

Flowing directly from their original interest in the subject, the General Electric Company went on further and developed the jet propulsion unit now used in the Lockheed P–80 Shooting Star. In the P–80 we have something fully as good as, and perhaps superior to, either the German or the British jet-propelled airplane. The P–80 has been studied by the NACA to show where modifications could be made or must be made to obtain satisfactory performance at altitudes for both winter conditions and summer conditions.

There is no way to measure the value in war of superior performance of aircraft. It may mean the difference between winning and losing a war. The Battle of Britain in 1940 is a case in point. But the scientific research of the NACA has also a tremendous economic value which can be measured in part in dollars and cents. Take for example the B–29 which has so rudely awakened the Japanese leaders from their comfortable dream of world conquest by putting so many of their industries to sleep. That airplane is at present the great white hope of the Army Air Forces in the long-range bombing of Japan, although the Army has three new bombers coming along which are expected to be more powerful and capable than the B–29. They are the Consolidated B–32 (virtually a parallel project to the B–29), the Northrop B–35, and the Consolidated B–36, which is larger than the B–29. The American people are putting into the B–29 program alone several million dollars. Now what is the end product of that great expenditure? Transportation of bombs for long distances. Recent NACA research indicates a possible fuel saving which could very materially increase the return upon that investment either in terms of dollars and cents, conservation of material resources, or by making more bombings sooner to shorten the war. And that research cost only a few thousand dollars.

I might give another interesting example of research as applied to the B–29. That airplane was designed for a certain gross load. Military necessity required that it carry a greater load. As a result the engines had to produce more power, which resulted in their overheating. This in turn shortened their service life, sometimes caused failure in flight, and endangered the airplane and crew. NACA research showed where the overheating was critical and pointed to a partial remedy which is being applied and is resulting in greater safety in operation and considerable lengthening of the service life of B–29's.

One of the most serious hazards to flying is ice formation. The NACA Exhaust Heat De-Icing System for the Prevention of Ice Formation on Aircraft is being applied on several types of military aircraft now in production and has permitted safe flight in weather conditions which otherwise would have grounded airplanes. It is one of the greatest contributions to the safety of flying ever made.

The NACA altitude wind tunnel, just recently placed in operation at the Cleveland laboratory, is unique. It has been the most difficult project ever attempted in the realm of aeronautical research anywhere in the world. In an airstream twenty feet in diameter, flowing 500 miles per hour, there are maintained conditions of temperature and density existing at elevations up to 50,000 feet, i.e., about 67 degrees below zero (Fahrenheit) and a pressure about one-ninth of that at sea level. And this has been accomplished despite the fact that the power output of the engine under test and the energy of an 18,000 horsepower motor to create the wind must be absorbed as heat by the rarefied airstream while the temperature is kept at 67 below. The NACA Altitude

Wind Tunnel is the only place on the surface of the earth where the performance of jet propulsion engines can be investigated under altitude conditions. Without it, many months and the running of dangerous trials would be needed to obtain flight data that can be obtained with ease and safety in the altitude wind tunnel in a very short time. It should give America a definite advantage in the development of jet propulsion airplanes.

Whereas a year ago only a few types of airplanes in all the warring nations could attain a speed in level flight of 400 miles per hour, the United States, England, and Germany now have airplanes in the 500 mile-an-hour range. Now NACA scientists, and no doubt those of other nations as well, are preparing to enter the entirely new field of aerodynamic problems involving flight of aircraft and of guided missiles at speeds above the velocity of sound. In order to explore these possibilities the National Advisory Committee for Aeronautics is actually constructing new supersonic wind tunnels having airspeeds much faster than the speed of sound, ranging up to the neighborhood of 2,000 miles per hour.

In time of war the NACA operates as a research and engineering facility of the Army and Navy. Its work is the most fundamental activity of the Government in connection with the development of aircraft, and its relations with the military services and with the aircraft manufacturing industry are constant and intimate. It is this teamwork that has made possible the great development of America's air power in this war. The results of NACA research will continue to be reflected in the steady improvement in the speed, range, carrying capacity, stability, controllability, maneuverability, ceiling, rate of climb, and, in short, in the general military effectiveness of America's aircraft.

One of the outstanding lessons the present war teaches is that the problem of insuring America's future security is inseparable from the problem of keeping America first in the air.

NACA research on military problems will be largely applicable to commercial and private aircraft after the war, with this significant distinction: In military aircraft superior performance *must* be achieved, if at all possible, even at the expense of lowered safety and economy. In the development of civil and commercial aircraft the research emphasis must be primarily on safety and economy of operation.

Following the war, the world will enter the aerial age about which men have dreamed. Air transportation to all parts of the world will shrink time and distance and bring the peoples of the earth closer together in forceful emphasis on the fact that we all live in one world. Underwriting America's future in the aerial age and underwriting America's investment in such future preparedness in the air as will effectively preclude attack is its liberal support of research in aeronautics. America's aeronautical research policy is thus, in the military sense, an insurance policy on the very life of the nation; and in the commercial sense it is a sound investment assuring America a position in the forefront of progressive nations when the progress of civilization is resumed after the war.

NACA's John Stack, Chief of the Langley Laboratory's Compressibility Research Division and recognized authority on high-speed problems, indicated in the last Annual Wright Brothers Lecture the possibility within a few years of aircraft traveling westward at "clock-stopping" speeds. This would mean a speed at our latitude equaling the earth's speed of rotation on its axis and giving the effect of keeping the sun in a fixed position. In other words, regular air transportation leaving Washington, say, at 12 noon would arrive at San Francisco at 12 noon the same day!

SCIENCE ON THE MARCH

ATOMIC THUNDERBOLTS

Just as people recall the circumstances under which they first heard the news of the attack on Pearl Harbor, so they will remember how the atomic bomb first burst upon their consciousness. In the Brownstone Tower the news came in the form of a telephone call from New York, early in the afternoon of August 6. The editor of a weekly wanted a photograph of Dr. J. R. Oppenheimer to run on the cover of his magazine. Could we provide a print of his photograph, which was published on page 574 of the SM for June 1941? Unfortunately, we could not. Later in the day we read a mimeographed news release entitled "Statement of the Secretary of War," and then came the newspapers.

We have no information about the atomic bomb that has not already been printed and broadcast everywhere. On the other hand, we could not disregard in Science on the March such a world-shaking stride in applied science. We can at least point out to our readers that they can prepare themselves for better understanding of future articles on release of energy from the nucleus of the uranium atom by reading or re-reading the brief article by Herbert L. Anderson on "Progress in Harnessing Power from Uranium" (SM, June 1940, pp. 570) and continuing on to the next article "Is Atomic Power at Hand?" The latter is an illuminating discussion of the subject by Robert D. Potter, who is both a physicist and one of the leading science news writers of this country. He should now rank as a prophet for he wrote:

> If the chain reaction of having one uranium atom split and liberate the neutrons which will split another one nearby, and so on, can be controlled, then a compact power source for military purposes could be achieved despite whatever the cost might be. Things which are uneconomical in a peacetime sense become practical for military services if they can perform tasks not possible, or carried out as easily, in any other way. No price can be put on such developments that might save the life of a nation that owned the discovery, anymore than one can put a price on a surgical operation which saves a man's life.

The dream of utilizing energy released by disintegration of atoms originated long before the 1939 discovery of the fission of uranium atoms by neutrons. No doubt it occurred to the first investigators of radioactive elements. An expression of this dream, so lately come true, may be found at the end of an article by L. R. Koller of the General Electric Company on "The Radioactive Elements" (SM, July 1928, pp. 54–56). Dr. Koller wrote:

> If we but knew how to control disintegration of this [atomic] kind and cause it to take place at will, the engineer might find in a few grams of matter the energy for which he now has to consume tons of fuel, and our whole economic system and daily life might be revolutionized.

Little imagination is needed to picture the destructive and possible constructive effects of controlled release of atomic energy. Will it be used for the good of the human race? Grave doubts are in many minds, and science is being regarded both with greater respect and with greater apprehension than ever before. It is appropriate that Dr. Weltfish's proposed oath for scientists should appear in this issue, because scientists should accept partial responsibility for the social consequences of the applications of scientific knowledge.

Many people, staggered by the implications of the atomic bomb, may feel that the advancement of science no longer needs any encouragement—that retrogression might be desirable. That notion, we think, is understandable but utterly wrong. If most citizens of the important countries of the world were equal in intellectual and ethical stature to those whose names appear in "American Men of Science" danger of misuse of atomic power would not exist. The most important fruits of science are the character and way of life of scientists; the material results of scientific knowledge are but by-products. It seems fair to conclude, therefore, that science should be advanced more rapidly than ever if the peoples of the world are to attain adequate respect for truth and sufficient understanding of man and nature to avoid catastrophe.

However deplorable the human deficiencies that made it necessary for American and British scientists to develop the atomic bomb, we must admire their glorious achievement. In the simpler days of old, it is said, Prometheus stole fire from the ruler of the gods and gave it to man for his use; now modern Prometheans have raided Mount Olympus again and have brought back for man the very thunderbolts of Zeus.—F. L. CAMPBELL.

DEFOLIATION OF PLANTS FOR PROFIT

THE phenomenon which occurs in green plant-leaves whereby carbon dioxide from the air, water from the soil, and energy from the sun are elaborated into sugars is, no doubt, the most important reaction known to man. Our food, heat, clothing, and shelter are practically all the direct or indirect results of this photosynthetic reaction. It is in the younger leaves of plants that most of the photosynthesis occurs, whereas the more mature leaves usually serve chiefly as organs of water and food storage for the plants.

In spite of their necessity and great importance in maintaining life on the earth's surface, the leaves of many deciduous crop plants reach a certain stage of maturity each season when they become useless and often are a detriment, rather than a help, to the production of seed, fruit, or fiber.

Too many large leaves may delay maturity of the early fruit. This is especially true of cotton, which is a perennial in its natural habitat, but in most countries is commercially grown as an annual. In a great many seasons the usual abundance of leaves prevents air circulation and sunlight from properly maturing the early fruit and boll rot often results.

In most seasons the seed of the soybean dries out on the plant only very slowly, and harvesting is consequently delayed. This is largely because the loss of water by transpiration forces the leaves to continue to pull water from the soil through the plant stems long after these same leaves lose their usefulness in increasing seed production. The harvesting of many crops is often hindered and delayed because of the presence of either fresh green or dry leaves. This applies particularly to the mechanical harvesting of cotton and grain crops. In the case of tomatoes, which require bright and direct sunlight for quick, full red-color ripening, late, abundant foliage may protect the fruit from this light so fully that harvesting is delayed until a considerable portion of the crop is lost by frost.

The necessity for some kind of foliage control to facilitate harvesting was brought to a focus in the development of the mechanical cotton harvesters. Getting rid of dried leaves and avoiding the chlorophyll stain on the fiber caused by crushing green leaves gave the engineers much worry. It had long been known that many chemicals will kill leaves, but they also kill or adversely affect the remainder of the plant. It is also a matter of common observation that when foliage is harmed in some manner, the plant will shed it and produce new leaves if growing conditions are favorable, and if the plant is not too nearly mature.

Leaf shedding is a normal growth process and is accomplished by the natural growth of a layer of abscission cells at the juncture of the leaf petiole and plant stem. Obviously, the phenomenon occurs only when the plant itself is unharmed. Defoliation by chemical means requires a compound which will stop the life functions of the leaf cells but will not damage the petiole nor be translocated, in harmful form, from the leaf to the plant stem and roots.

A chemical dust, now on the market and in which calcium cyanamide is the active ingredient, fulfills these requirements for a number of plants. Applied dry on leaf surfaces, when dew is present or when dew will form in a few hours after treatment, it quickly combines with water and interferes with the respiration of the leaf cells, causing cell deterioration, but does not harm the leaf petiole or stem of the plant.

The active chemical causing this lethal effect on the living cells is not stable. By the time the leaf cells are killed it has further reacted with water to form harmless compounds which are not translocated from the leaf to the stem of the plant. The plant is left unharmed and immediately sheds the damaged foliage by natural growth processes.

When other more stable toxic chemical compounds, which are known to be harmful to plant cells, are applied to plant foliage

they slowly kill not only the leaves but also the petioles and the plant stems. The extent and rapidity of the killing depends on the concentration of the material applied, the susceptibility of the plants involved, and the period in the growing season. They do not cause defoliation because they kill not only the leaves but also that part of the plant where the abscission cells are grown—"freezing" of the dead, dry leaves on the plant usually results.

The removal of plant leaves with this chemical dust is not entirely a chemical reaction. To grow abscission cells and shed its leaves a plant must be in growing condition. If, for example, a plant has been subjected to such severe drought conditions that it does not, each night, recover from its daily wilt, defoliation will not occur. A week or so after such plants are reactivated by rain or irrigation, they defoliate readily.

When crop plants are defoliated in late season, the conditions for growth of new leaves (long days, optimum temperatures, and sufficient moisture) are usually so unfavorable that they are not produced or, if so, they do not grow in profusion. As a consequence, the fruit or seed on the defoliated plant matures much more rapidly and is ready for harvest at a much earlier-than-normal date.

One of the greatest advantages of cotton defoliation is the avoidance of crop loss, due to boll rot, through which as much as 50 to 60 percent of the crop may be lost in some years. Cotton sets its earliest and best bolls in the lower part of the plant. In normal to wet years the foliage growth is often so luxuriant and heavy that the fiber in these early-opening bolls rots because it does not get enough direct sunlight and air circulation to dry out each day. As soon as the plants are defoliated all boll rotting is stopped.

With the leaves off the plants, the late bolls open up more readily and a much higher percentage of the total crop can be harvested in one picking. This always improves the grade of the cotton because the harvest is completed so soon after the bolls open. With no leaves to hide the bolls, hand-picking is facilitated. Work can be started earlier in the morning because, due to the absence of foliage, the cotton dries out so much more quickly and is ready to pick.

Both green and dried leaves have been a handicap to the mechanical harvesters. If green leaves are mashed by the mechanical pickers, a chlorophyll stain is left on the fiber which is almost impossible to remove. These stains show up in cotton cloth and do not take dyes the same as the unstained part. In the past, mechanical strippers were not used until after the plants were defoliated by frost. This always meant late harvests and much loss of grade in the cotton because of weather exposure of the opened bolls. With chemical defoliation this type of harvesting can be started two to three weeks before frost, and the entire crop can be gathered before bad weather sets in.

Where hand "snapping" or "stripping" (pulling off the whole boll) is practiced, laborers working in defoliated fields can harvest practically twice as much cotton in a day as in fields where the foliage is left on the plants.

Many entomologists believe that destroying cotton leaves in early fall will assist in boll weevil control, because the late broods of weevil may go into the winter in a starved condition and many may not survive. As a control measure, in Lower Texas where the pink bollworm is slowly spreading, the law requires that all cotton plants be destroyed, or plowed under, by September 1. Defoliation benefits these Texas cotton growers because it enables them to start harvesting at an earlier date.

Some growers believe that where cotton is grown under irrigation maximum yields have not been attained. The large amounts of water and fertilizers required for highest yields have not been used because they would cause excessive foliage growth. Now that the foliage can be eliminated in the fall, before it causes damage to the opened cotton, these growers believe they can increase yields to as much as three bales per acre.

Cotton leafworms are frequently a serious pest in cotton-growing areas. In only moderately heavy infestations these worms defoliate cotton plants about as completely as it is done with chemicals, and with similar effects. However, the final result is most unsatisfactory because of the damage to,

and grade lowering of, the fiber caused by their excrement. If heavy infestations of this pest are imminent, chemical defoliation of the plant robs them of their food and keeps the cotton fiber clean.

The Midwest soybean acreage has been greatly expanded because of the war, but farmers have not yet found the proper place for this crop in their usual rotations. Wheat should logically follow beans, but the latter mature so late that the best wheat seeding dates are past before the beans are ripe enough to harvest and store. Soybeans cannot be safely stored until their moisture content is down to 13–14 percent, and with the leaves on the plant the seed does not reach this low moisture level until late in the season. Chemical defoliation of the plants any time after the seeds lose their green color causes rapid loss of moisture without reduction in yield or quality of seed.

The date of safe harvesting is, consequently, advanced a few weeks, and the harvesting operation is greatly simplified because there is much less quantity of plant material to go through the combining machine. In years of bad weather in late fall and early winter many beans are lost by storm damage because they cannot be harvested early. Defoliation permits of harvesting before the average dates for damaging storms.

Throughout the northern states and in areas on the West Coast it is quite common for tomato growers to lose a large percentage of the fruit already produced on the plants because they do not ripen before frost. With declining temperatures and decreasing hours of intense sunlight in early fall days, the fruit on tomato plants with heavy foliage does not rapidly take on a full red-ripe color so necessary for high quality. If left in the field long enough for this color to develop, the fruit is often lost through frost. Commercial canneries oftentimes can operate only half time in late season because tomatoes do not ripen rapidly enough to keep them busy. Defoliation of tomato plants after the fruits are full sized greatly hastens ripening without reduction in yield and prevents crop losses due to unfavorable weather.

Nurserymen, who grow deciduous fruit tree stock, often have difficulty in getting the young trees to mature early enough in the fall for timely digging and storing. Defoliation of this stock, after the season's growth is made, induces early maturity without harm to the young trees—as a result, the stock goes into storage in good condition at an earlier date and the harvest labor is spread over a longer period of time.

The leaves of some crop plants such as cotton and tomatoes can generally be removed by applications of only 30 to 35 pounds per acre of defoliant. Soybean foliage, on the other hand, is more resistant and applications of as much as 100 pounds per acre are sometimes required if the plants have been toughened by dry weather.

Even distribution of the material so that some of the dust falls on each leaf is of utmost importance if complete defoliation is desired. Those leaves on a plant which do not get any of the dust are entirely unaffected by the application. If the dust can be sufficiently well controlled, selective defoliation of any portion of a susceptible plant can be practiced. This, however, is almost impossible under field conditions.

Airplane applications of defoliant dust have been completely satisfactory when experienced operators did the work. Pulling ground-dusters through the mature crops sometimes causes moderate loss by mechanical injury to the plants, but this means of application is widely used where airplanes are not available.

Defoliation experiments are under way on an expanding number of crops and plants on which the foliage serves no useful purpose after a certain date and which may delay and interfere with harvest, lower the crop quality, or harbor harmful insects.—M. V. Bailey, Director of Agricultural Research, American Cyanamid Company, New York, N. Y.

BOOK REVIEWS

FROM FUNCTIONALISM TO CULTURAL DYNAMICS

The Dynamics of Culture Change. Bronislaw Malinowski. 171 pp. 1945. $2.50. Yale University Press, Connecticut.

BRONISLAW MALINOWSKI was an outstanding figure of recent anthropology, and his death has deprived the discipline of one of its most provocative and stimulating personalities. Trained as a physicist, his interest in the social mechanisms of human life turned him to anthropology, and the outbreak of the last war found him in the Trobriand Islands, on the extended field trip that was to be so important in shaping his thought. For out of the experiences of these three years of living with the people of the small community where he worked, and the intensive researches he made into their customs, he evolved the approach toward the study of culture he called functionalism.

This approach was in line with the *gestalt*-type of thinking prevalent at the time of its enunciation in the middle twenties, and, as in all the sciences, represented a reaction against the atomistic approaches that preceded it. The term functionalism caught on immediately, and the reports on various aspects of this field-work that began to come from Malinowski's pen received a wide and sympathetic hearing. The first of these works, entitled "Argonauts of the Western Pacific," was an analysis of Trobriand economics, an aspect of native culture that had been quite neglected by anthropologists to that time, incredible as this may seem today. Later came an analysis of Trobriand social structures and marriage customs—a notable contribution to perspective in the field of psychoanalysis—studies of the myths of these people, and of other phases of their existence.

Malinowski was never content merely to describe the life of the people among whom he had worked. His fertile mind was ever in search of new relationships, so that he invariably drew generalizations from his data that might be tested in other societies. Indeed, one criticism that was most often laid against him was that he saw the whole range of non-European cultures in the light of a single tribe—a criticism that was perhaps more inspired by those who were stung by his biting comments than by reason of the generalizations themselves, which, in fact, showed the insight of their author by the extent to which they proved to be valuable working hypotheses. Functionalism, however, was essentially a method, and it was the application of this method to wider fields that brought his attention to Africa.

For Africa, with its vast spaces, its great resources, and its numerous populations, was of far more consequence in London, where Malinowski held his professorship, than an obscure islet off the coast of New Guinea. Under his direction were the Fellows of the International Institute of African Languages and Cultures, and through directing their work he came to know, at once removed, the cultures of British Africa. His one trip to the continent was in the nature of a survey; it was confined to East and South Africa, and no intensive work was included in it, though "spot" studies, the results of which he never published, were carried out.

Unlike the Trobriands, where isolation had permitted the culture of the native folk to remain stable and relatively undisturbed, Africa, over all its enormous bulk, was in the throes of cultural change, the result of the impact of European expansion and imperialism on native life. Here tribes, numbering tens and hundreds of thousands, confronted new techniques, were required to make new economic orientations, adjust to new value-systems and a new world view; and all this under conditions that could only spell demoralization. Malinowski felt this as a challenge to him as a scientist, a teacher, a humanitarian, and accepted it by advancing the doctrine of what he termed "practical anthropology"—the anthropology of the changing native which should be as alert to the need for prescribing programs of action as to the discovery of scientific truth.

But Malinowski was deeply attuned to thinking in terms of a system of thought derived from study of an adjusted, conservative, small community. His theory of functionalism, was essentially ahistorical, if not

antihistorical, as his writings demonstrate in the many passages containing severe criticism of diffusion studies, of historical reconstructions, of any consideration of the factor of time depth in the analysis of human culture. For culture was to be considered in terms of the interplay and interrelation of its aspects, which functioned in their entirety so as to make a well-integrated whole that filled the life of a people, giving it meaning for them and affording them the satisfactions they grew up expecting to obtain in their society.

Africa, however, was dynamic. Cultural change, not stability, was the outstanding aspect of its social life. And in this situation, where the factor of time was paramount, the ahistorical system of functionalism did little to facilitate study. Malinowski's students did achieve brilliant results, and though they employed the term functionalism diligently in their writings, one has the impression that they achieved these results not because of their theoretical approach but because Malinowski, a magnificent teacher and field-worker, trained them to observe and report the facts as they found them, irrespective of the frame within which they worked. For Malinowski himself was never really at his ease when he considered the African field. His writings on the subject have a strident quality never found in his earlier work, as though his arguments were directed toward convincing himself first, the reader only secondarily.

The volume here under review, which has inspired these reflections, represents passages from his published discussions of the anthropology of cultural change, finished pages for the work this book was intended to be, and materials in note form. These were assembled with devotion by one of his students, Dr. Kaberry, who though a specialist in Australian anthropology, did remarkably well in what must have been a most difficult task in handling such fragmentary materials relating to African problems. But the work does not come off. I, for one, prefer to think of Malinowski's contribution as he made it in the exciting pages where he hammered out his theory and method of functionalism; where with dry humor and incisive analysis, he stripped pretention to its sterile reality and joined others in infusing life into what should be the most living of sciences, the science of human civilization. It is little service to his memory to present his jottings to the world. His contribution will live long, but this work will not be a part of it.

MELVILLE J. HERSKOVITS
DEPARTMENT OF ANTHROPOLOGY
NORTHWESTERN UNIVERSITY

A TAXONOMIC GUIDE TO SOIL FUNGI

A Manual of Soil Fungi. Joseph C. Gilman. 392 pp. Illus. 1945. $5.00. The Collegiate Press, Inc., Ames, Iowa.

THE author of this volume is one of America's well-known mycologists, with years of experience in teaching and research. He has long been especially interested in the fungus flora of the soil. In 1927, in collaboration with Dr. E. V. Abbott, a colleague at Iowa State College, he published a paper, "A Summary of the Soil Fungi," in which the effort was made to provide a taxonomic key to all the species of fungi known to have been isolated from the soil. The paper was based in part on their own study of the fungi of Iowa and Louisiana soils during the preceding five-year period. The paper proved useful to investigators in the field of soil microbiology, as well as to workers in industry and medicine. Impressed by the demand for separates, indicating a growing need for such a publication, Dr. Gilman undertook its revision and now presents it in book form, brought up to date and greatly amplified by the incorporation of much additional information taken from the mycological literature of the intervening years.

As emphasized by its author, the book is largely a compilation. The species of fungi included in it are those which have been recorded in the American and foreign literature as having been isolated from the soil. The treatment is strictly taxonomic. A detailed technical description of each species is given, accompanied by citations to the publications in which its isolation from the soil has been reported. Dichotomous keys are provided to aid in the identification of unknown material in hand. The reader is assumed to have knowledge of the techniques employed in pure culture studies of the fungi, no information being provided con-

cerning them. The species included in the manual are chiefly those that have appeared in plate cultures made from soil samples. Few of them have been observed growing in the soil itself. Some have been isolated repeatedly by various investigators. Many others seem to have been included in the book on the basis of a single record. Species which do not develop in the fruiting condition on the usual types of artificial media are automatically excluded from consideration. The terrestrial mushrooms and the many other soil-inhabiting *Basidiomycetes* and *Ascomycetes* are in this category. The only member of the *Basidiomycetes* to be found in the volume is *Pellicularia filamentosa,* easily recognizable in the mycelial condition and long known as *Rhizoctonia solani.* Though *Sclerotium rolfsii* is given a place, none of the other sclerotial fungi of the genera *Sclerotium, Sclerotinia, Botrytis, Typhula,* etc. are included. The *Actinomycetes* and *Zoophagaceae,* unquestionably important soil fungi, are intentionally omitted. So also are the yeasts. Plant pathogens considered to be soil borne but which have not been reported as having been isolated directly from the soil are excluded. The same is true of the large number of species recorded as growing in leaf mold, decaying wood, and other similar substrata not yet completely incorporated in the soil.

The *Ascomycetes* are covered in twenty pages of the text, descriptions of four *Discomycetes* and twenty *Pyrenomycetes* being provided. Ten of the latter are species of *Chaetomium.* The material on the *Phycomycetes* covers 133 pages. The treatment of the *Mucorales,* based on that of Zycha, is relatively extensive, embracing nearly one hundred species, forty-one of which are members of the genus *Mucor.* The *Saprolegniales* receive more space than was to have been expected, sixty-one pages being given to the presentation of lengthy descriptions, most of which seem to have been copied essentially verbatim, with the consent of W. C. Coker, from his well-known book. Though the manual includes descriptions of fifteen species of *Pythium,* the absence of reference to any of the papers of Drechsler emphasizes the point that species stated to have been isolated from diseased roots, bulbs, or plant debris, rather than from the soil, have for that reason alone been omitted. Though species of many genera of the Fungi Imperfecti are incorporated, they are almost exclusively members of the *Moniliales* (*Hyphomycetes*). Moreover, of the 187 pages given to this group, a total of 111 is used for detailed descriptions of 124 species of *Penicillium,* 32 of *Aspergillus* and 41 of *Fusarium.* The diagnoses of the *Penicillia* and *Aspergilli* are copied chiefly from the two well known volumes by Thom, and Thom and Church respectively, while the descriptions of the *Fusaria* are translations of those in Wollenweber and Reinking's "*Die Fusarien*" used by permission of the Alien Property Custodian. Similarly, keys and descriptions are taken from Zycha's "*Mucorineae.*" The professional mycologist and others to whom the original papers are available will doubtless prefer to consult them, since they are more inclusive and more adequately illustrated. Workers dependent largely on the manual for their taxonomic information concerning the fungi will, however, find themselves provided with many thoroughly reliable and adequately detailed descriptions prepared by specialists on the largest and most difficult genera.

In his introduction Dr. Gilman points out that many of these so-called "soil fungi" have air-borne spores which are widely disseminated and germinate to produce mycelium and form spores again on many sorts of substrata. They cause spoilage of stored foods, cloth, paper, cork-products, wallboard, leather goods, and even casein paints. They are, therefore, not exclusively soil fungi, and they have a much wider interest and greater importance than results from their significance in connection with the problems of soil microbiology. During the present war the molding and decay of tent fabrics and many other materials in use by our army and navy under exceptionally humid conditions in tropical climates has brought these molds sharply to attention. Trained mycologists are serving in laboratories established by the armed forces for the study of these fungi with emphasis placed on the development of preventive measures designed to reduce the loss in time and materials resulting from their attacks. In in-

dustry and medicine also the molds have gained enormously in practical significance in recent years, and an increasing number of research workers are engaged in the study of vital problems involving a knowledge of the identity of the various species as well as of their physiological activities.

Information concerning the morphology and taxonomy of the molds is scattered through many publications, some of which are not available except in the larger libraries that have specialized in their accumulation. There has been no single volume to which the average worker could turn for a reasonably adequate treatment of these fungi. The appearance of Dr. Gilman's attractively bound, clearly printed, and carefully prepared volume is well timed, and we predict a good sale for it. The book fills a distinct gap in the mycological literature and meets at least partially a very definite need. Moreover, it contains an extensive bibliography of 169 references to many of the other significant taxonomic publications. A glossary of technical terms is appended and an adequate index is provided. The generic characters are illustrated by 135 small pen sketches.

The reviewer feels that should a second edition be prepared the author might well give consideration to the advisability of providing more abundant illustrations designed to aid in the separation of species. He feels also that the title selected for the book is misleading since many of the species included are no more typical of the soil than of many other substrata. Moreover, since the appearance of a mold colony on a plate culture made from a soil sample fails to provide proof that the organism is a member of the normal fungus flora of the soil, the criterion used for selection of the species incorporated in the volume seems lacking in any fundamental significance. The book would have been considerably more valuable to the plant pathologist than it is if descriptions of soil-borne pathogens such as species of *Phytophthora* had been included.

H. M. Fitzpatrick

Department of Plant Pathology
Cornell University

JET PROPULSION THEORIES

The Coming Age of Rocket Power. G. Edward Pendray. 244 pp. Illus. 1945. $3.50. Harper and Brothers, New York.

The character of this book is indicated by the statement on its last page ". . . those of us who have spent years in the study and development of rockets have acquired an emotion about them which is almost religious." This attitude is not one which could produce an analytical study, but is admirably suited to fantasy and speculation. The book is primarily a statement of what a rocketeer feels that the rocket will be able to do rather than an attempt to ascertain its foreseeable developments in the not-too-distant future or its ultimate limitations.

The principal contribution of the book is the historical part dealing with the origin of the rocket, its use in the Napoleonic era, the Congreve rockets, and the uses of the rocket in the present war for the propulsion of projectiles, for assisting the take-off of aircraft, and as the motive power of aircraft. The rudimentary experiments of the German and the American rocket societies are discussed at some length and due weight is given to the pioneer work of Goddard and of several German experimenters.

The elementary theory back of all this is presented briefly and so incompletely as to give the reader a false idea as to rocket potentialities. For example, a table is given of theoretical jet velocities obtainable from various fuels—a factor of major importance in determining possible rocket velocities. The tabulated jet velocities are obtained by the simple process of equating the kinetic energy of the jet to the heat of combustion of the fuel. The objections to this procedure cannot be detailed here, but the results obtained from it are entirely misleading. Or again, it is stated that the changing size and shape of the combustion chamber in a rocket is a factor in determining its performance. Actually it has no influence so long as there does not result a change in the area of the reacting surface, as is the condition in a modern rocket using an extruded solid propellant.

The terminology of rockets is in process of being established and it is important that

care should be taken in this matter by the writers of early books. The reviewer finds himself at variance with the author as to a number of the terms which he employs. In the title and in the text "Rocket Power" is used quite unjustifiably. The title should be "The Coming Age of Rockets." We do not speak of the present as the age of automobile power or the age of airplane power, but rather as the age of the automobile or the airplane.

Then again, jet propelled craft operating without compression are described as having either "intermittent duct engines" or "continuous duct engines." Apart from the uncertainty as to whether it is the duct or the engine that is either intermittent or continuous (a problem similar to that found in menus on the matter of half-broiled chicken or half broiled-chicken), the fact that the operation occurs in a duct is not fundamental —the same procedures can be carried out in structures of a form that no one would call a duct. In fact, the Karavedine engine—the earliest precursor of the "intermittent duct engine"—has no resemblance to a duct.

The play of fantasy permeates most of the book. While atomic energy as a source of power is dismissed as something for the distant future, such sources of energy as liquid monatomic hydrogen and liquid ozone are given more serious attention. The rocket as a commercial device for the transportation of mail and freight is pictured as a development of the relatively near future. A chapter is devoted to journeys to the moon but without any optimism as to their realizability.

LIONEL S. MARKS
GRADUATE SCHOOL OF ENGINEERING
HARVARD UNIVERSITY

PHILOSOPHICAL THOUGHTS ON SCIENCE

The Romance of Existence. Ross Bundy. 188 pp. Illus. 1944. $2.00. The Pitman Publishing Corporation, New York, Chicago.

THIS volume is a pungent treatise of philosophical reflections concerning scientific knowledge of the physical universe, particularly with respect to our earth and the development of life upon it. This is a tremendous subject, for it brings together the most profound findings of astronomy, geology, physics, chemistry, biology, and even religion. The author delves deeply into fundamental discoveries of science, reflecting upon them in most discerning ways with unique phrases and sentences, and spicing these with a generous use of newly coined words. The book is a mental stimulant for readers who take delight in contemplating the origin of our earth, evolution, and universal law.

In producing this treatise, the author has given us such mental gems as these:

"Leaves in autumn reveal concealed, locked-up colors as they wither away—so do men and women as they return to the elements—illuminative energy has fulfilled its fate and reverted to other functions in the plans of creation."

"The Earth with its lights and shadows of life is outward bound with that wanderlust which first detached it from a diamond cloud of sundust and sent it reeling among the stars. And as it speeds on through the horizonless night shades of the Milky Way—over seas of darkness—a midget race of human species attempts to fathom the realisms of material things, and often those of the spirit, hoping ever for a flight from destiny."

Amid such delightful, thought-provoking passages as these, however, are numerous lengthy, involved sentences which would prove to be work-outs even for an English professor. For example:

"The cosmology that has been developed by modern science in gravitational and electrical fields, upon living protoplasm and organisms, on mind and matter, the Soul, on heredity and genes, over the mechanics of being, and the origin and evolution of life on Earth are summarized by the intellect of the scientist in the resonant and tremendous philosophical retrospection of faded ages, the neoversions of brilliant centuries, and the most recent conclusions of man undulating through the misty clouds of fact, fancy and romance." (pp. 120–121.)

The preface of the volume is misleading, for it claims the book to be "the story of our world—of its Geology and Natural History." It should not be called a story, for it is not that. Rather, its chapters are a series of essays and treatises, unsequential, rambling, and more or less unrelated; they are conducive to mental contemplation of the mysteries of Time and Space, Life and Death. To appreciate fully the philosophies

of science, the reader must have more than a passing acquaintanceship with the various physical and biological sciences; and if he has this, he will discover a number of errors, misstatements, outmoded beliefs, and misspelled words, as well as theories and opinions which are presented as facts. A few may be cited:

The author states that the moon does not turn on an axis and presents only one side to the sun (p. 31); he gives the impression that there are only eight planets (p. 15); and believes that the interior half of the earth is a molten mass (pp. 23, 24, 35. 181). Regarding fossils, he indicates that the mastodon became extinct in the Pliocene Epoch (p. 91), gives the impression that complete fossil skeletons and vestiges are unknown from rocks older than the Tertiary Period (p. 63), and uses the word "equestrians" for an order of Coal Age plants (p. 109). Apparently he does not know that man has penetrated, by means of shafts and wells, to depths much greater than 6000 feet (p. 13). Among the misspelled words are "planetismal" (p. 15) for *planetesimal,* and "cyclad" (pp. 53, 109) for *cycad.* Probably the most conspicuous errors occur in respect to the relative ages of the geologic eras, periods, and epochs. According to the author, the Cretaceous Period and the Age of Dinosaurs are distant by 30,000,000 years (pp. 1, 28); yet the Cenozoic Era, which follows the Cretaceous, began 60,000,000 years ago (p. 88). He indicates, also, that the Silurian Period is distant by 130,000,000 years (p. 72); but the Devonian, which follows the Silurian, is over 500,000,000 years away (p. 105)! Other errors and inconsistencies occur with respect to the ages, but it would be superfluous to detail them. Although a few millions of years, more or less, are of little consequence in the enormous magnitude of geologic time, there is no excuse for jumbling them into abnormal superposition.

In spite of inaccuracies such as these, the advanced reader may well find the book to be, as the publishers suggest, "a mental banquet." Certainly he will be drawn to a contemplation of two stupendous, opposing philosophies concerning the nature of our earth: "Living, and even Mind may be but cosmic accidents (p. 187); or, "In a lonely corner of the universe the Earth and its life may yet be but passing phases in the schemes of a greater Mind beyond" (p. 183).

RAYMOND E. JANSSEN

DEPARTMENT OF GEOLOGY
MARSHALL COLLEGE

COMMENTS AND CRITICISMS

"When the Comet Struck America" vs. "Neptune's Racetracks"

The April number contains a very readable fantasy by Dr. C. Wythe Cooke, Senior Geologist, U. S. Geological Survey and Editor of the *Journal of Paleontology*. The purpose of his article is to refute the theory of extraterrestrial bombardment as the explanation of the origin of the parellel, elliptical "bays" of the Carolina coast by the substitution of his own hypothesis.

Briefly, Dr. Cooke states that the land rose, exposing a plane covered with ponds of various shapes and sizes. A strong, steady wind blew from the glacial ice sheet and struck the surface of these ponds. Where the waves struck a shore obliquely, a long-shore current was produced. There had to be a reverse current to stabilize the surface. This returned along the opposite shore, completing the rotation. In case two currents were started, they met at the far end of the pond and returned up the middle, building a dividing sandbar at they came. In the first case there existed a whirlpool turning in one direction; in the second there were two whirlpools turning in opposite directions, but in the same pond. These whirlpools took on the properties of a gyroscope which, considered in connection with the latitude, determined the direction of the long axes of the ponds. No duplication of this phenomenon is known throughout the earth. He submits three excellent aerial photographs which refute his theory.

Has anyone ever seen water whirling in a pond due to wind action? Can anyone produce such a phenomenon in a laboratory? I asked Dr. Cooke these questions. He replied that he had never made laboratory experiments on the shape of eddies because he did not have access to the necessary facilities, but that he had actually seen water rotating in a lake in Florida. At least: "The water must have been rotating, either as a whole or in eddies; otherwise the water would have piled up, and its surface would not have remained in equilibrium."

Apparently Dr. Cooke overlooks certain facts: wind acts on the *entire* surface of a pond; water does pile up at the far end to a limited extent, and the return flow is subsurface—not a return surface current of rotation; the ponds have to be approximately the shape that he tries to account for in order to have any currents. Take, for example, a triangular pond with one of its angles facing the wind; obviously no long-shore current could result since there would be no oblique shores for the waves to strike. The photographs show no exceptions; every pond apparently was perfect for the accomplishment of his theory. I asked the Dr. if, as he states, the returning central current could build a sandbar which would make two ponds where one had existed before, why would not this repeat, resulting in numerous long ponds. His answer was:

"I suppose that a long-continued eddy would tend to split up into smaller eddies. In fact, Figure 2 of 'Neptune's Racetracks' shows four well-defined ellipses that probably originated in this way. However, this process of splitting would not continue indefinitely, for the gyroscopic action continues only so long as the wind blows, and the current continues to flow. Moreover, the splitting is opposed by the inertia of the moving water."

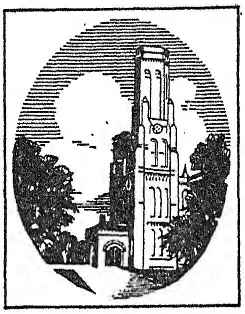
Look at the photographs. The ponds could not have been of various shapes—they were all suitable for forming elliptical ponds. If Figure 2 shows four ponds that were made by the division of one, why are there large ponds that have not divided at all, as shown in Figures 1 and 3? If you consider that the four ponds shown in Figure 2 had originally been one, the shape would preclude rotation; also the bar separating the two center ponds should be wider since this would have been the primary division. This is opposite to what the photograph shows. Ponds produced by splitting should be of equal length. These outer ponds are shorter than their supposed twins. He stated that ponds would not continue dividing because: ". . . the gyroscopic action continues only so long as the wind blows, and the current continues to flow." His original hypothesis does not mention gyroscopic action in connection with the formation of the ellipses, but merely in their *direction*. If the wind blew long enough to form the four ponds of Figure 2, then why was a similar result absent in the large ponds shown in Figures 1 and 3? There is no indication of any partially built dividing bar in any of the photographs.

There is no object in going into the gyroscopic phase of his hypothesis other than to mention that he says in his letter: "Strictly speaking, I suppose it is improper to call this effect gyroscopic. . . ." He is unable to give any reason why the water should have revolved or even how it would be possible to reproduce such a miracle in a laboratory.

Wind striking a pond does not cause the water to rotate. Dr. Cooke's entire hypothesis is based on a fallacy.—Chapman Grant.

We accepted the "readable fantasy" by Dr. Cooke not knowing that we were to become involved in a scientific controversy. When Major Grant, a herpetologist, took issue with Dr. Cooke, a geologist, on a geological question, we put our hypothetical money on the latter. But the Major was undaunted; he came up with a brand-new hypothesis of his own, which, contrary to our own rules, we expect to publish in the SM.—Ed.

THE BROWNSTONE TOWER



THE time and place are not important, but because of other circumstances I remember that Mr. Christensen's proposal was made on February 10, 1945, about 1 P.M., as he and Dr. Moulton and I were walking along Twelfth Street under the semicircular colonnade of the Post Office Department. We were returning to the Smithsonian after lunch —the last I was to have downtown for many a day. As our banter ceased, Mr. Christensen became serious and popped a question that had long been on his mind. "Would you like me to become book review editor of the SM?" he asked. "I'd be glad to take the responsibility for getting reviews and preparing them for publication," he added. "Sold!" I exclaimed, and Dr. Moulton agreed. So Mr. Christensen became book review editor of the SM in addition to his duties as advertising manager of *Science* and the SM.

Only four days later I lay on my back suffering from rheumatic fever, one of the sequels of scarlet fever. Spring came and summer was approaching before I saw the brownstone tower again. During those three weary months I wrote "The Brownstone Tower" but did little else for the SM. Dr. Moulton acted as editor and Mrs. Scovill and Mr. Christensen did the rest. Now I want to thank them publicly for their effective work.

Our book-review policy is based on unscientific assumptions. Lacking a Gallup poll, we assume first that book reviews are desired by our readers. Then we assume that we should limit our reviews to books that are similar in character to the contents of the SM; that is, to books that deal with or pertain to science and lie in the popular field between highly technical monographs and elementary textbooks. Finally, we make assumptions on the number, length, and character of the reviews that we should publish each month. As it does not seem feasible to get and publish reviews of all new books that may lie within the scope of the SM, we announce some of them only in the form of brief notices, one or two columns of which are published each month on page iii or iv. These notices are now written by Mr. Christensen.

Most books received for review in the SM are sent to us voluntarily by their publishers, but not all of these seem appropriate for review. Some are merely noticed and others are ignored. Unsolicited books that are not sent out for review are given to the library of the Smithsonian Institution. Few solicited books fail of review; those few are returned to the publishers. Every book reviewed for the SM becomes the personal property of the reviewer.

The quality of our reviews varies at least as much as the quality of the books reviewed. We shun reviewers who give us a dry inventory of contents, who continually gush praise or find fault, or who pay little attention to a book but use it merely as a platform for their sermons; we favor reviewers who pursue a happy medium, disclosing the nature of a book as they comment on it with illustrations drawn from their own knowledge. One of the greatest virtues of a reviewer is promptness. For the author and publisher the value of a review decreases as the interval between publication of the book and its review increases. A reviewer who follows the Golden Rule will return a book if unforeseen circumstances prevent him from reviewing it before the deadline agreed upon. Sometimes while examining a book a reviewer finds himself allergic to it. In such cases we think that the promise to review should be revoked and the book returned to us; another person may not be so affected.

We have never set an inflexible limit to the length of our reviews but we think 1000 well-chosen words ought to be enough to characterize and evaluate a book and that fewer should usually suffice.

We are trying to increase the number of those who review books for the SM, though it is a temptation to send books to the old reliables. The usual complete transaction requires three letters: (1) Will you do it? (2) Thanks for your promise; the book is on the way. (3) Thanks for your review. Item (1) can sometimes be eliminated by an enterprising person who writes: "If the new book by ———— entitled ———— is available and unassigned, I should like to review it for the SM." Letters such as this and all other letters concerning SM book reviews should be addressed to Mr. T. J. Christensen, AAAS, Smithsonian Institution Building, Washington 25, D. C. He is a good man.—F. L. CAMPBELL.

THE SCIENTIFIC MONTHLY

OCTOBER, 1945

SCIENTIFIC BEACHCOMBING*

By O. F. EVANS

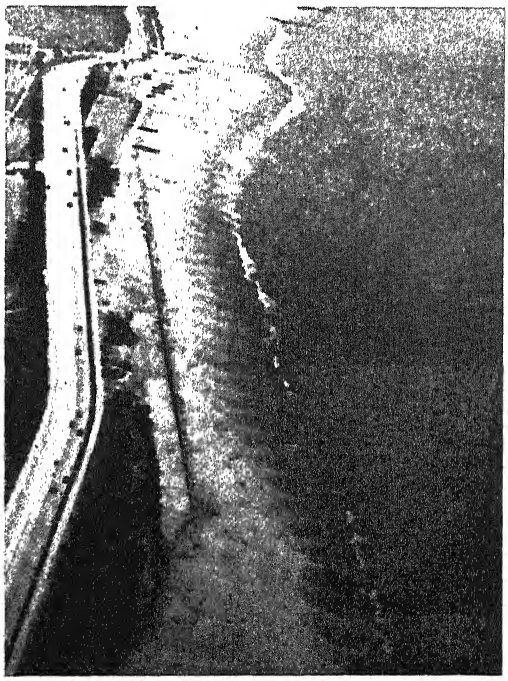
PROFESSOR OF GEOLOGY, UNIVERSITY OF OKLAHOMA, NORMAN

All through the August night, a light wind had fanned the waters of the lake. Sparkling in the moonlight, a continuous procession of small waves had marched in, only to break against the long, clean, sandy beach in front of the cabin. I had watched for several hours before turning in, and tomorrow, I had told myself, tomorrow I bet I'll find out. Conditions were just right. So, in the early morning I dressed hurriedly and started for the shore—hoping, as so many times before, to find some clue to the solution of the problem.

It was an old problem, this question of how beach cusps are formed, and had been the subject of discussion, study, and experiment by geologists and other nature students for more than a century. Many articles had been written and many theories advanced, by both laymen and scientists, yet the answer seemed as far away as ever.

And it was a tantalizing problem, almost maddening in its simplicity. The processes involved were few and well known, and the forming of the structures certainly took place in plain sight on the open beach. Yet, strangely, no man could say he had seen their building.

Such a problem becomes a game, filled with all the hopes, thrills, and disappointments of the hunt. You may follow the trail for days, using all the tricks and devices suggested by a long experience, without get-



Spence Air Photo

BEACH CUSPS[1]

WHY IS THIS SANTA BARBARA SHORELINE SCALLOPED?

[1] This photograph was kindly provided by Dr. Martin A. Mason, Beach Erosion Board, War Department, Washington, D. C.

* In "The Brownstone Tower" of the SM (April 1945) the editor called for stories of detection in which customary scientific reserve would be broken down and the drama of scientific discovery played up. "Scientific Beachcombing" is the first response to the editor's appeal. Previously Professor Evans published for specialists a detailed, conventional account of his discovery (The Classification and Origin of Beach Cusps, *Journal of Geology*, 46: 615–627. 1938); now he presents a part of the same story dramatized for laymen. The editor will be glad to consider for the SM other vitalized stories of research.—Ed.

ting a glimpse of the quarry. Then there suddenly comes the elation of discovery after discovery.

But this was the most baffling of anything I had tackled in a lifetime of research. I used every known trick of the trade. I searched the literature, I made experiments, I haunted the beach for weeks. And from the illiterate fisherman to the erudite mathematical physicist, I pestered my friends with questions. But results were discouragingly few.

Despairingly, I almost gave up a dozen times. Sometimes I would think, "It is, after all, only a 'pure' science problem. No one's life depends on my finding the answer; neither will anyone, including myself, be the wealthier for its solution."

Yet I never quite gave up. In such times of discouragement, because I love the sea, and the sound of its waves quiets and soothes me, I was always drawn again to the beach. There, as I watched the waves, some new angle of the problem would always show up, and, with new enthusiasm, I would be off again on a new trail.

Thus for three months, since early in June, I had followed the sandy shores of the lakes, studying the wind, the waves, the currents, and the shoreline, trying desperately to wrest from nature the jealously guarded secret.

Thus, hopefully, yet with little real expectancy, I looked at the broad expanse of sandy beach and quiet lake that August morning. Thoughtfully, I strolled along, noting every movement of the water, every change in the shore, and every new combination of wave effects.

With increasing interest, I saw that during the night the waves had built a little ridge, two or three inches high, along the edge of the water. But there was nothing unusual about this one. It followed along just above the water's edge, without a break, except where a quick turn in the shoreline had prevented the waves from doing their work. There, instead of the ridge, was a gentle, even slope of water-washed sand, along which the little swashing breakers traveled.

Alert, through long practice, to every change in sound or movement of the water, my ears caught a changed note and timing in the murmur and plash. A new breeze was moving in from the lake, and the waves, outrunning it, were just reaching the shore.

Almost subconsciously, I noted that these new waves were a little higher and longer than those that had built the ridge. As the first one curled against the shore, water from its slightly uneven crest splashed, here and there, across the ridge's top. The next wave did the same, and by the time the third broke, it was certain the ridge was being divided into segments.

That was what I had been looking for! I watched, all attention, as the swash and backwash continued and the spacing of the breaks became more equal. To my astonishment, within a minute and a half the process was complete. A perfect series of beach cusps had formed. Indeed, had I happened along two minutes later, I would have supposed them to be many hours old.

The thing was so sudden, I stood for a couple of minutes gazing at the newly formed cusps, before the full significance of the event burst upon me. I had just witnessed a process in nature never before seen by human eyes; at least by any eyes that understood what it was they saw. My first impulse was to rush up and down the beach, shouting the good news.

But the inhibitions of a long professional life are strong. Besides, no one was in sight at that early hour, and I felt sure that those still asleep in the cabin would not be too appreciative of being awakened, only to be told that I had seen the waves splashing in a new way against the beach. Also, with some uneasiness, I saw that the wind was rising and that the waves might soon sweep the beach clean and smooth again. So I smothered my emotions and set to work at once with measuring tape and notebook.

Now, at last, I understood why the solution of the problem had eluded me so long. The delicate balance of factors entering into the process, combined with the rapidity of the action when the exact adjustment is reached, almost completely masks the operation.

The start of the process depends on the ability of the waves to break the ridge into segments. This they can do because of the

slight inequalities in the height of the wave crest. But if the waves of the new series are too large, the beach ridge, instead of merely being broken, is completely destroyed, but if they are not quite large enough to overtop the ridge, they just build it higher.

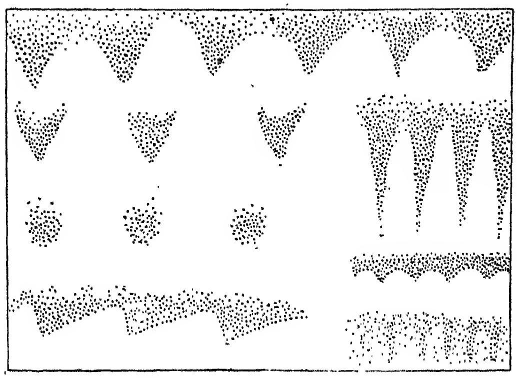
The breaks, at first, are spaced quite unevenly. But some are larger than others, and the parabolic currents of the swash and backwash have an equalizing effect on the spacing. Thus the larger breaks become the focal centers which determine the spacing as some of the smaller ones fail to develop.

And the process is remarkably rapid. A dozen waves, uniform in height and of the right size, are sufficient to complete the process and give the cusp series all the appearance of old age. And even now that I understand the process, this almost unbelievably delicate adjustment is still a source of astonishment.

Seven years have passed since that memorable August morning, yet never again have I seen the complete process of cusp formation go on to completion. Occasionally I see it start, then fail to finish because of some change in strength or direction of the wind. Also, I think I have been present a few times at the finish, although it is more difficult to be sure of this.

However, now that I know the process, I can perceive the truth or falsity of many of the beliefs held by former students. Sometimes the cusps build along a low cut bank; its edge acting as the crest of the beach ridge, and I have become convinced, through examination of hundreds of cusp series, that they form only when some sort of ridge is present; never when the beach slopes uniformly up from the water to beyond the limits of wave action.

The cusps I saw forming were perpendicular to the shoreline, but I have discovered, by many anxious days of shore watching, that if the adjustment between waves and ridges is sufficiently close, cusps may be formed with their axes at almost any angle to the shoreline. And it is still more surprising that a change in wave direction sometimes brings a change in this angle without destroying the cusps, although under slightly different conditions they will be destroyed



VARIOUS BEACH CUSPS[2]
MANY VARIATIONS IN FORM HAVE BEEN OBSERVED.

almost instantly. Thus, on a north and south shore, a cusp series built by a west wind may be changed by a southwest wind to look like one built by a wind from the northwest. No wonder speculative observers have given so many conflicting accounts of what they saw!

A constant source of surprise is the variety in size, form, and spacing of the cusps. Some are long and slim, others short and broad. Some have sharp crests and steep sides, others are low and rounded. And they vary in size from little scallops to a series of veritable capes.

Perhaps, for the average observer, the most striking feature is the apparent uniformity in spacing, which has even led some scientific observers astray. Somehow, symmetrical regularity of feature seems out of place on a beach, and maybe the unconscious comparison induces a sort of optical illusion.

Careful measurements of my cusp series gave a spacing of 1.7, 1.6, 1.6, 1.9, 1.4, 1.0, 1.3, 1.6, and 1.4 feet. Thus the variation in spacing was 90 percent although, at first glance, it seemed almost uniform. In measuring larger cusp series, some of which had a spacing of over 20 feet, I found variations ranging from 40 to 147 percent.

However, with the finish of my field observations, my troubles were not over. The question at once came up, "What is a beach cusp?" Going back again to the literature of the subject, I found that a beach cusp is

[2] Reproduced by permission from page 465 of the late Douglas Johnson's *Shore Processes and Shoreline Development*, John Wiley & Sons, New York.

defined more by inference than by direct statement. It is clear that beach cusps are so named because of their shape. They are merely points extending out into the water. Thus it seems that any point of sediment, more or less temporary in nature, sticking out into the water from the shore, has a right to be called a cusp.

Now it is perfectly certain that not all such points of sand are formed in the way I observed, and not all of them occur in series. So, because of looseness of established definition, I was forced to make a classification of beach cusps, including some forms that I do not really think belong there. Thus, as is not unusual, I was forced in my scientific writing to defer somewhat to the generally accepted, though sometimes erroneous, statements of earlier writers.

"But now you've found this out, what's its value and how can we use it?" you inquire; a fair question, and one that could well be asked at the finish of any investigation.

Here is your answer. In the long history of the earth, the sea has always been advancing and retreating across the continents. Sometimes this movement is relatively rapid, again it is very slow. As it goes on, many shore structures are built, of which the beach cusp series is one. And sometimes such structures are solidified and preserved in formations like the Medina or the Potsdam sandstones. Finding such a fossilized beach cusp series on an old shoreline and knowing how it was made, we can say what was the direction of the wind, the size of the waves, and something about the shape of the beach on that far-off day in the earth's history. And so we are brought another step nearer to knowing how this old earth was made.

OREN F. EVANS

Oren F. Evans, Ph.D., is Professor of Geology in the University of Oklahoma. He writes:

"The one thing I have in common with many of the famous men of my generation is that I was born in a log cabin. That most important event occurred on a June day 67 years ago in a clearing in the woods a few miles from Shelby, Michigan.

"That country was pretty new and crude in those days, but I have sometimes thought that many of the things I learned, as a pioneer boy, among the hills, woods, and lakes of Michigan have been as valuable to me in my work as a field geologist and teacher as a lot of the more formal training that came later from the schools.

"I began teaching at an age when, according to modern standards, I should have still been in high school. I taught for a number of years, going to school between times. As a result, my college education was considerably broken, and because of changing conditions and changing desires, I was a student at four different institutions of higher learning: Purdue University, Michigan State Normal College, Albion College, and the University of Michigan. A few years after graduation, I was called back to Albion College, where I taught mathematics and physics two years. In 1920 I joined the faculty of the University of Oklahoma, where I am still working as a Professor of Geology.

"My summers, for many years, have been devoted to physiographic research, mostly along the line of shore processes. I have been a contributor to the *Journal of Geology, Science, American Journal of Science,* and *Journal of Sedimentary Petrology* as well as having written a number of semi-popular scientific articles." In one of his letters Dr. Evans added: "For a number of years, I have been pondering this matter of popular scientific writing and finally decided to do something about it, at least in my own science. So for two years now, I have been training under two of my author friends, Stanley Vestal (Professor W. C. Campbell) and Foster-Harris. This accounts for the techniques of the fiction writer which I used in 'Scientific Beachcombing.'"

SAFEGUARDING OUR SEAWAYS—THE MODERN NAUTICAL CHART

By A. L. SHALOWITZ
PRINCIPAL CARTOGRAPHIC ENGINEER, U. S. COAST AND GEODETIC SURVEY

To all peoples whose territories touch the sea or who have any interest in the commerce of the sea a full and complete knowledge of the coast, its nature and form, the character of the sea bottom near it, the locations of reefs, shoals, and other dangers to navigation are of the greatest practical value. Modern nautical charts supply such information.

Of the total area of the earth's surface the oceans occupy nearly three-fourths, affording highways open to the nations. To conduct international commerce by water the ships of one country must enter the ports of another, so that on the open sea and in the harbors there is an interest, common to seamen of all nationalities, in the publication of charts. Progressive maritime countries have therefore long recognized that one of their important political obligations is to survey their coasts and to publish charts showing the results of such surveys.

While many innovations have been introduced in the art of navigation, all of these would be of little value were the charts permitted to deteriorate in accuracy. Radio bearings may enable the mariner to locate his position far out at sea, but he must have a chart on which he can accurately plot that position and determine his relationship to surrounding submarine features and to the topography of the land.

An important distinction exists between the nautical chart and maps in general. Whereas the latter may serve as reference mediums, the nautical chart in its special and accurate delineation is an instrument to be worked upon so that a ship's course may be laid off with accuracy and ease, and positions readily determined.

From Ptolemy to Mercator. Although the modern chart is of comparatively recent origin, the period from Ptolemy to Mercator saw three great developments in cartography that have profoundly influenced contemporary chart making. Claudius Ptolemy—Egyptian mathematician, astronomer, and geographer—who lived in the early part of the second century, stands without doubt in the front rank of early geographic thought. His *Geographia* represented the sum of all geographic learning and served as a groundwork for future cartographers. Ptolemy gave details for the construction of 26 maps and a general world map and is credited with being the originator of the conic projection and with being the first to divide the earth by means of meridians and parallels.

Another great development in cartography came towards the close of the middle ages and forms a notable exception to the prevailing darkness of that period. The Italian and Catalan chart makers produced what were known as The Portolanos, or "handy plans." These charts came into being generally with the introduction of the compass in navigation. No projection was included, but in its place there were networks of straight lines, each network radiating from a common center like the spokes of a wheel and corresponding to the points of the compass. The Portolanos achieved only an approach to mathematical accuracy, but it was enough to give the seamen of that period the confidence which they needed to sail the open sea. It remained of course for Mercator, 150 years later, to *solve* the problem of cartography for the navigator.

The influence of the Portolanos on chart making was felt for several centuries after their introduction, and Juan de la Cosa in 1500 still covered his chart with the spiderweb lines (Fig. 1). Cosa accompanied Columbus on his first voyage as master of his flagship and as cartographer on his second voyage. The Cosa chart is of great interest historically, being the earliest map now extant which shows the American coast.

The third great influence on the modern nautical chart was the contribution of Gerhard Kremer (Mercator), the Flemish mathematician and cartographer who lived in the

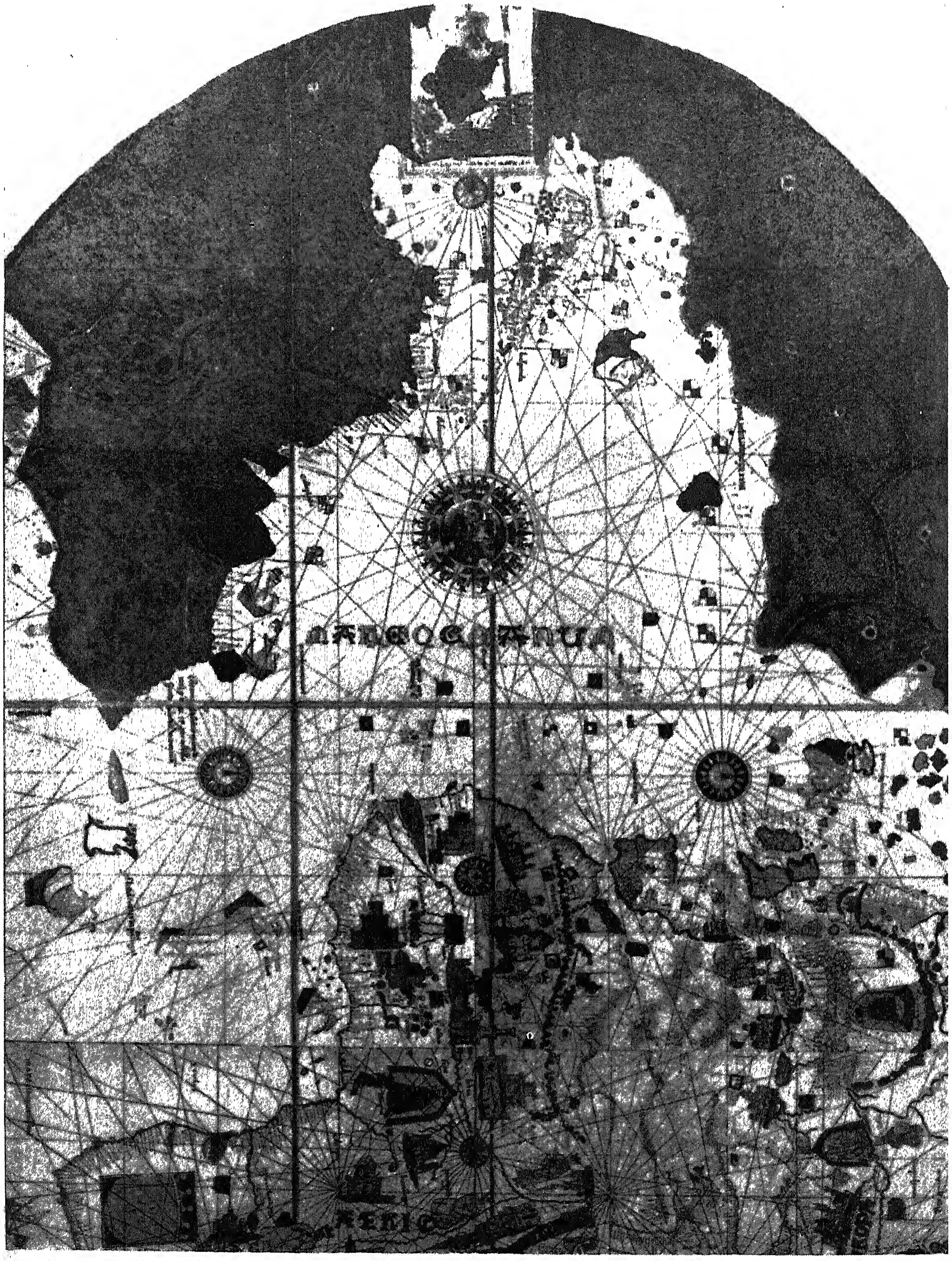
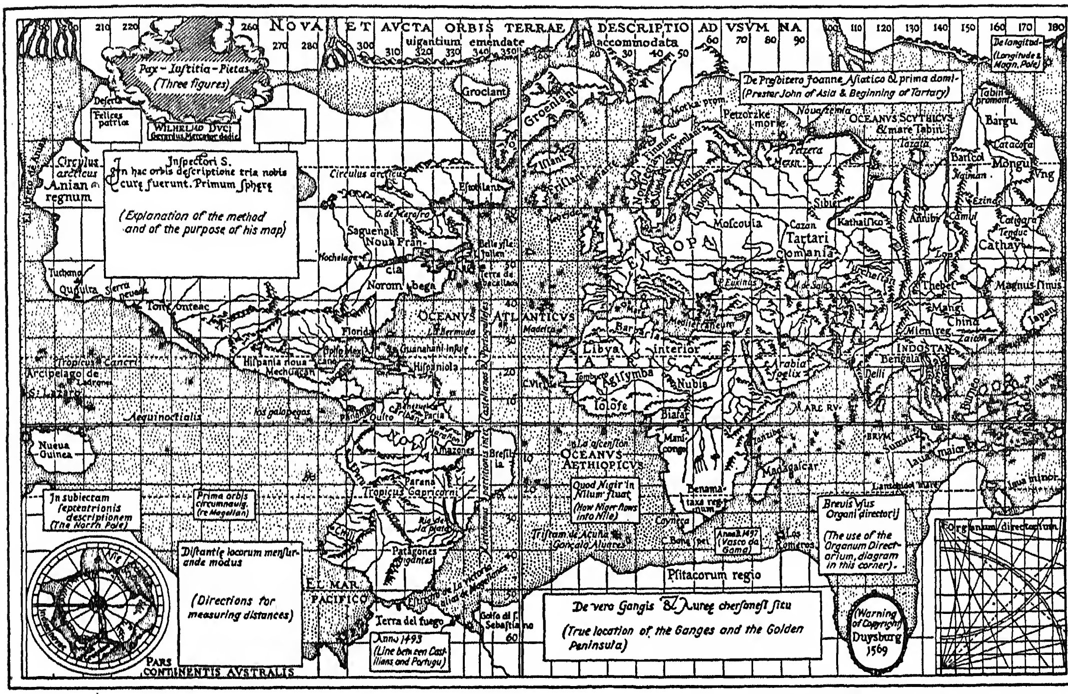


FIG. 1. CHART OF NORTH ATLANTIC OCEAN BY JUAN DE LA COSA, 1500
THE COSA CHART WAS DRAWN ON OXHIDE AND IN BRIGHT COLORS. THE RADIATING LINES, WHICH CHARACTERIZED THE PORTOLANOS, ENABLED THE NAVIGATOR TO SET HIS COURSE AT ANY POINT BY AID OF THE MAGNETIC NEEDLE. TO RECOGNIZE THE WESTERN COAST OF EUROPE AND AFRICA TURN THE MAP, RIGHT-HAND SIDE ON TOP (NORTH).



Courtesy Encyclopaedia Britannica

FIG. 2. MERCATOR'S WORLD CHART OF 1569

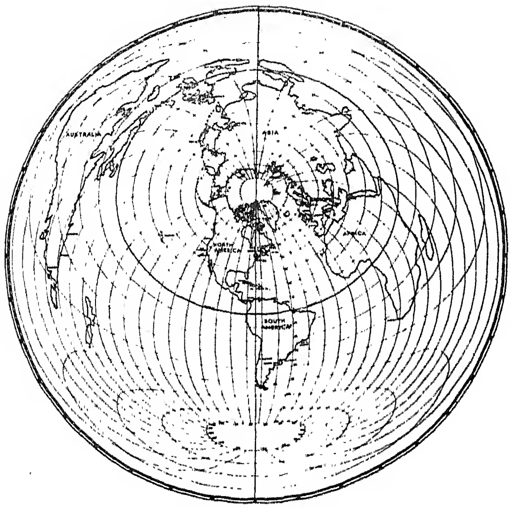
ON HIS ORIGINAL CHART MERCATOR ADVISED: "IF YOU WISH TO SAIL FROM ONE PORT TO ANOTHER HERE IS A CHART AND A STRAIGHT LINE ON IT, AND IF YOU FOLLOW CAREFULLY THIS LINE YOU WILL CERTAINLY ARRIVE AT YOUR PORT OF DESTINATION. BUT THE LENGTH OF THE LINE MAY NOT BE CORRECT YET IT POINTS IN THE RIGHT DIRECTION. CONSEQUENTLY IF YOU ADHERE TO THE LINE YOU MAY GET TO YOUR DESTINATION SOONER OR YOU MAY NOT GET THERE AS SOON AS YOU EXPECT, BUT YOU WILL CERTAINLY GET THERE."

sixteenth century. Ptolemy, as we saw, introduced the latitude and longitude idea in maps. The Portolano chart makers of the fourteenth century neglected this concept and used the points of the compass as *their* "grid system." Mercator combined the scientific theories of the one with the practical advantages of the other and devised the well-known projection which bears his name. In his famous "World Map of 1569," the latitude and longitude lines are straight parallel lines intersecting each other at right angles (Fig. 2). The meridians of longitude Mercator spaced equally throughout his map, based on their distance apart at the Equator. This, of course, caused a spreading of the meridians everywhere except at the Equator, since meridians on the earth converge toward the Poles. To compensate for this, Mercator conceived the idea of also spreading the parallels in exactly the same proportion as he spread the meridians.

What Mercator sought to accomplish by this arrangement of meridians and parallels was to provide the navigator with a chart on which a straight line—the simplest of all lines—joining any two points would determine the course he must steer in sailing between those points. Such a line is known as a *rhumb line*. On the earth it cuts all the meridians at the same angle and is a continually curving line, always approaching the Pole but theoretically never reaching it. A ship sailing a "rhumb" is therefore on one course continuously. The uniqueness of the Mercator projection lies in the fact that on it and it alone the rhumb line is a straight line.

Although it took nearly a century for navigators to appreciate this property of the Mercator projection, today the projection is looked upon as one of the most useful ever devised for marine navigation and will likely be so considered as long as ships "sail the rhumb."

There are, of course, certain limitations



Courtesy General Electric Company

FIG. 3. AZIMUTHAL EQUIDISTANT MAP
DISTANCES AND DIRECTIONS ARE CORRECT FROM SCHENECTADY, N. Y., THE CENTER FOR THIS PROJECTION.

inherent in the Mercator projection which should not be overlooked. For example, a great circle—the shortest distance between two points on the surface of the earth—is a curved line on the projection. This means that radio bearings, which follow the paths of great circles over the earth, cannot be plotted as straight lines on a Mercator chart without correction. Also, as one recedes from the Equator, both north and south, the scale of the projection continually increases, reaching infinity at the North and South Poles. This expanding scale makes the projection unsuitable for charting the polar regions and limits its use for ordinary geographic purposes where equal-area representation is desirable.

But notwithstanding its limitations, Mercator's contribution will always remain one of the great landmarks in the development of nautical cartography.

Much criticism has been directed against the Mercator projection—particularly in recent years with attention focussed on long-distance aeronautical flights—because of its failure to embody some of the properties of other projections. In fairness to Mercator it should be stated most emphatically that for world mapping the "projection to end projections" has not yet been devised—nor will it ever be—for the simple reason that one cannot flatten the surface of a globe without distorting it. And as long as the surface is distorted, the component parts of the earth cannot be represented in their true relative sizes, shapes, locations, and directions. Any map projection is, at best, a compromise, and the choice of projection usually depends upon the purpose which the map or chart is to serve.

One hears much these days of the "azimuthal equidistant projection" as though it were the fulfillment of a cartographer's dream. A glance at Figure 3 will show how erroneous this is. Note, for example, the distortion in both shape and size of Australia. In reality if Australia were superimposed on the United States it would roughly coincide with it. This projection, however, does possess the useful properties that straight lines radiating from the center represent great circles in their true azimuths from it and that distances along these lines are true to scale. But these properties do not apply to other portions of the map. In the Mercator projection, the straight rhumb line is the essential property to be preserved and other properties are subordinated to it. To preserve this property Mercator introduced the distortion in the higher latitudes. To consider this distortion as a weakness of the projection is to overlook completely the purpose for which it was devised.

The Development of Systematic Surveying. The great explorations which followed the discovery of the New World added immensely to the meager geographic and hydrographic knowledge of the times, and the information thus acquired was compiled both in chart form and in the form of sailing directions. But these were largely the contributions of individual effort and private undertaking. The results obtained were in many cases commendable and meritorious, but in others there was much to be desired. The early chart of New York Harbor is an example of the charts of that period (Fig. 4). This chart, like all of the early maps, suffered from two great defects—the want of detailed surveys and the lack of a rigid system of connection between the various ports.

It was not until the early part of the nineteenth century that governments began to

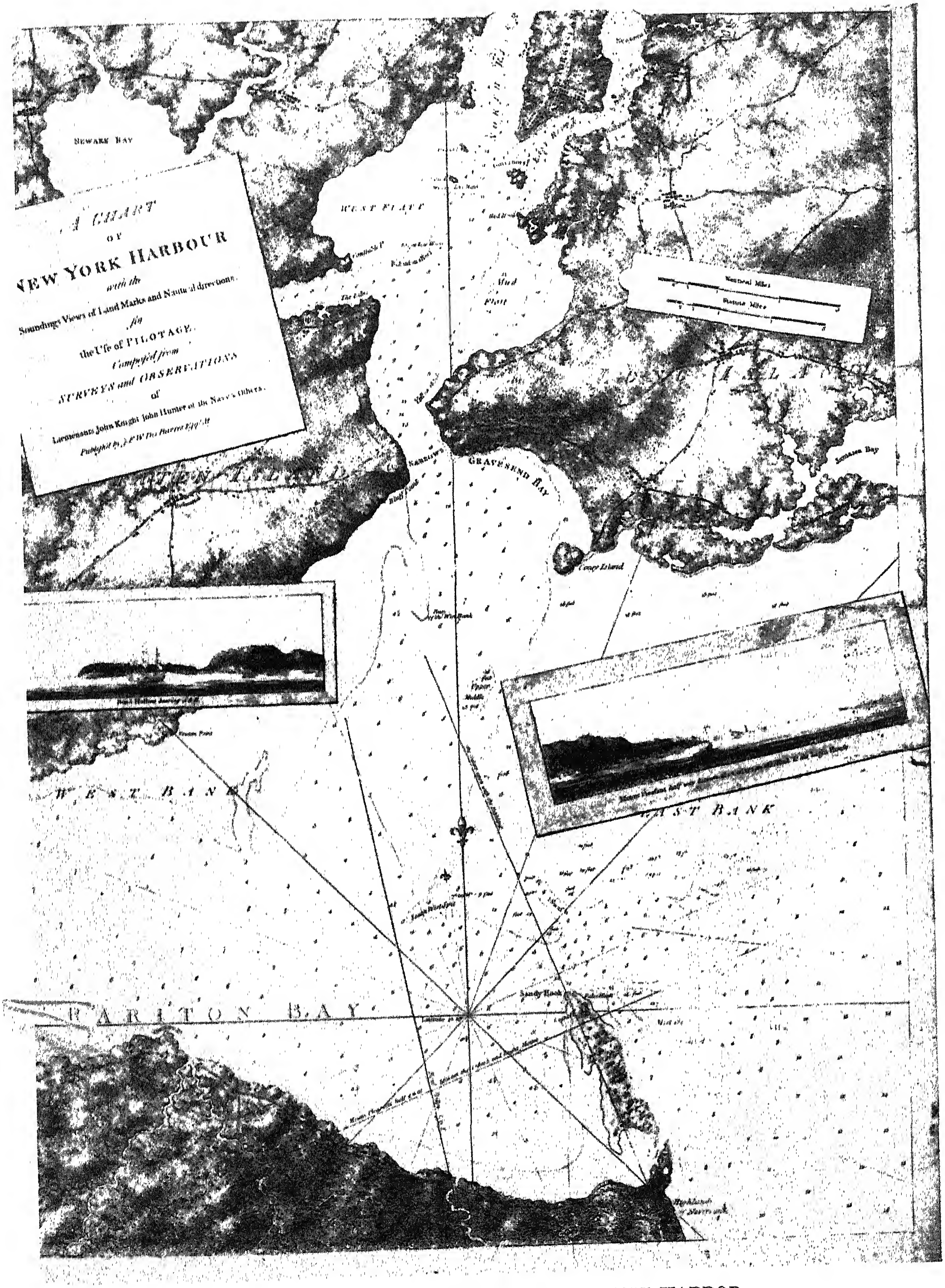


FIG. 4. AN EARLY CHART OF NEW YORK HARBOR
FROM THE ATLANTIC NEPTUNE COLLECTION, AN ATLAS OF CHARTS COMPILED BY DES BARRES, 1775 TO 1781.

recognize the wisdom of systematically surveying and charting their coastal waters as a necessary prelude to their commercial intercourse with other nations. It marked a new era in chart making and was the beginning of the accurate chart of today. In this country, the Coast Survey was created in 1807 to survey and chart the coastal waters of the United States, although actual work was not begun until a much later date.

The first chart published under the new governmental set-up was of Newark Bay, New Jersey, in 1835. This chart was a striking contrast to anything that had previously been called a chart of our coast. The outstanding improvements were high accuracy of geographic position, more thorough hydrography, and complete topography.

Charts of this period were entirely engraved on copper. Every line, every figure, and every letter was engraved by hand. The fineness of detail that was possible by this method of reproduction afforded the chart engraver an opportunity for artistic expression seldom equalled in any other method.

Many of the early charts had their margins adorned with elaborate views of harbor entrances and headlands for the guidance of the mariner, one of the finest of these being the view of Anacapa Island (Fig. 5) which was engraved by James McNeill Whistler, while employed in the Coast Survey. Whistler's stay in the Survey was brief but hectic. The rules of the office were soon found too exacting for his artistic temperament, and he became a habitual late-comer. When chided about it in later years he always replied, "It was not that I arrived too late in the morning, but the office opened up too early."

Since the publication of the first chart by the Coast Survey, the chart has undergone changes both in character and appearance, resulting mainly from improvements in methods of surveying and of reproduction. Physics, mathematics, the lithographic arts —all contribute toward producing a chart that the navigator can use with confidence.

Some of the more significant changes have been the adoption of a single depth unit for

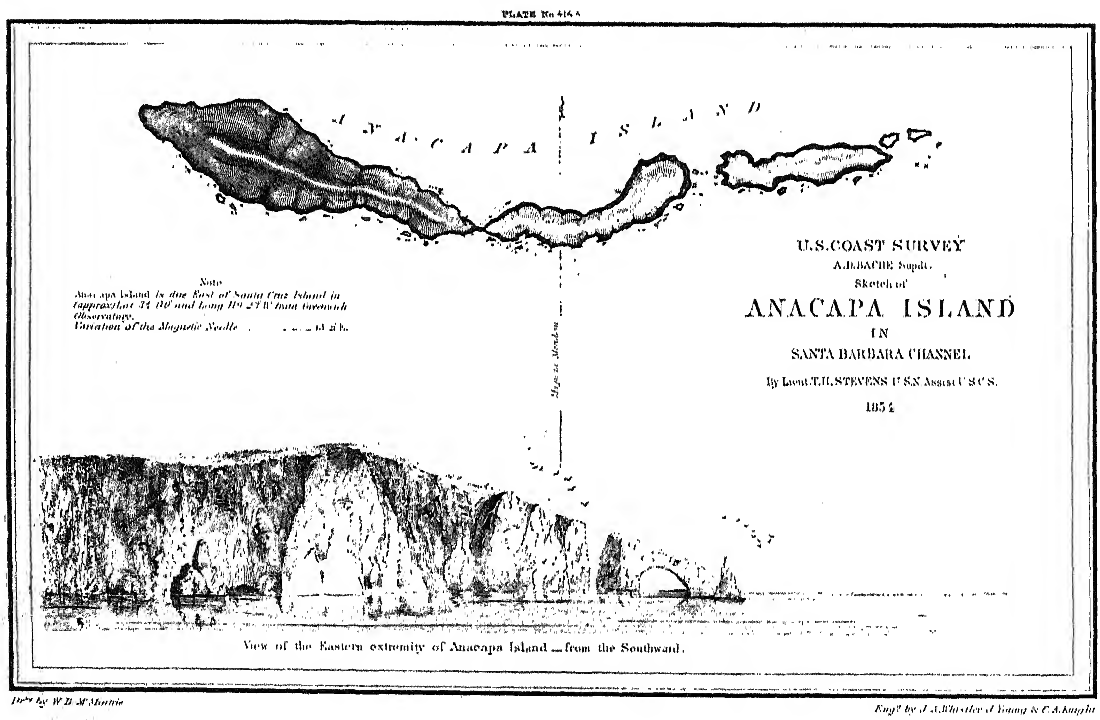


FIG. 5. ANACAPA ISLAND, CALIFORNIA
FROM AN ENGRAVING BY JAMES McNEILL WHISTLER, DONE WHILE HE WAS EMPLOYED BY THE U. S. COAST SURVEY.

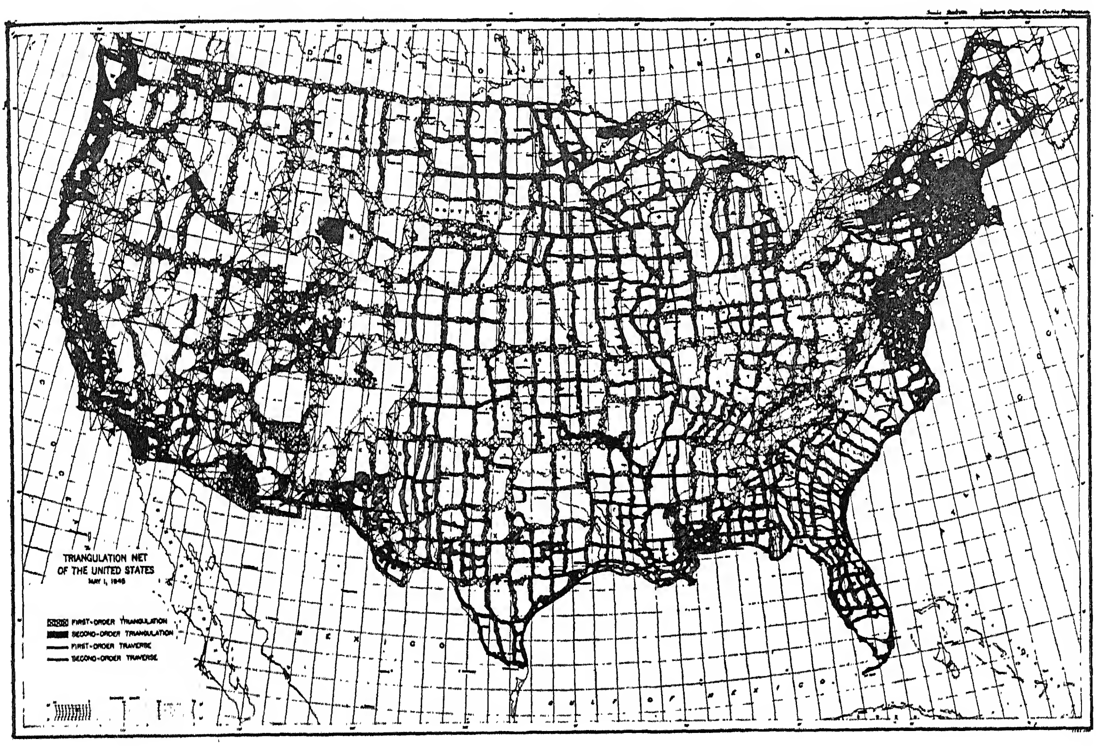


FIG. 6. TRIANGULATION NET OF THE UNITED STATES

97,000 LINEAR MILES OF FIRST- AND SECOND-ORDER TRIANGULATION FURNISH A RIGID FRAMEWORK FOR THE MAPS AND CHARTS OF THE COAST AND GEODETIC SURVEY, AND PROVIDE STARTING POINTS FOR LOCAL SURVEYORS.

any given chart to avoid confusion to the navigator; enlargements of the depth figures and the compass rose; the replacement of hachures by contours for showing topographic relief; and the adoption of a standard style for the nomenclature—vertical lettering for the land features, slanting letters for hydrographic features (Fig. 16).

The introduction of photolithography in chart reproduction has made it possible to use colors for emphasizing important navigational features. For example, the coloring of buoys to correspond to their colors on the ground; the accentuation of lighted aids to navigation by using a color overprint; and the use of tints for the land and shoal-water areas. The nautical chart of today is a synthesis of the utilitarian and the artistic, and is suitable for meeting present-day demands for quality and quantity production. Today each rotary lithographic press in the Coast Survey rolls off 3 to 5 thousand impressions an hour, as compared with the hundred-a-day maximum possible by the older method of printing directly from engraved copper plates.

Good and reliable charts can be made only from correct surveys. No one appreciated this more than Professor Hassler, the Swiss engineer to whom President Thomas Jefferson entrusted the important work of directing the survey of the coast. From the very beginning, Hassler insisted that all the harbor and coastal surveys be controlled by a backbone of triangulation (Fig. 6) that would knit together into one harmonious whole the surveys and charts of one locality with the surveys and charts of other localities. Notwithstanding the criticism heaped on him by certain politically minded gentlemen of the day, who could see no need for elaborate base measurements and triangulation systems, Hassler's scientific approach prevailed. And this tradition of accuracy which was inaugurated by him has been steadfastly maintained through a century

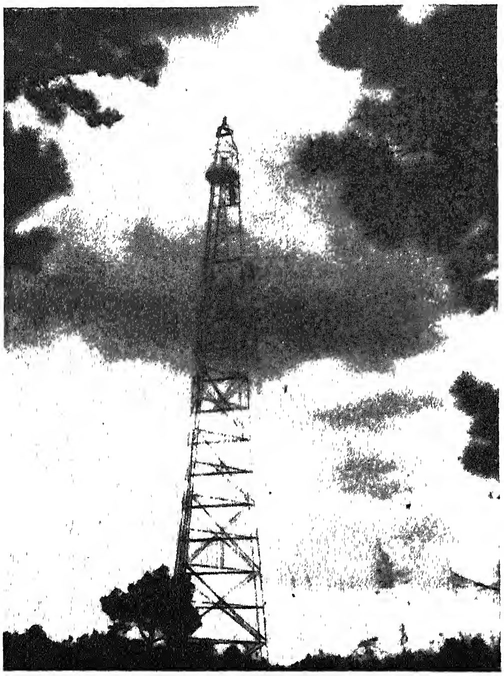


FIG. 7. 116-FT. TRIANGULATION TOWER
TO OVERCOME INTERFERENCE IN VISION DUE TO THE EARTH'S CURVATURE, FOREST GROWTH, ETC., THE OBSERVING INSTRUMENT MUST BE ELEVATED ABOVE THE GROUND. THIS STEEL TOWER IS PORTABLE AND CAN BE ERECTED OR DISMANTLED IN A FEW HOURS.

and a quarter of progressively increased activity.

Continuous, accurate triangulation has now been extended from the lower end of Mexico through the United States, Canada, the Yukon Territory, western Alaska, to the Little Diomede Island in Bering Strait. All of this work is based on a single geodetic datum, the North American datum of 1927, the initial for which is Station Meades Ranch in central Kansas. The establishment of this datum has resulted in the complete coordination between nautical charts of the Atlantic and Gulf Coasts and those of the Pacific Coast and Alaska—a most enviable position for any country to be in.

Improved instruments and equipment have accelerated the progress of triangulation work. When Hassler first started his work in 1816, he had a special carriage built to transport his great cumbersome theodolite (24-inch circle). The theodolites used today in first-order work (9-inch circle) can be carried by one man. Night observing on electric signal lamps and the portable steel towers (Fig. 7) have also contributed to this progress.

An important feature of the nautical

FIG. 8. MAPPING AN ALASKAN COASTLINE WITH THE PLANETABLE
THE PLANETABLER CONSTRUCTS HIS MAP AS HE SURVEYS. THE RODMAN ON THE POINT OF ROCKS IS HOLDING A "TELEMETER" ROD AND THE OBSERVER IS MEASURING ITS DISTANCE AND DIRECTION FROM THE PLANETABLE.

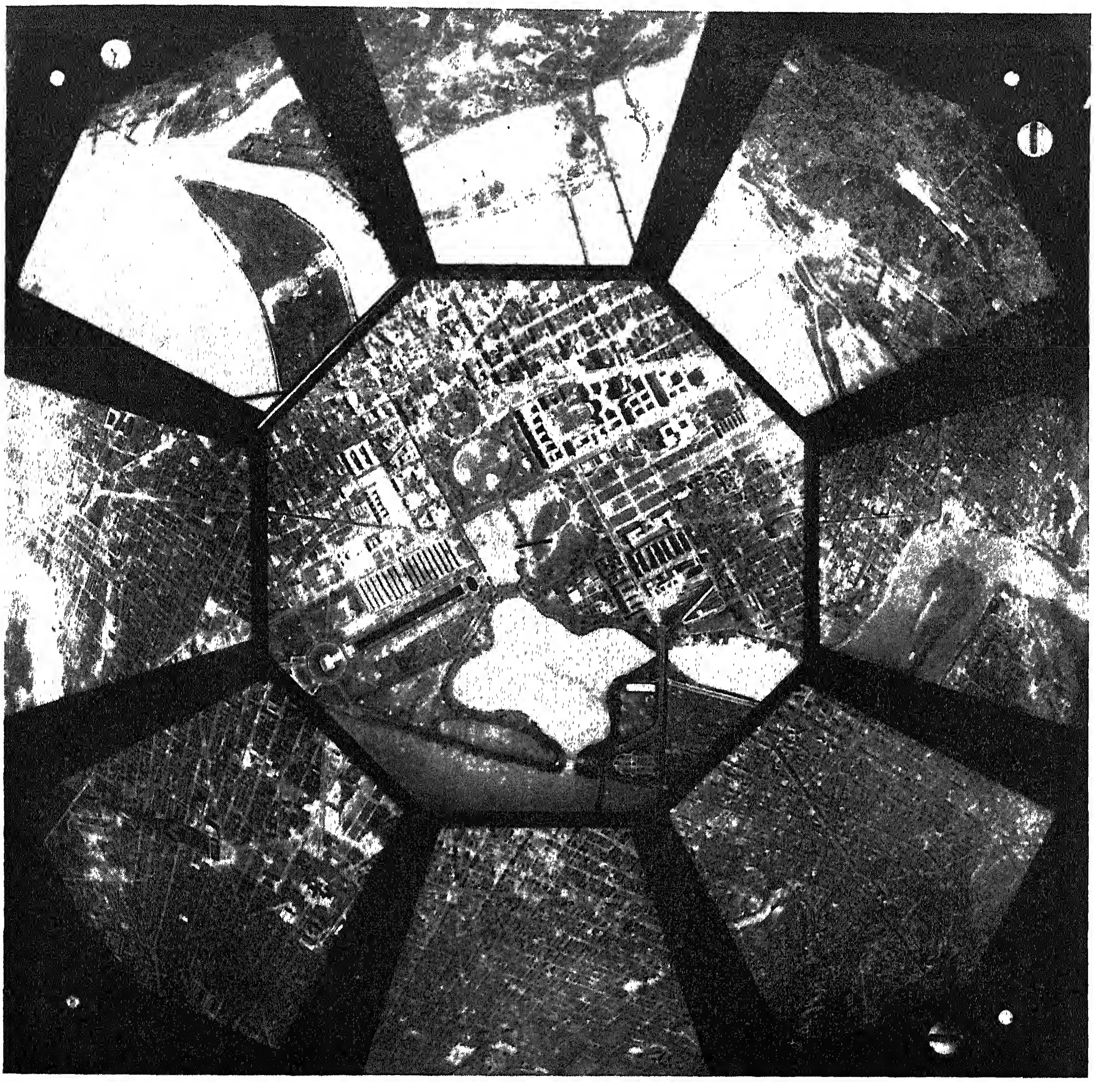


FIG. 9. CONTACT PRINT FROM 9–LENS AERIAL CAMERA
AT AN ELEVATION OF 14,000 FEET, APPROXIMATELY 125 SQUARE MILES ARE PHOTOGRAPHED IN A SINGLE EXPOSURE.

chart is the land area with its characteristic shore forms, its landmarks, its elevations and depressions. In piloting, a navigator relies a great deal on prominent shore objects to fix his position, and sometimes even uses the configuration of the shoreline for identification. For mapping from the ground the planetable is still the most satisfactory instrument (Fig. 8). The introduction of the "telemeter" around 1865 greatly facilitated the surveying of complex shorelines and gave the chart increased fidelity. Ground topographic methods are rapidly giving way to the more economical and more expeditious method of aerial topography (Fig. 9). The wealth of information and fullness of detail embraced in an air photographic survey cannot be matched by any other practicable method of surveying.

Hydrographic Advances. From the standpoint of the nautical chart the greatest advances made, since the publication of the first chart by the Coast Survey, have been in the field of hydrographic surveying. The correct charting of the water area is of supreme importance to the navigator because, unlike the land area, it involves fea-

tures hidden from his view. Modern hydrographic methods permit the accurate delineation of the ocean floor with its intricate patterns of submerged valleys, shoals, ridges, and plateaus. These characteristic features serve the navigator as permanent submarine "landmarks" for identifying his position at all times, irrespective of adverse weather conditions.

Hydrographic surveying consists essentially of the measurement of depth and the determination of the survey vessel's position at the time the depth is obtained—both of which are necessary in charting. The revolutionary advances made in this field during the past two decades have had a profound effect on the usefulness of the nautical chart. The old and well-known methods of sounding—the hand-lead and line for shoal water and the wire sounding machine for deep water—have given way to echo sounding. Echo sounding is an outgrowth of the experimental work begun in 1912 for detecting the presence and nearness of icebergs, and of the submarine detecting devices used during World War I.

Echo sounding is a method of determining the depth of water under a survey vessel by measuring the time required for a sound wave to reach the bottom of the ocean and return as an echo. We are all familiar with echoes in air and realize that they are caused by reflections of sound from some distant object. If the time is measured between the production of a sound and the reception of its echo, the distance may be determined by multiplying one-half the time interval by the velocity of sound in air.

The same principles are applied in measuring water depths by echo sounding. A sound is produced at the survey vessel near the surface of the water; it travels to the bottom, from which it is reflected back to the vessel as an echo. From the elapsed time and a knowledge of the velocity of sound in sea water the depth can be computed. Echo-sounding equipment is designed to produce the sound, receive the echo, measure the intervening time interval, convert the time interval into depths, and register these depths on a graduated dial (Fig. 10). As many as 20 soundings per second can be obtained in

Courtesy New York Daily Mirror

FIG. 10. THE DORSEY FATHOMETER—A VISUAL-TYPE DEPTH RECORDER

2 FATHOMS OR 2000 FATHOMS ARE READ WITH EQUAL FACILITY ON THE DIALS OF THIS PRECISION DEPTH INDICATOR.

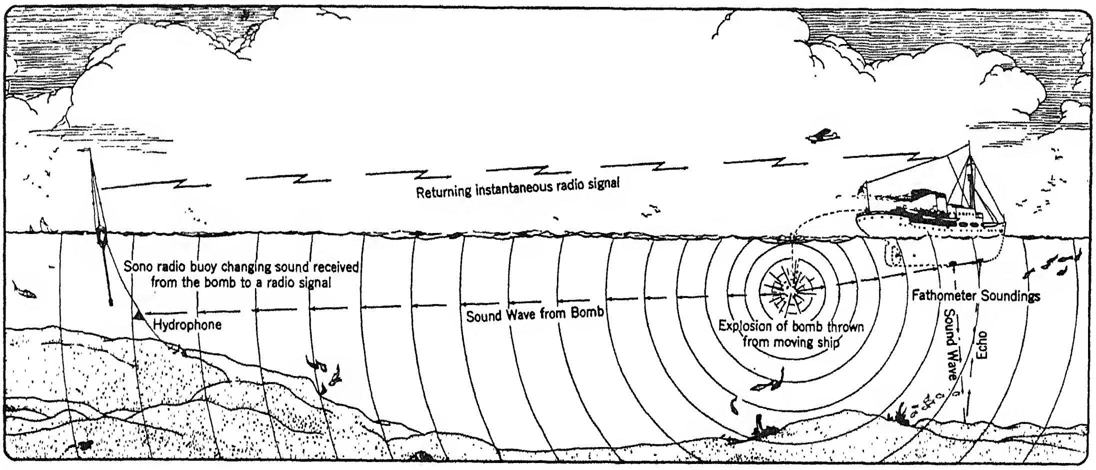


FIG. 11. MEASURING DEPTHS AND DISTANCES BY SOUND

TWO OF THE SCIENTIFIC METHODS USED BY THE COAST AND GEODETIC SURVEY TO DETERMINE WATER DEPTHS AND THE PRECISE POSITIONS OF SUCH DEPTHS ON NAUTICAL CHARTS—ECHO SOUNDING AND RADIO ACOUSTIC RANGING. BOTH OPERATIONS ARE CARRIED ON SIMULTANEOUSLY BY DAY OR NIGHT WHILE THE SHIP IS UNDERWAY.

the shoaler depths, giving an almost continuous profile of the bottom.

Our knowledge of the ocean floor has been greatly augmented by the advent of echo sounding. The measurement of great depths, which formerly required an hour or more, can now be obtained in a matter of seconds, thus making it possible to take thousands of soundings in areas where formerly only a few scattered ones were economically feasible.

Geographic position, no less than depth, is an essential element of every hydrographic survey. The early hydrographers located their soundings by sextant angles on shore objects. When out of sight of land, positions were determined by celestial observations or by "dead reckoning." Although suitable for navigation, these methods had certain inherent weaknesses when used for surveying purposes, and an accurately coordinated offshore hydrographic survey was the exception rather than the rule. These difficulties, long an obstacle to adequate charting, have been overcome by the use of under-water sound ranging and other methods (Fig. 11). By exploding a bomb in the water near the survey vessel and measuring the time of arrival of the sound wave at two or more previously located sono-radio buoys (Fig. 12), the distance of the vessel from the buoys can be determined, for the velocity of sound

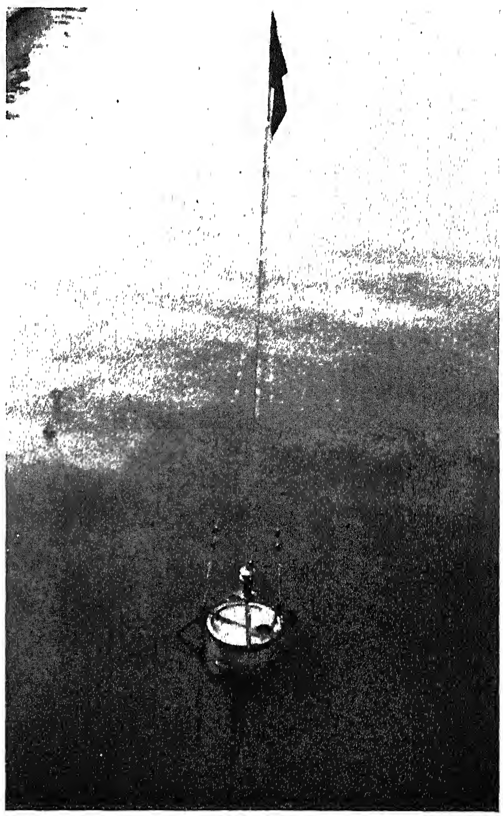


FIG. 12. SONO-RADIO BUOY

IN USE IN OFFSHORE HYDROGRAPHIC SURVEYING FOR POSITION LOCATION. THE BUOY CONTAINS THE MECHANISM FOR RECEIVING THE SOUND IMPULSE AND FOR AUTOMATICALLY RETURNING A RADIO SIGNAL TO THE SHIP.

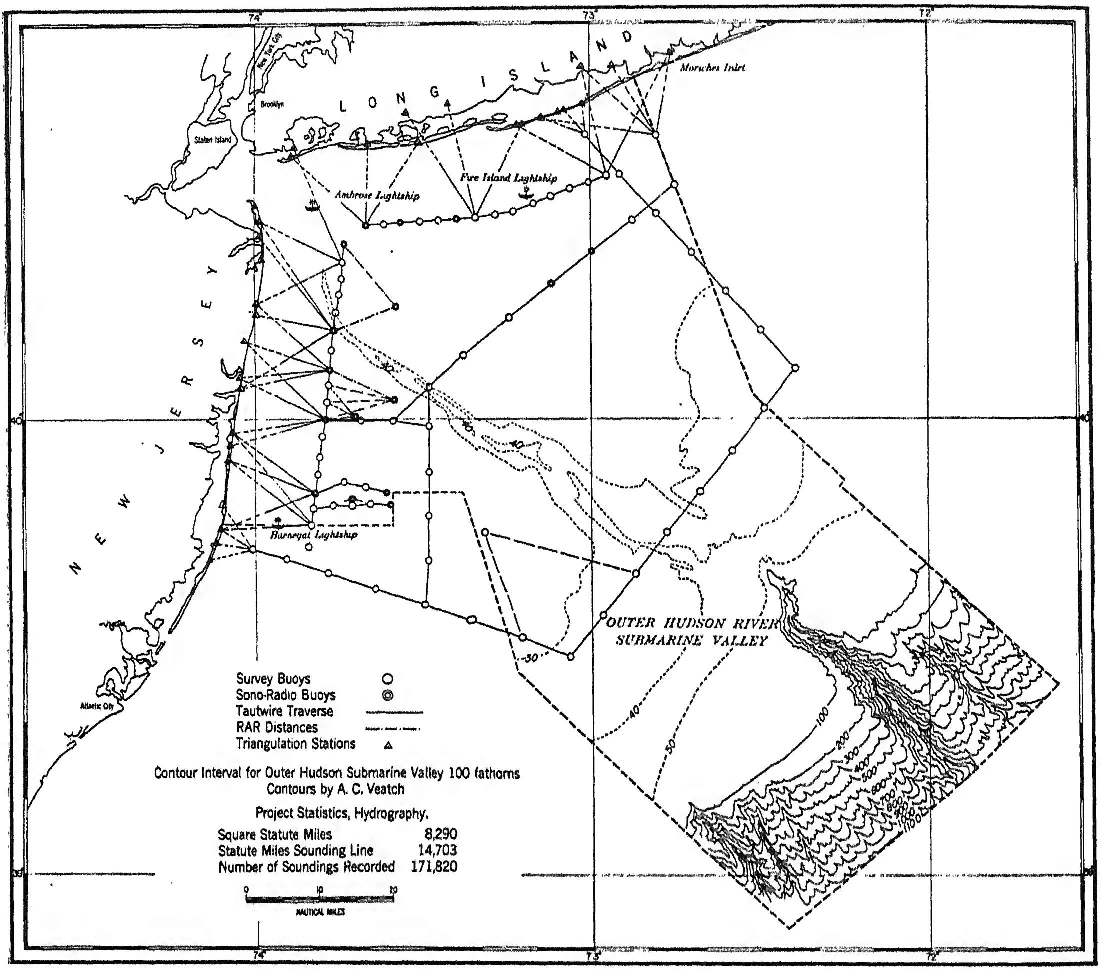


FIG. 13. COORDINATION OBTAINABLE IN MODERN OFFSHORE SURVEYS
METHODS USED TO EXTEND CONTROL FROM THE COASTLINE TO OUTER HUDSON SUBMARINE VALLEY, 100 MILES AWAY.

in sea water is known. This method of position determination is known as Radio Acoustic Ranging and was developed in the Coast Survey after World War I.

The acoustic methods of surveying are steadily pushing seaward the frontiers of accurate hydrographic surveys, and are being used today to explore the intricate patterns of our deep coastal slopes with an accuracy and completeness undreamed of by the older methods—thus adding to the safety of life and property at sea (Fig. 13). Acoustic surveying is rapid and sensitive enough to detect and chart wrecked ships lying on the ocean bottom (Fig. 14).

The Wire Drag. Some years ago a method of hydrographic surveying was developed to supplement the ordinary methods of sounding in areas where pinnacle rocks, boulders, or coral reefs were likely to exist. This consists in towing through the water a wire, sometimes a mile or more long, supported at an adjustable depth by floats and buoys. The wire will catch on any obstruction which extends above the depth at which it is set. Many dangers to navigation have been found by this method along the rocky coasts of California, Alaska, and New England, the most spectacular of these finds being located in southeast Alaska close to the steamer lane. From a depth of 650 feet it rises shaftlike to within 17 feet of the surface. The function of a hydrographic survey is to find such hidden dangers; the function of the chart is to show them accurately. Once the obstruc-

tions are charted, the navigator is put on notice of their existence and can shape his course to avoid them.

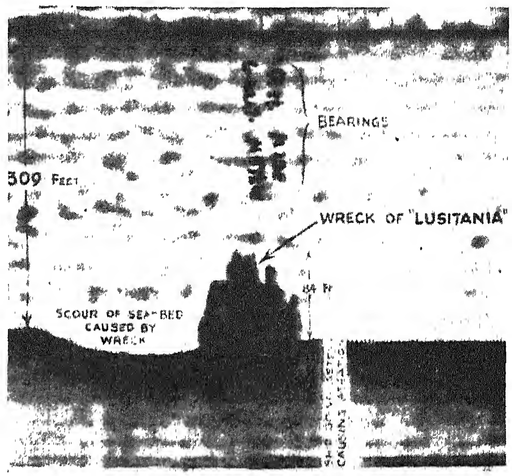
The wire-drag method is still in use today. During the war it has been instrumental in the location and charting of the many wrecks along our Atlantic Seaboard, resulting from enemy submarine warfare.

Scientific Chart Making. The improvements in navigational and surveying techniques of the past two decades have not been lost to the nautical chart maker. In the last few years a new type of chart has been issued which utilizes to the fullest extent the wealth of submarine detail contained in modern hydrographic surveys. By a judicious use of depth contours, characteristic features of the ocean bottom are brought into prominence, which is lacking on the conventional-type chart where soundings alone are used. Depth contours are to the nautical chart what land contours are to the topographic map. Imagine the difficulty in interpreting a topographic map if elevations alone were shown. From these charted features the navigator can not only identify his position, but he can also shape his course in advance so as to pass in close proximity to them.

This type of chart was not possible before the advent of modern hydrographic methods, because the contours could not be delineated with sufficient accuracy. Today it is standard practice for many of the Coast Survey charts.

Navigation by depth contours is not yet fully appreciated, and their use for charting the ocean bottom seems to have aroused some surprise among those accustomed to the conventional form of chart. It is curious to note, however, that contours were actually used for delineating the floor of the ocean even before their use on land. Two hundred years ago the Dutch engineer N. Cruquis used them to show the bottom of the Merwede River, and Philip Buache, a Frenchman, used them to outline the depths in the English Channel.

The office work of preparing a nautical chart for publication is carried through with the same painstaking care with which the field work is accomplished. No approximations and no economic short cuts are allowed to jeopardize its accuracy or to vitiate its usefulness. Experienced cartographic engineers verify every aspect of the field surveys before the data are applied to the charts. There is a distinction between a survey and a chart that should be kept in mind. A survey is an original record of field data—a chart is a compilation from such surveys. Compilation is a process of selection. Even the largest-scale chart would contain but a fraction of the information shown on a hydrographic survey. The chart compilation is an engineering product in which all the rules of engineering are meticulously observed. The special and frequently adverse conditions under which charts must be used on shipboard call for good judgment throughout their preparation. Names, notes, and symbols must be so placed that they can be easily and quickly read. Even the paper on which charts are printed is of importance in order that it may be suitable for plotting and subject to as little distortion as possible. The printing of charts is done in a temperature- and humidity-controlled pressroom and on paper with the proper moisture content to insure accurate registration of colors.



Courtesy Henry Hughes & Sons, Ltd.

FIG. 14. THE SUNKEN *LUSITANIA*

ECHO-SOUNDING DEVICES TRACE A PROFILE OF THE SEA BOTTOM OVER WHICH THE SURVEY SHIP IS PASSING.

The usefulness and accuracy of the chart depend not only upon the material entering into its construction but upon the critical appraisal of such material and upon the intelligence with which the essentials are por-

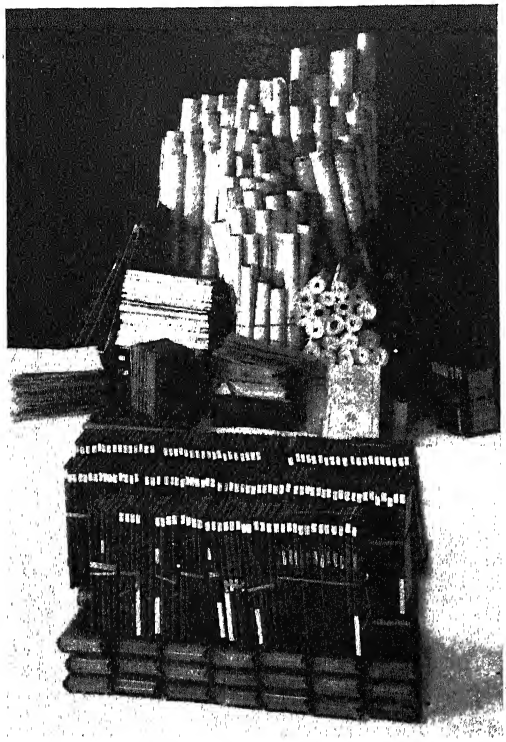


FIG. 15. RECORDS FOR ONE CHART
THIS ILLUSTRATION SHOWS THE 469 FIELD SURVEY SHEETS, SOUNDING AND TIDAL RECORDS, AND VOLUMES OF DATA THAT WERE REQUIRED IN THE MAKING OF A SINGLE NAUTICAL CHART BY THE U. S. C. & G. SURVEY.

trayed. "Easy reading is hard writing" can well apply to nautical charts. The skilled cartographer must sift from the mass of data (Fig. 15) before him the important from the unimportant, the strong from the weak, the stable from the changeable; rejecting some entirely, some in part, and coordinating and selecting from the rest the information to appear on his finished chart.

Besides these engineering elements the chart compiler must be ever conscious of the importance of artistry in the chart. There must be no crowding of matter to confuse the navigator and there must be no haphazard arrangement to throw the chart off balance. Such charts are ungainly to look at and difficult to use. The style of the lettering, the placing of the names, and the amount of hydrography and topography are factors which distinguish good work from amateurish efforts. It is sometimes erroneously thought that the more crowded the soundings are on a chart, the more thoroughly has the region been surveyed. This is true in a limited sense only, but usually it is the earmark of an unskilled cartographer.

Keeping Charts Current. The publication of a chart by no means completes the problem of the chart maker. Charts must be kept alive if they are to serve their purpose properly. They must be revised frequently to give an accurate and up-to-date picture of existing conditions. Both man and nature are constantly changing the face of the earth —breakwaters and jetties are built, channels and harbors are dredged, and new paths of commerce are opened.

The coastal region is the zone where two great physical provinces meet—the land and the sea—and where changes often take place rapidly. Wave-lifted and current-borne material has driven Rockaway Point, Long Island, a distance of over 4 miles in 100 years until a jetty built in 1934 arrested its westward growth. Rivers empty vast quantities of sediment near their mouths to build out the coastline, one of the best examples being the Mississippi. In times of storm barrier beaches are often broken through to form inlets of a temporary or permanent nature. A deep-water inlet 300 feet wide and 18 feet deep was cut through the barrier beach off the south coast of Long Island during the hurricane of September 1938.

While these visible effects on land are being produced, others even more important to the navigator are going on beneath the surface of the water. Bars and channels shift, new shoals form, old ones disappear. The safety of navigation depends upon an accurate representation of all these changes on the published charts.

Because of these changes it is necessary to print some of the charts in small issues to avoid early obsolescence. The New York Harbor Chart, for example, is printed about four times a year. It is the endeavor to furnish the navigator with the very latest information available. Sometimes the presses are even stopped to apply an important correction to the printing plate. Between printings, important corrections are applied to the chart by hand before issue.

From here on the navigator is on his own

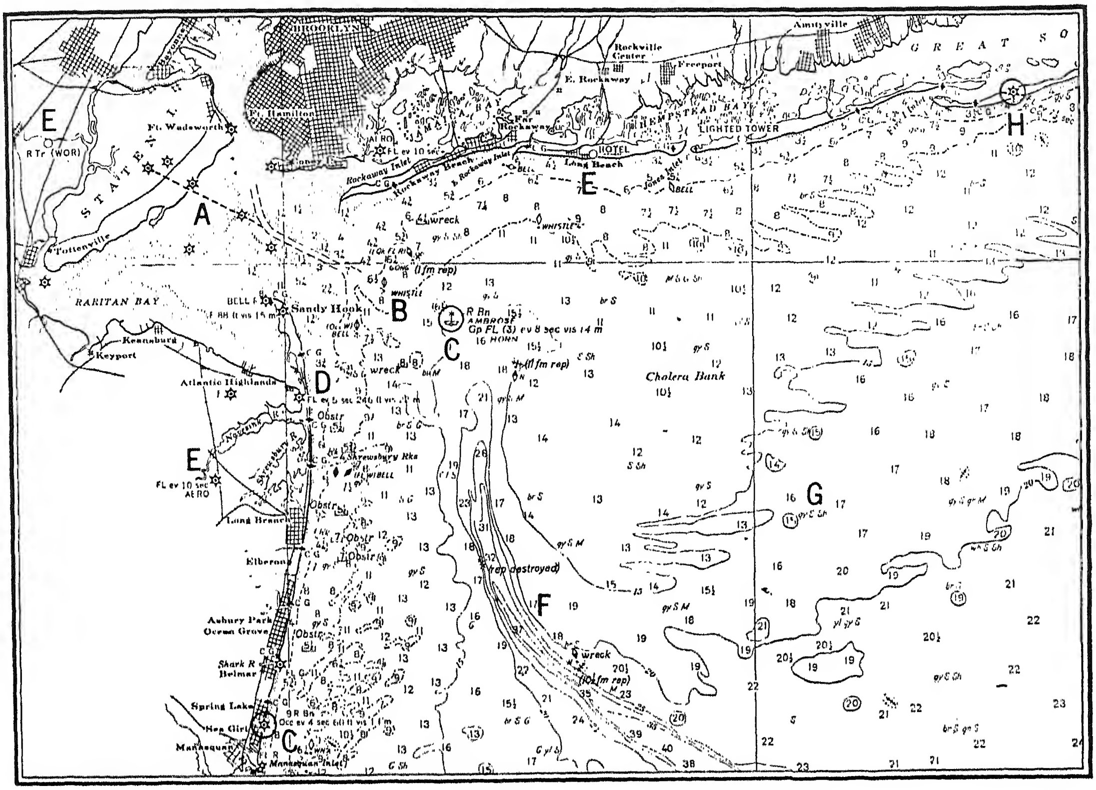


FIG. 16. MODERN U. S. C. & G. SURVEY CHART
SECTION OF CHART 1108 (REDUCED) SHOWING APPROACHES TO NEW YORK HARBOR. SEE TEXT FOR EXPLANATION.

and must bring his chart up to date by scanning the weekly *Notices to Mariners* and other special publications issued for his benefit. The locations of outstanding dangers, newly discovered, are furnished him by radio. Every effort is thus being made by the Government to keep the navigator fully informed of vital changes in the chart.

Aids for Safe Navigation. Figure 16 shows a section of the modern chart of the approaches to New York Harbor. It is of interest to note the various navigational aids the mariner has for determining his position as he leaves or approaches the metropolis:

At *A*, he has a range for a line of position through Ambrose Channel.

At *B*, he has danger warnings and channel markers—buoys by day and lights of various characteristics by night.

At *C*, he has radio beacons on the lightship and on shore for obtaining bearings by radio compass from the vessel.

At *D*, he has the height and visibility of a light for determining position.

At points *E*, he has landmarks for taking angles and bearings—structures and natural objects by day and lights by night.

At *F*, he has depth contours for use with an echo sounder.

At *G*, he has bottom characteristics, an ancient method, but still in use, for determining approximate position.

And at *H*, he has a Radio Direction-Finder Station, which furnishes him with true bearings for plotting on his chart.

One rightly wonders how the ancient mariner, without any of the modern navigational aids and contrivances, ever managed to reach his port of destination. Perhaps the answer is that very often he didn't!

The accurate chart of today is a scientific achievement. Its evolution has kept pace with the economic development of the nations and with the progress in science and engineering. For the navigator it is robbing the sea of its hidden perils; for the scientist it is giving new orientation to his physiographic concepts. And although we have moved far from its early crude form, it is

not to be supposed that the nautical chart has reached its limit of usefulness or that its character has been fixed.

While it would be difficult to predict what the full impact of the war will be on the nautical chart of the future, it is reasonable to expect that some of the techniques developed in this and related fields will affect both the character and the production of nautical charts. The period following World War I saw tremendous forward strides in the science of hydrographic surveying, with resultant effect on the nautical chart. It may well be that the adaptation of radar to marine navigation—which would make it possible to see through fog and darkness and observe the positions of ships, buoys, and shorelines—may require further modification in the present character of the chart. It is the function of a modern chart-making agency to serve the maritime public in the best and most expeditious way possible. To this end the Coast and Geodetic Survey will be ever on the alert to adapt its product to the changing conditions in the fields of marine surveying and navigation.

A. L. SHALOWITZ

A. L. Shalowitz, Research Cartographic Engineer in the U. S. Coast and Geodetic Survey, was born in Lithuania in 1892. He was graduated from the Baltimore Polytechnic Institute in 1911. In 1916 he entered the field service of the Coast Survey as a hydrographic and geodetic engineer and made surveys in the States, Alaska, and the Virgin Islands. Seeking a less nomadic life, he transferred to the Washington Office in 1921. He received his LL.B. in 1926 (with first honors) from Georgetown University and his LL.M. in 1930 from George Washington University. He has specialized in the interpretation of the surveys and charts of the Bureau, for legal and other purposes, and is the author of a number of technical papers and reports, among them *The Geographic Datums of the U. S. Coast and Geodetic Survey*, *Our Changing Coastline*, and *Navigability—A New Supreme Court Interpretation*. He has collaborated recently in the preparation of the *Hydrographic Manual* of the Coast Survey. Mr. Shalowitz is the editor of *Surveying and Mapping*, journal of the American Congress on Surveying and Mapping. His editorial and cartographic experience was evident in his meticulous preparation of the manuscript and illustrations of his present article. Nothing was overlooked that would help the editor do his best for the author and reader.

MAN'S UNINVITED FELLOW TRAVELER—THE COCKROACH*

By JAMES A. G. REHN

CURATOR OF INSECTS, THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA

THE cockroaches are insects which to the average person are house-haunting pests, living secretive lives away from the light of day, and creeping into one's larder when given the slightest opportunity. Most definitely they produce in the majority of people a strong feeling of aversion. It often takes some effort to convince the "doubting Thomases" that the number of species of cockroaches which are domiciliary pests is greatly limited—in fact less than one percent of the known forms—and that cockroaches of many kinds are diurnal, with hundreds of species tropical forest foliage forms, others semiaquatic, some in one sex living in the ground, a few wood-boring, while a dozen or so genera will be found, in a state of either known or suspected commensalism, in the nests of ants, wasps or termites.

From all these far more interesting biological associations, however, we are almost always brought back to those domiciliary cockroaches which to most people give the group a reason for its existence, and it is on these that the oft-repeated questions are centered. An inevitable one is, "Where did this species come from originally?" Rather helplessly most entomologists then pick up a few standard and rather well-thumbed textbooks, and read this or that wording of stereotyped traditional explanations that this species "came from the Orient," and the other "is a native of tropical America." Unfortunately hardly any standard work of reference has given correctly the most probable original homes of some half-dozen of our better-known domiciliary, or habitation litter inhabiting, species, and almost every new treatment of these species simply repeats the erroneous assumptions of the past. The chief justification for this course will be found in the unfortunate technical specific names applied to them long ago by systematic pioneers, appellations such as *orientalis, germanica, americana, australasiae, surinamensis* and *maderae*. According to their light of many decades past these early scholars applied specific names suggested by the territory from which they received their specimens, or that from which it was believed they had come. The difficulty was that in the former case the species often had gone there as a fellow traveler with the early voyagers, and thus became one of the "first settlers."

During the past thirty-five years, individually or with my colleague Mr. Morgan Hebard, I have made a number of critical studies on the systematics and distribution of the cockroaches of both the New and Old Worlds. Mr. Hebard personally has added to these many equally important contributions, and a large amount of as yet unpublished critical information has also been assembled from the unsurpassed collections of the family which have been assembled at the Academy of Natural Sciences of Philadelphia. In addition the series of these insects in virtually all other important American institutions have been drawn upon, and we also have been able to study extensive representations from the collections of great European institutions such as the British Museum (Natural History), the French National Museum of Natural History, the Museum of the Belgian Congo, and a number of other museums scattered over the world.

In the cockroaches, or Blattidae, as in most other groups of the Orthoptera, we find a very marked degree of geographic limitation of genera, very few except those spread by commerce being world-wide in distribution. However, in analyzing problems of blattid distribution or centers of origin, it

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is necessary to realize that we are dealing with a group which in considerable part possesses a broad range of coverage in adaptability, and also in ease of transport. We are also concerned with a very ancient group, with long-tested and often quite flexible survival powers, as the blattids would not have covered their great span of geological time if this were not so. The group has, and doubtless has had, many forms so highly specialized that they are virtually incapable of utilizing various means of transport, or of surviving in reasonably different environments if they should be so placed. On the other hand, we have a more limited number of adaptable types, which readily can be transported, and, given their required temperature and humidity tolerance, are thoroughly capable of establishing themselves fully and enduringly on the other side of the world, or at any suitable intermediate station.

Until the last forty years, our knowledge of the taxonomic relationship of many of the blattids was very unsatisfactory, particularly the concepts of generic units. While the classification of the superfamily Blattoidea remains in a somewhat unsettled condition, without general agreement as to the limits of the higher groups, in the main it is definitely on a sounder basis than it was in 1900, and our understanding of the character, limits, and relationship of many of the genera now rests upon a far greater knowledge than was possessed at that time. In consequence, it is possible by drawing upon information now available on the distribution of near relatives of species which have acquired a domiciliary status to secure important evidence as to the original homes of the latter.

We must bear in mind that domiciliary habits are acquired ones, like domesticity in the dog, cat, horse, ox, sheep, or goat. Probably some cockroaches, in a feral state, fed upon and lived in material which early men pre-empted as a food, and their passage into his habitations, in that way, was at first a physical transfer. With an assured abundance of food, particularly of varied character such as would appeal to insects which are largely omnivorous, it is not difficult to appreciate the beginning of the domiciliary habit.

Dependence of cockroaches upon human habitations, however, varies in degree, and some species, such as *Pycnoscelus surinamensis,* though occurring commonly in the litter and topsoil about buildings, are not as a rule found within buildings except greenhouses and similar structures. Again, many species are accidentally transported from tropical regions in fruit and similar products to much colder regions, and are there unable to survive, or at least propagate, in houses or similar structures, even when the proper food is provided, unless the required temperature is maintained. One species will require only a temperature above a specific minimum; for another, a certain range of relative humidity is as important as temperature.

Taxonomic and distributional studies of recent cockroaches, or the family Blattidae, with which I have been engaged for a number of years, have brought together a very considerable amount of information on the thousands of existing species of this ancient group. One of these is the cockroach probably best known to American entomologists and perhaps laymen as well: *Blatta orientalis,* the so-called Oriental Cockroach, Shad Roach, or Black Beetle (Fig. 1). With us it is one of our commonest household pests, at home in almost any situation which promises food and warmth. In the literature of the past we find the very general assumption that *Blatta orientalis,* the "Oriental Cockroach," came from what is rather vaguely defined as "the East." Linnaeus, the great father of systematics, in 1758 considered the species as native to America and as introduced in the East. He noted it as common in Russia and as having reached Stockholm in 1739. The exact reason for his indication of America as its original home is not at all clear, but certainly he was wrong. No members of the genus to which *orientalis* belongs or of any very closely related genera are American endemics. Carl Brunner, the great Austrian orthopterist, in 1865 felt that *orientalis* originated in Asia, and he then stated it "abounds in the East Indies as

well as in Asia Minor," but he added "it is rare on the coasts of the Mediterranean and seems to be completely absent from Greece." He then continued, "It is equally rare in Italy and in southern Spain," but noted that he had it from Algeria. In 1882 the same author, in his *Prodrome of European Orthoptera,* said merely that the species is not known in a wild state, and that in the last two hundred years it entered Europe from Asia. Miall and Denny in *The Cockroach* were more specific as to their understanding of the species, and said it "is native to tropical Asia and long ago made its way by the old trade routes to the Mediterranean countries," but why they so definitely fix its original home is not stated.

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FIG. 1. ORIENTAL COCKROACH[1]

[1] Many entomologists and most pest control operators prefer the abbreviation "roach" for this and other specimens of domiciliary cockroaches. Oriental and German roaches have been seen by most citizens of the United States, who think that one is a roach and the other a water bug. It should be understood that "roach" and "water bug" are synonymous and that it would be desirable to discard the latter name.

In this and the other ten illustrations the insects are shown in their natural, or actual, size; thus, the sizes of the adults of the different species can be compared. Appropriately, the American roach is altogether superior to the Oriental and German roaches in size, appearance, and ability to use its wings in flight.

In most of the illustrations, two specimens are shown: male and female adults. In some figures the wings of a specimen are spread. In Figure 6 an immature form is also shown. For identification one of the following symbols is printed under each specimen: ♂ = adult male; ♀ = adult female, and X = immature form.—ED.

The passage of *Blatta orientalis* westward across Europe, like that of *Blattella germanica,* is well documented and need not be dwelt upon here. It is sufficient to know that into northern and north-central Europe this species quite definitely came from the East, and according to Lucas, in his *Monograph of British Orthoptera,* it had made its way to Holland and England by the time of Elizabeth. Early in the seventeenth century Swammerdam knew it in Holland, and in 1624 Moufet mentions it as occurring in wine cellars in England. There, however, its spread was much slower than on continental Europe, as Gilbert White, in 1790, speaks of it then as an unusual insect at Selbourne. As to the localization of its distribution in Mediterranean Europe to which Brunner referred in 1865, paucity of information at that time was probably responsible. Ignacio Bolívar, the distinguished Spanish orthopterist, in his 1898 *Synopsis of the Iberian Orthoptera* speaks of the species as "acclimated in the great part of Europe," without qualifying comment.

The most interesting and comprehensive summary to appear in recent years of the information then available on the native home of *Blatta orientalis* is contained in the posthumous work of Robert Shelford entitled *A Naturalist in Borneo.* For some years before his death in 1912 Shelford was our most promising student of the cockroaches, and in this interesting volume will be found many observations and conclusions assembled as a result of his residence at Kuching, where he served as Curator of the Sarawak Museum. He wrote:

> [*Blatta orientalis*] has not been met with in a truly wild state until quite recently; the first specimens that were found were caught in houses, and though it has always been assumed that it was imported into Europe from the East, I am not aware that it has ever been found in Asia except as an unwelcome guest in human habitations. The discovery (by von Adelung) of specimens in the Crimean peninsula living under dead leaves, vegetable detritus and stones, in woods and copses far from any human habitation, is a fact of considerable interest, and it is perhaps permissible now to regard Southern Russia as the centre whence this ubiquitous insect has spread.

My personal interpretation of von Adelung's find is that the original home of the species was not located, but instead a "way station" on an ancient line of travel, where the species doubtless has been established for centuries. My reason for so believing is that no wild species related to *Blatta orientalis* is known from Europe.

Taxonomic studies of the Blattidae of Africa, with which I have been engaged for a number of years, have brought to light several previously undescribed, wild, close relatives of *Blatta orientalis;* one from Uganda, another from Kenya Colony. Like these species, all the other wild forms properly referable to *Blatta* as now restricted—which means all except *B. orientalis*—are African, ranging southward to the Cape of Good Hope and west to the Cameroons. Examinations of large series of as yet unreported Oriental Blattidae disclose no specimens of species of *Blatta,* and the literature, as mentioned by Shelford, gives no concrete information on the occurrence of *orientalis* there, except in a few large ports serving a world commerce. All indications are that *orientalis* does not thrive in the true lowland tropics, and the only parts of South America where the species seems to have been well established for many years are Chile and Argentina, neither of which is truly tropical. Philippi recorded *orientalis* (under a synonymic name) from the former country as early as 1863. Other early American occurrence records were from Jamaica in 1842 by Sell, from Guadeloupe in 1837 by Lherminier, and from Honduras in 1868 by Walker. There has been nowhere in tropical America as complete occupation of a country by *orientalis* as in the U. S.

Turning back to Africa, the records of *orientalis* show that, except for its presence at Windhoek in Southwest Africa, at Cape Town, and in Natal—these clearly detached colonies established by commerce—it came from Morocco, Algeria, Tunis, Tripoli, Cyrenaica, Egypt, and Somaliland. Finot has reported the species in Tunis as occurring in desert encampments. The North African material which I have examined is from localities reaching from Mogador, Morocco, to the Sinai Peninsula.

From the positive and negative evidence now available, I feel justified in concluding that *Blatta orientalis* was originally a native of North Africa, and that it probably found its way into eastern Europe in Greek, or even Phoenician, vessels, spreading into Byzantium, Asia Minor, and the Black Sea region, and thence slowly northward and westward over the remainder of Europe. The colonies in Chile and Argentina were doubtless established by way of Spain; where it probably was introduced from Moorish lands in North Africa long before the species, in its westward spread over most of Europe, had reached adjacent France. Over most of the continent of North America *orientalis* is as much at home as in Europe, but in the more humid southern United States it yields its usual role quite generally to the species of *Periplaneta.* Similarly in the humid tropical areas of South America *orientalis* has made little headway, and there it is not the domiciliary problem which the Periplanetas are. In brief, *orientalis* seems to have been derived from an area which combines summer heat and moderate winter cold, as the species can stand more of the latter than the Periplanetas, yet is not adapted to conditions of tropical or subtropical humidity. I have little doubt that comprehensive work in North Africa will disclose *Blatta orientalis* living under the same conditions of freedom from dependence upon human habitations as noted in the Crimea. Certainly the nearest known relatives of *orientalis* are wild forms of east-central Africa.

PROBABLY the most ubiquitous species of cockroach, and one certainly as well known as the Oriental Cockroach, is the so-called German Cockroach, or Croton Bug, (*Blattella germanica*) (Fig. 2). There are many other names for it; the English call it Shiner or Steam Fly; in Russia it has been called the "Prussian," and in Prussia it was known as the "Russian." Its steady spread across Europe was very similar to, but definitely more recent than, that of the Oriental Cockroach. In England it seems to have become broadly established only by the middle of the last century, and, according to

an anonymous writer quoted by Miall and Denny, was supposed to have become established at Leeds by means of bread baskets of soldiers returning from the Crimean War. Burr, writing in 1936, says it had been established in England for a century. Brunner in 1882 quoted Fischer de Waldheim to the effect that the species occurred feral at Moscow, and that it was similarly present in Thuringia, Saxony, the Hartz Mountains, in Westphalia, and at Kloster Neuburg near Vienna. Brunner then added, however, that he had never found it in a wild state. In 1898 Ignacio Bolívar stated that it was encountered in all of the Iberian peninsula and the remainder of the Mediterranean littoral.

As with *Blatta orientalis,* most authors give the original home of *germanica* as "Asia," and consider that it reached western Europe across Russia and Germany.

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FIG. 2. GERMAN COCKROACH

The very imperfect appreciation, until recent years, of generic limits in the complex group or genera to which this species belongs makes any deductions on the basis of generic distributions as of the literature prior to 1910 virtually valueless, as *germanica,* before that time, was considered a member of an enormous, entirely unnatural "blanket" genus, now more logically broken into a considerable number of components and even into several genera groups. Thus the evidence of closely related species as indicators of the probable original home of *germanica* is our sole reasoning point. Purely historical information, of course, is not available, except as to its spread in the last century or so.

We have in the Oriental region a limited number of species now generically associated with *germanica,* but all of these show very definite morphological differences, and none is from what is generally assumed to be the "Asia" of ordinary language; that is, Central Asia. Instead they are Indo-Iranian and chiefly Indian in distribution. On the other hand, in northeastern Africa, from between the great African lakes and Eritrea and the Anglo-Egyptian Sudan, occur fifteen distinct species intimately related to *B. germanica,* which also occurs there as well as westward across northern Africa, much as does *Blatta orientalis,* although *germanica* is also of broader establishment at many localities in the most tropical parts of the continent.

Therefore it would seem that the early human associated history of *Blattella germanica* is essentially the same as that of *Blatta orientalis:* From northeastern Africa it was transported by Greek or Phoenician vessels to Byzantium, Asia Minor, and the region of the Black Sea. In much of southern Russia it remained for centuries until the gradual opening up of occidental commerce with that country, probably after the Thirty Years War, made the passage of the species westward a possibility. It then spread gradually over western Europe and thence to America, and by commerce to virtually all parts of the world. The tolerance by this species of many conditions apparently not acceptable to *Blatta orientalis* has made its distribution much more cosmopolitan than that of the larger species.

THE cockroach genus *Periplaneta* is made up of a number of fully winged and active species, of which at least three have become domiciliary in habits, and two of these are outstanding pests in tropical, subtropical, and even warm temperate areas of virtually the entire world. These are the so-called American Cockroach (*Periplaneta americana*) (Fig. 3) and the equally poorly named Australian Cockroach (*Periplaneta australasiae*) (Fig. 4). In the United States the first of these is found quite generally as a domiciliary insect over most of the warmer, more southern area, frequently taken as far north as New England, but to the northward only under definitely protected conditions.

The Australian Cockroach is more partial to consistently warmer conditions and can exist continuously over much of the United States only under conditions of maintained warmth.

Most of our older, and some modern, authorities assumed that Linnaeus and Fabricius respectively were correct in the implications of the names they gave to these

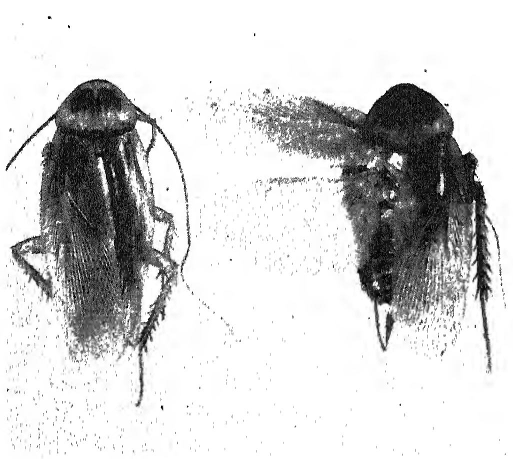

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FIG. 3. AMERICAN COCKROACH

species; i.e., that the first originated in America and the second in Australia. It is yet asserted regularly, and with rather monotonous unanimity, that *americana* is native to tropical America and that *australasiae* came from the Antipodes. However, the far-seeing Shelford questioned this in his *Naturalist in Borneo,* saying, "it is certain that *australasiae* is only a rare immigrant to Australia, and I believe that tropical Africa or perhaps South-Eastern Asia was its original home."

No nondomiciliary species of *Periplaneta* occurs in the New World, except for the localized occurrence in our southeastern states of one species clearly introduced, which can be found in a variety of situations, as in houses, about buildings, under signs or on wharves, but always within the limits of cities or towns. The exact origin of this species is still uncertain, and it is as yet unknown from the nearby West Indies. Native nondomiciliary species of *Periplaneta* occur in many parts of tropical and southern Africa and in the Indo-Malayan region. The nearest relative of *Periplaneta* is *Pseudoderopeltis,* which is a dominant and peculiarly African genus with a score or more species, occurring from Senegal and Egypt to the Cape. Throughout tropical Africa both *Periplaneta americana* and *australasiae* occur almost everywhere under domiciliary conditions, and in the vicinity of, as well as in, buildings, huts, and shelters of all kinds. Both are now very abundant in tropical America under domiciliary conditions, but there they are not as frequently encountered outside of human structures as in tropical Africa, as I can testify from personal experience across the width of Central Africa, in the West Indies, as well as in a number of countries of Central America and several of South America. Apparently, the occasional European records of these two species have been due to individual commercial introductions and not to sheet infiltration as in certain other species.

From our present knowledge, I feel we are warranted in concluding that, though *Periplaneta* also occurs native in Indo-

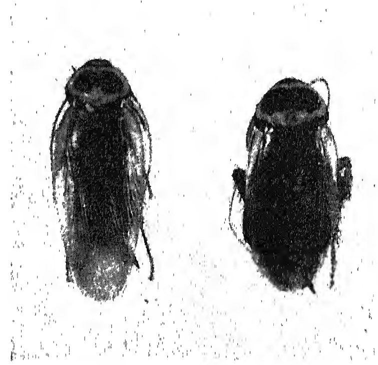

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FIG. 4. AUSTRALIAN COCKROACH

Malaysia as well as in Africa, the evidence points more directly to tropical Africa as the original home of *Periplaneta americana* and *australasiae,* and perhaps the less frequent *P. brunnea* as well. Slave ships from the West African coast, continuously moving for nearly two centuries, doubtless provided the means of introduction into South America, the West Indies, and the southern United States. The flying ability of *Peri-*

planeta, which is often exercised and by both sexes, has furthered the broadening of distribution when colonies had been established. The sole controlling factor with these species seems to be the maintenance of temperature above a certain minimum. Along the periphery of their distribution they can survive only under protection in greenhouses and similar uniformly heated places.

Perhaps the most pleasingly patterned of our domiciliary cockroaches is *Supella supellectilium* (Fig. 5), for which the vernacular name Brown-banded Cockroach is the most appropriate of several which have been used. First described by Serville from Mauritius, it was, as its specific name indicates, recognized by him as a household form. The species is now known from a considerable part of the tropics and subtropics of the Old World, although apparently much less evenly or broadly distributed to the eastward than it is over eastern, southern, and northeastern Africa. It was introduced into the West Indies probably by slave ships and first recorded there in 1862 by Saussure, the great Swiss orthopterist, as the synonymous *Blatta cubensis*. A few records are available of its occurrence at coastal points in South America, but it is not at all broadly established there or in Central America. From the United States I first reported it in 1903 as taken at Miami, Florida; doubtless introduced from Cuba, where it is quite abundant in houses. In 1912, with my colleague Mr. Morgan Hebard, I found it common in a fruit store in Key West, and in recent years its distribution in the United States has been steadily extended, so that today it is known to occur under domiciliary conditions as far northeast as Philadelphia, westward to San Bernardino, California, and in the interior northward to Nebraska. Shipments of fruit from Florida have probably provided a ready means for distribution in our territory.

The genus *Supella* is now under critical taxonomic study, and there are yet to be properly characterized a number of native African species living under natural conditions and not dependent upon human habitations. The species *supellectilium* is distributed over much of Africa outside of the Guinea forest areas. No nondomiciliary species of *Supella* is known except from Africa, and it is therefore quite reasonable to conclude that the genus, hence the species *supellectilium*, is of African origin, and that slave ships probably were responsible for its American introduction.


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FIG. 5. BROWN-BANDED COCKROACH

The most widely distributed member of the blattid subfamily Panchlorinae is a species which possesses no accepted vernacular name, but which may be called the Bicolored Cockroach (*Pycnoscelus surinamensis*) (Fig. 6). Supposed by Linnaeus to be of American origin, he gave to it the specific name *surinamensis*. While the subfamily Panchlorinae has many endemic American species and a considerable number of genera so limited, *Pycnoscelus* is clearly not one of these. The species *surinamensis* is virtually world-wide in distribution within the humid tropics and subtropics and is less likely to occur within doors than under stones, boards, tiles, dead palm trees, or any other loose litter or trash about houses or stables. It has also been taken from under the bases of living palm leaves, in bromeliads, under boulders away from houses, in rotted logs, in cracks of semidried mud and in the litter of wood-rats' nests. The very different-looking immature stages are often found burrowing in topsoil. Within the United States

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FIG. 6. BICOLORED COCKROACH

surinamensis has been reported as established outdoors in peninsular Florida, in Louisiana, and in southern and south-central Texas. In greenhouses and similar places with artificial heat during cold weather it may occur as far north as New England.

This widely spread *Pycnoscelus* has one peculiarity which may have a confirmatory value in establishing its original home. Virtually everywhere in the Western Hemisphere, and probably in other areas in which it occurs, the species is apparently parthenogenetic. As yet I do not know of an adult male specimen taken under condition of nature in the New World, the single case brought directly to my attention being a male captured in the greenhouses of the New York Botanical Garden, the source medium for which may have been a recent importation of Oriental shrubs. As far back as 1865 Carl Brunner noted in the series of *surinamensis* before him that the only males were from the East Indies, none being included in his tropical American representation. In 1893 the same author said he had seen additional males from Burma, but still none from the New World. Hebard in 1916, examining much larger series than Brunner had seen and from a considerable number of localities, noted only one male from Lombok in the Lesser Sunda Islands. In 1927 Rehn and Hebard found no males among fifty adult specimens from twenty-four West Indian localities. Since that time series totaling many hundreds of specimens from a very large number of localities have been examined by me. Males were found only in Oriental representations.

In Indo-Malaysia occur other endemic species of the genus *Pycnoscelus*, none of which ranges over more than that general area. This, taken with the inference which can be drawn from the localization of males of *P. surinamensis*, leads one to the conclusion that this species is of Oriental origin, and that it owes its present wide distribution to commercial transport, augmented by its habit of hiding in soil, thus making possible its transfer with plant stock earth. Its introduction into Africa may have been due to Arab traders, who for a considerable time before the Portuguese reached the East coast of Africa had carried on an extensive commerce across the Indian Ocean to the east. Again, its introduction there may have been due to the Portuguese voyageurs themselves. In western Mexico it may have been introduced in Spanish galleons from the west, as discussed under *Neostylopyga rhombifolia.* For most of tropical America on the Atlantic side slave ships probably furnished the means of introduction, as they did with so many other species of insects brought from Africa. With accumulated litter and trash slave ship holds carried to America many undesirable immigrants. Possibly the introduction of *surinamensis* into the United States was a secondary one from the West Indies, where the species is known to be firmly established in all of the Greater Antilles and a number of the Lesser group.

In 1926 Fielding showed that in Australia *Pycnoscelus surinamensis* is the intermediate host and agent for the transmission of chicken eye worm (*Oxyspirura parvovum*). The parasite was found to be present in both the abdominal and thoracic cavities, as well as in the legs of the cockroach, and passed to the fowl almost immediately after *Pycnoscelus* reached the bird's crop.

A LARGER and quite striking member of the subfamily Panchlorinae is the so-called Madeira Cockroach (*Leucophaea maderae*) (Fig. 7), which is broadly established in the West Indies and in coastal Brazil, with more

recent and localized colonizations in Central America, but in the United States has as yet been taken only as an adventive brought in on bananas or similar shipments. In all probability it eventually will become established in our Southern States, as it is universally prevalent in Cuba, Jamaica, Hispaniola, Puerto Rico, and the Bahamas, where

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FIG. 7. MADEIRA COCKROACH

it frequents habitations, warehouses, and other structures. At times it is a very abundant and serious pest.

Palisot de Beauvois first reported this insect from America in the early years of the nineteenth century, presumably from Hispaniola. He then stated his belief that it originated in Africa, and that it was imported into the French colonies in America. In all the Greater Antilles and a number of the Lesser ones, as well as in the Bahamas and the Virgin group, the species is now thoroughly established. Outside of tropical America and tropical Africa *maderae* is also known from Madeira, the Canaries, Morocco, Andalusia in Spain, and Corsica, doubtless as infiltrations in colonial commerce with West Africa. In Asia and the Pacific Islands it is known only from Java, the Philippines, and the Hawaiian group. Its presence in Java and the Philippines can be explained by accidental colonial introduction from Africa, either directly or secondarily from the Canaries or the western Mediterranean region, and in Hawaii by more recent transplanting, probably from the Philippines. The absence of the species from India, Australia, southern China and the greater part of Malaysia attests its nonendemism there.

The other five members of the genus *Leucophaea* are entirely tropical African in distribution, and *maderae* also occurs over most of that continent south of the Sahara from Senegal to Kenya Colony and to Angola and Natal. A very closely related species is restricted to West Africa between Liberia and the Gabon. It appears very probable to me that *maderae* was originally a native of West Africa, and probably that portion usually spoken of as Upper Guinea, where it commonly occurs today and where its nearest ally (*L. puerilis*) is also found. Slave ships doubtless brought the species to the West Indies and the coast of Brazil prior to 1800, thus establishing it in the New World.

PROBABLY the most bizarrely marked domiciliary species is the Harlequin Cockroach (*Neostylopyga rhombifolia*) (Fig. 8). Both sexes of this strikingly patterned species are flightless, the tegmina, or forewings, being but short, lateral, articulate, but functionally useless, slips, while the hind wings are absent. Male individuals of *rhombifolia* ordinarily occur much less frequently than females, but we have no evidence that males are unnecessary for reproduction as in *Pycnoscelus surinamensis*.

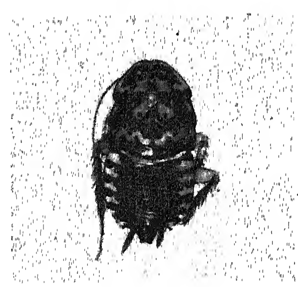

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FIG. 8. HARLEQUIN COCKROACH

The first record of *rhombifolia* from the New World was of its occurrence at Acapulco, Mexico, in Venezuela, and in Argentina (Brunner 1865). In 1893 Saussure and

Zehntner reported it from Brazil. There has been little amplification of the other New World records in recent years, but the west Mexican colony has been productive of a spread of the species along the west coast of that country, northward over Sinaloa, and even to Nogales on the Sonora-Arizona line, as well as its establishment for nearly fifty years in the southern part of Lower California.

The species *rhombifolia* is abundant over the greater part of the Indo-Malayan region, particularly in the Philippines. It is also found in the Hawaiian Islands, probably as an introduction in recent years from the Philippines, and it is also quite general in Madagascar, Mauritius, Rodriguez, in the Seychelles and adjacent islands, and along the eastern coast of Africa, there extending inland along trade routes to Nyasaland and the Zambesi valley, while it has also been reported from Madeira. The last is probably an isolated colony established by a chance introduction on a Portuguese ship Europe-bound from the Indian Ocean.

What particularly interests us is the introduction of the species on the west coast of Mexico a matter of more than eighty years ago. Very probably if the species becomes established as a domiciliary insect in the United States it will be from this colony. Acapulco was the port at which the classic Spanish galleons from the Philippines landed their cargoes for land transfer to the Atlantic side, to be reloaded for Spain. Rather curiously, we have an exact parallel to the problem of *Neostylopyga rhombifolia* in western Mexico in the cases of the legless lizard, *Typhlops braminus,* and of two other reptiles, *Peropus mutilatus* and *Hemidactylus frenatus,* which, as Taylor has recently shown, were certainly introduced from the Philippines into western Mexico, and in all probability by way of the galleons reaching Acapulco. There can be little question that this now broadly spreading colony of *Neostylopyga* was an additional galleon immigrant.

The Indo-Malayan region was clearly the original home of *Neostylopyga rhombifolia,* and the occurrence of the species even on the east coast of Africa is certainly due to the inadvertent agency of man.

A PRETTY domiciliary species of much of the tropics is the Cinereous Cockroach (*Nauphoeta cinerea*) (Fig. 9), which, although not as yet found in the United States, is known from Cuba, Hispaniola, Mazatlan in Mexico, Brazil, and the Galápagos. Its introduction into the United States is quite conceivable when the breadth of its present world cover-

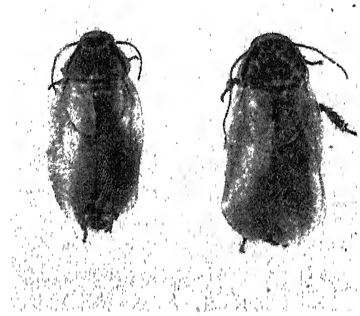

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FIG. 9. CINEREOUS COCKROACH

age is considered. In Indo-Malaysia it is broadly if not solidly distributed—in the Philippines, Sumatra, and Singapore—and it also occurs in Australia, while eastward it has reached New Caledonia and the Hawaiian Islands. It also occurs in Madagascar and Mauritius, and its East African records reach from Egypt, through the Sudan (where it occurs even in the huts of the Shilluk natives), to eastern Tanganyika. It has also been reported from the Transvaal and Natal, and there is one record from the Cameroons in West Africa.

In a recent detailed study of the African distribution of this and certain other African species of the genus, it is concluded that its native home was East Africa, that it spread to the Malagasy region probably through the medium of Arab trading ships, and that the more distant Philippine and similar Oriental centers were established through Portuguese or Spanish voyageurs. From the Philippines the western Mexican colony was probably founded by transport, in Spanish galleons, as with *Neostylopyga rhombifolia,* already discussed, that in Brazil by Portuguese traders on long voyages with well-established ship colonies of *cinerea,* while the Galápagos population doubtless was due

to camps of tortoise-hunting seamen from ships of numerous nationalities. The Cuban and Hispaniolan representatives may have come from the west in goods brought from the Philippines via Mexico, as the Atlantic galleons often called at Cuban or Hispaniolan ports in the sixteenth and seventeenth centuries.

An interesting case of localized introduction of a domiciliary species is that of the Buprestid Cockroach (*Oxyhaloa buprestoides*) (Fig. 10), which is a widely distributed African species, now long and thoroughly established in a localized territory in eastern Cuba. The genus otherwise is Ethiopian in its range. Most curiously the first technical name applied to this species, and that which we must use for it, was based on Cuban material, which, however, is entirely inseparable from very extensive African representations now available. Certainly the species was established in Oriente Province, Cuba, prior to 1862, but *buprestoides* has not as yet extended its range in Cuba over more than the eastern part of the island, although in tropical Africa, as I know from personal experience, it is widely distributed and abundantly represented. In 1893 Saussure and Zehntner reported the species from Mexico

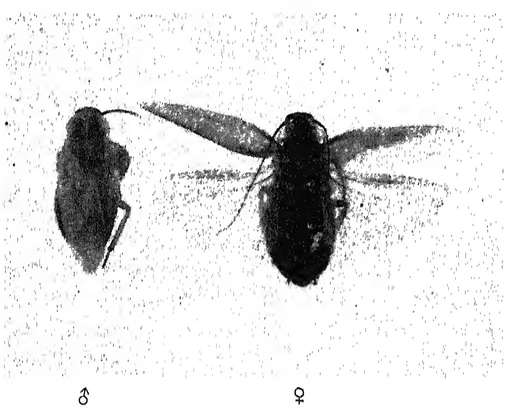

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FIG. 10. BUPRESTID COCKROACH

and Guatemala. These specimens more probably represent immigrants from the Cuban colony, possibly through the port of Santiago, than direct introduction from Africa. While *Oxyhaloa buprestoides* is widely distributed over tropical Africa, it has not been reported from the Oriental region, the American localities being the only extralimital ones, if that word may be used. I feel no hesitation in concluding that the New World occurrence of the species can be traced directly or secondarily to slave ship introduction from the West African coast.

Another quite attractive domiciliary cockroach is one for which no vernacular name has been used, but which may be called the Pale-bordered Cockroach (*Leurolestes pallidus*) (Fig. 11). It was described from and

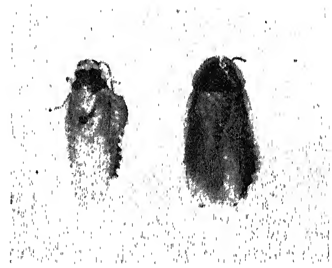

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FIG. 11. PALE-BORDERED COCKROACH

is common in Cuba, where it is found all over the island in houses, under lockers, boards, etc. It also occurs in Jamaica, Hispaniola, Puerto Rico, and certain of the Lesser Antilles. It has been recorded from Mexico, Guatemala, and Brazil, as well as the Canary Islands and southern Florida, where it has been encountered in Key West and on Key Largo. At Key West Hebard and I found it in a fruit store associated with *Blattella germanica, Periplaneta americana,* and *Supella supellectilium,* which gives an idea of its ecological associates.

I believe the occurrence of *pallidus* in the Canaries is due to colonization from the West Indies, and that the species, and incidentally the genus, is of West Indian origin. In *Leurolestes* we have, I am convinced, a reversal of the usual flow of blattid immigration; that is, movement from instead of to the West Indies.

Of the preceding eleven domiciliary species of cockroaches, five (*Periplaneta americana, P. australasiae, Supella supellectilium, Leucophaea maderae,* and *Oxyhaloa buprestoides*) very definitely reached America by the slave ship route from West African

sources; another (*Nauphoeta cinerea*) is also of African origin, but in part at least reached America by a more circuitous route; one (*Neostylopyga rhombifolia*) was probably of Indo-Malayan origin, or at least came from the Indian Ocean area; two (*Blatta orientalis* and *Blattella germanica*) almost certainly reached America directly from Europe, which, however, represented a way station on the long trek of these originally north or northeast African types; one (*Pycnoscelus surinamensis*) is of Oriental origin, but probably in part at least reached America via Africa in slave ships; and one (*Leurolestes pallidus*) is an endemic West Indian type, slowly spreading by commerce into southern Florida and other parts of the American tropics and subtropics.

Another ten years of careful checking on the presence in various parts of the world of certain of these fellow-travellers of humanity may greatly amplify our knowledge of what might be called the prehistory of their wanderings, but the basic conclusions here presented represent the results of some decades of careful study, and probably will be strengthened, rather than contradicted, by information yet to be secured.

JAMES A. G. REHN

JAMES A. G. REHN, Curator of Insects at the Academy of Natural Sciences of Philadelphia, was born in Philadelphia, in 1881. A boyhood interest in zoology eventually crystallized into a life-time application to entomology. Appointed in 1900 a Jessup Fund Student at the Academy, his life has been spent continuously at or in the service of that historic institution, of which he also has been Secretary or Corresponding Secretary for twenty-five years. His published entomological researches upon the systematic, distributional, and phylogenetic aspects of the Dermaptera and Orthoptera, the special fields of his work, total approximately three hundred titles. In developing the largest existing collection of these insects at the Academy, a considerable portion of his life has been spent in contributory field work, involving the entire United States, portions of Central and South America and the West Indies, and a cross section of Central Africa. In 1940 he served as Secretary of the Biological Sciences Section of the Eighth American Scientific Congress. For the past few years he has been President of the American Entomological Society, of Philadelphia, the oldest and most historic of its character in this country, and is the 1945 President of the Entomological Society of America.

The foregoing was written by Mr. Rehn. It is of interest to add that his enthusiasm for field work seems to be undiminished. In reply to an inquiry Mr. Rehn's associate, E. T. Cresson, Jr., wrote as follows: "We do not know where he is or whether he will return before mid-September. The removal of gasoline restrictions has allowed him to extend his collecting itinerary."

HUMAN ENGINEERING IN THE ARMY AIR FORCES*

By Major SAMUEL R. M. REYNOLDS

RESEARCH ASSOCIATE, CARNEGIE INSTITUTION OF WASHINGTON, ON LEAVE OF ABSENCE

OUR success in making it possible for our airmen to keep pace with their aircraft has been the result of the efforts of medical men and engineers, on the one hand, and of the lessons learned in combat, on the other. The successes are the more notable because the difficulties to be overcome were great and many, while the time in which success had to be achieved was short. The intensity of V-weapon warfare on our bases in England serves as reminder of this.

The human problems to be overcome stem from the following facts:

Man in flight is different from man on the ground, resulting from the effects of lowered barometric pressure and temperature upon the body.

The speeds at which some aircraft can fly—nearly 1000 feet per second— must be controlled by a body, some of whose reflex reactions require a fifth of a second—time for the aircraft to travel about 200 feet.

The centrifugal accelerations encountered, even in heavy bombers, may transiently impose upon the body a force equal for a moment to twice that of gravity, so that a man weighs, with equipment on him, 400 pounds instead of 200 pounds. The problem of controlling voluntary movements in or about an aircraft, along a cat-walk, or by an open waist window, or to escape through a small escape hatch, is difficult.

One good look at the environment of the combat flyer will explain what I mean. Take, for instance, the operation of a B-17 Flying Fortress or a B-29 Superfort. A glance in the pilot's cabin will convince one that the flight of a 30-ton B-17 is actually a complex engineering operation, demanding a coordination of manual and mental skills which put the driving of a five-ton truck or even a streamlined locomotive in the kiddy-car class of human learning. The compartment is lined—front, sides, ceiling, and part of the floor—with controls, switches, levers, dials, and gages. I once counted 130 of them in a B-17. I have seen a B-29 having a gross weight of 128,000 pounds take-off, fly, and land as if it were a commercial air liner.

The efficient operation of all the gadgets on an airplane would be difficult in the swivel-chair comfort of an air-conditioned office. *But*—cut the size of that office to a five-foot cube, engulf it in the roar of four 1,200-horsepower engines, increase its height above ground to four or five miles, reduce the atmospheric pressure by one-half to two-thirds, lower the outside temperature to 40 or 50 degrees below zero. Then, to meet these conditions the flyer must don bulky flying clothes with suit, gloves, and boots all heated through an electric cable, strap on his parachute harness, put on a "Mae West" life preserver vest, put on a flying helmet with earphones and another wire attachment, cover his face with an oxygen mask containing a microphone, making sure that the oxygen supply hose is connected and the microphone wire is plugged in, add a heavy flak helmet and about 23 pounds of body armor.

The man is now ready to go to work, not alone, not on a single problem, but as a member of a team of 10 men working on a multitude of problems. With them he will solve, while flying from one pinpoint on the map to another some 600 to 1,500 miles away, the higher mathematical relationships of engine revolutions, manifold and fuel pressure, aerodynamics, fuel consumption, oxygen supply, barometric pressures, altitude, air speed, ground speed, wind drift, compass heading, position, and plane attitude. He and his crew will do all this whether they can see the ground or are flying through an overcast, and they will do it not as an isolated unit but in relation to their

* From an address delivered before the Society of Automotive Engineers, District of Columbia Section, Statler Hotel, Washington, D. C., on May 8, 1945.

position in a formation of several dozen other bombers.

Of course, there may be occasional individual interruptions in flight operations due to an earache from pressure changes, a joint pain from the bends, a cramp from intestinal gas expansion, a little dizziness or nausea from air-sickness, a frost-bitten hand, a general numbness from cold, or the subtle effects of oxygen-lack.

Now consider the impact of these *unearthly* forces on the body. Flight destroys the sense of security and well-being which man, as the result of thousands of years of evolutionary adaptation, derives from "keeping his feet on the ground." The spirit of adventure and the thrill of flying are so strong that they quickly overpower the natural anxiety induced by projection into a strange environment. Yet, while man learns to like the idea of flying and develops a sense of mastery in the air, he remains either consciously or subconsciously afraid of his new habitat, and when he gets into trouble in the air he becomes acutely aware of his fear of falling. This is the psychological picture in *peaceful* flight.

Interject the greater fears of combat—the enemy fighter plane closing in with guns blazing—the setting of a steady course through a seemingly impenetrable wall of flak—the sickening bump when a shell tears a hole through wing or fuselage—the sight of other planes in the formation going out of control and at times exploding in midair—the danger of gasoline catching fire and engulfing the plane in flame—the sitting-on-a-keg-of-dynamite feeling of carrying a bomb load—the ordeal of dead or seriously wounded crew mates on board—the danger of asphyxiation or freezing if the oxygen or electrical systems are shot out—the hazard of having to bail out and clear a spinning plane with one's parachute—the possibility of a crash landing or a ditching at sea.

In this bizarre situation of intense concentration amid intense distraction, the fear of death becomes very real.

The maintenance of bodily efficiency of the flyer in the face of these stresses and strains is a trying task of aviation physiology. It is not easy. The aeronautical engineer has carried the mechanical performance of aircraft far beyond the physiological limits of the human body; technological warfare has imposed an abnormal load on the psychological motivations of the human spirit. The problem is to take a man whose body is adapted to function in a ground environment and whose mind is conditioned to seek peace and security and then fit him for the life of a flying, fighting animal. Since we cannot build the flyer to specifications or wait for evolution to turn him into a superman, we have but two alternatives: One is to select and train the individuals best fitted physically and mentally for this duty; the other, to *provide them with devices and methods for protection against their limitations.* It is this second alternative that will concern us here, although the first is a fascinating story in its own right.

HAVING done everything practicable to pick the best qualified man for flying training, the Army Air Forces' next task is not exclusively that of making him an expert in the mechanical and tactical operation of an airplane. High altitude flight imposes physiological stresses which require exacting application on the part of the flier to maintain his efficiency. Such forces as barometric pressure or temperature are capable of injuring or killing an airman who loses control of the environmental situation at high altitude. Therefore, a vital portion of his training must concern the body in flight. This necessitates training the airman in the rudiments of physiology, so that he is aware of the dangers of high altitude and other factors in operational flying, and of the absolutely necessary means of guarding against them.

Consequently, an "Altitude Training Program" was organized in 1941 and put into operation in 1942. This grew out of study of the physiological effects of atmospheric pressure changes dating from World War I and earlier; in fact, from the time De Rozier made the first human flight in 1783. The AAF training program makes use of such standard training methods as lectures, discussions, demonstrations, motion pictures, and literature. It is distinguished, however, by its use of low-pressure, or altitude, chambers in simulated flights made by groups of trainees. There have been more than 60

such altitude chambers in 45 Altitude Training Units. The units are operated by Aviation Physiologists with the assistance of enlisted technicians. The Aviation Physiologists include some medical officers, but are mainly doctors of a biological science trained in aviation physiology at the School of Aviation Medicine. Since July 1943, trainees have made more than a million man-flights in altitude chambers. Each airman is required to make three chamber flights during the course of his flying training, one to 30,000 feet pressure altitude and two to 38,000 feet. The first flight is devoted largely to a demonstration of the effect of oxygen lack. This brings home to each flier the subtle dangers of oxygen want as the first step in convincing him that rigid oxygen discipline is necessary for survival at high altitude. The second flight emphasizes use of the standard types of aircraft oxygen equipment, and the third, usually given a short time before the flier goes overseas, deals with the practical problems of operational flying and is administered to the aircrew as a unit.

While altitude indoctrination gives major emphasis to the problem of anoxia, it also covers the effects of cold, the effects of blocking of the ear and sinus passages with changing gas volumes and pressures, the bends, intestinal gas expansion, blacking out due to centrifugal force, and the profound difference that exists between vision in daylight and in dim light, at night.

The Altitude Training Program has been closely coordinated and integrated with the work of the Personal Equipment Officer, who is, in effect, the man responsible for seeing that the teachings of Aviation Physiologists are heeded in the theaters of operations. The Personal Equipment Officer is a ground operations position originated by the Medical Service in the Eighth Air Force and trained under medical direction at the AAF School of Applied Tactics, Orlando, Florida. Most of our Aviation Physiologists also have been trained as Personal Equipment Officers, and some are engaged jointly in both fields.

When one understands that from the human standpoint the thousands of missions of heavy bombers over Europe were dependent on oxygen discipline, one may appreciate the value of the Altitude Training Program. The contribution made by the Aviation Physiologist in his altitude chamber and by the Personal Equipment Officer who preaches the gospel of survival in flying units in this country and overseas may be judged from the statistics for anoxia accidents in the Eighth Air Force. The total anoxia accident rate among heavy bomber crew members was cut in the last year from 116 per 100,000 man-missions to 23, a decrease of 80 percent. This record is remarkable. It was achieved in the face of more than a hundredfold increase in man-sorties and an over-all increase in average bombing altitude of 5,000 feet—from 22,000 feet to 27,000 feet. This is *more* significant when one realizes that at 27,000 feet the length of useful consciousness without oxygen added to the air one breathes is less than five minutes; it is considerably longer at 22,000 feet.

The equipment which flyers wear in order to withstand the effects of altitude and the other stresses of flying have come to be called *personal equipment.* The personal equipment problems encountered by an operational air force depend on the nature of its mission, the geography of its operational zone, the climatic conditions in that zone, and the competency, preparation, and experience of the service facilities available to it. The problems encountered, therefore, will vary from one air force to another.

For example, the difficulties experienced by our air forces based in England were conditioned by the great strength and long preparation of the enemy, the hazard of flights over treacherous waters, and the rigorous climate over the northerly latitudes of Europe. Moreover, since these air forces embarked on large-scale offensive operations at a time when some equipment had to be used before it had been completely tested, they frequently served as proving grounds for the final testing of protective flying equipment.

In anticipation of these difficulties a new type of medical organization was set up in the Eighth Air Force—the First Central Medical Establishment. One of its main sections, the Department of Physiology, was charged by General Grow with the testing of all personal and safety equipment used

by flying personnel. The staff consisted of trained air corps and medical corps officers. By field trips and training Personal Equipment Officers, this organization maintained a close liaison with the combat squadrons all over England.

The circumstances calling for establishment of this activity are striking. Briefly, they were as follows:

For a month in early 1943 our air forces saved less than 1 percent of our airmen who were forced to land in the waters about England. At the same time, with greater activity and largely at night, the RAF saved nearly 30 percent of their ditched personnel.

By March of 1943, when our aircraft were forced to go higher and higher to avoid the vigor of enemy resistance, a new type of oxygen system came into widespread use. Because it was novel and required special handling, difficulties arose; one of every nine heavy bombers taking off for enemy territory was forced to return early because of oxygen trouble. With 10,000 man-hours in England alone put into every ton of bombs on the target in 1943, the cost of these aborted flights was staggering.

In the first six months of 1943, there were more casualties to our airmen from frostbite than there were from enemy action. This, too, was a frightening, and preventable, toll.

By development and procurement of equipment, by proper care, and by training, each of these difficulties was overcome. Before I enlarge on the manner by which these and some other problems were solved, I will present the results. By the end of 1943, we were saving nearly 70 percent of our airmen who ditched. On one raid alone, 218 of 221 men forced into the North Sea were saved! Last year, on D-day and D + 1-Day, 82 of 86 troop carrier crewmen were saved, and the four who were lost had been killed or wounded by enemy action before ditching. If circumstances permitted—which they do not—a still more interesting story could be told of our air-sea rescue activities in the Pacific.

Reverting to the early return from oxygen failures, we find that these diminished to such a degree that they were a minor, a very minor, cause of early returns from missions instead of third-ranking cause.

As for frostbites, adequate and proper clothing—especially developed—turned this from the first cause of casualties to nearly the last. While frostbite is of necessity a hazard of every high altitude flight, it diminished from a frequency of 250 per 10,000 men flying to 44 per 10,000. This advance is attributed to a number of factors, including installation of waist-window inclosures in heavy bombers, development of face protection, vastly improved types of electrically heated suits, gloves and shoes, and combined efforts of Flight Surgeons, Personal Equipment Officers, and Aviation Physiologists to indoctrinate airmen in the methods of preventing frostbite.

Before I describe *how* these and some other problems were solved so well, I must refer to another success which probably contributed more to the saving of our flyers' lives in military operational flying than any other single development. I refer to body armor, commonly known as the flak suit. This was employed at the insistence of Brigadier General Malcolm C. Grow, a Flight Surgeon and founder of the Aero Medical Laboratory. As surgeon of the Eighth Air Force in England, General Grow observed that 79 percent of wounds among heavy bomber crews returning from missions over Europe were from low-velocity fragments of common shells and that 85 percent of the fatal wounds occurred in head, neck, and trunk regions which could be protected by armor. He investigated the various types of body armor of medieval England, and with the collaboration of a British sword-maker, developed a quick-release vest, apron, and helmet of shingled steel. The flyers' fear of flak from antiaircraft guns, something against which he cannot fight back, made the body armor immediately popular. Study of a series of cases in which flak-suited flyers were hit by enemy missiles of all types showed that 69 percent were uninjured and an additional 21 percent were only slightly wounded.

Everyone now is quite familiar with the exploits of the B-29 Superfortress in long-range, high-altitude bombing missions against Japan. Aviation medicine played a significant role in the development of this super-

bomber. In fact, Flight Surgeons and Aviation Physiologists, teaming with aeronautical engineers, worked out the basic physiological problems of efficient human performance in the stratosphere long before the Superfortress was conceived.

From the medical viewpoint, the B-29's greatest distinction from previous bombers is the pressurization of its cabins to produce a relatively constant air pressure inside, irrespective of the great reduction of atmospheric pressure in a flight from sea level to the stratosphere. There is, as it happens, nothing new in the idea of compressing the air in a sealed cabin to maintain the partial pressure of oxygen in the lungs. More than 30 years ago, when the heavier-than-air flying machine was just getting off the ground, Cruchet and Moulinier observed with considerable clairvoyance: "As a matter of fact, the [oxygen] problem will never be satisfactorily solved until crew and passengers sitting in an airtight cabin shall breathe at all altitudes an atmosphere practically identical with that at sea level."

Let us review the physiological oxygen principles which give validity to this statement. Air has weight, which at sea level exerts a pressure of 14.7 pounds per square inch of surface. This weight decreases with increasing distance above the earth until at 40,000 feet the air pressure is only 2.7 pounds per square inch. This total atmospheric pressure of 2.7 happens to be slightly less than the partial pressure of oxygen at sea level. As a matter of fact, when the air pressure decreases to below the 10.1 pounds per square inch found at 10,000 feet altitude, the partial pressure of oxygen is no longer sufficient to maintain an adequate arterial saturation of oxygen in a flyer accustomed to living at lower altitudes. Above 10,000 feet the amounts of anoxia to which he would be exposed increase so rapidly that an attempt to breathe free air between 30,000 and 40,000 feet would result in loss of useful consciousness within 15 seconds and in death within a few minutes.

To maintain normal oxygen saturation of the blood at high altitude, the AAF provides the flyer with a standard diluter demand type of oxygen mask and regulator. The oxygen is supplied from low-pressure oxygen cylinders. Actuated by an aneroid and a demand value, the demand oxygen regulator reacts to reductions in atmospheric pressure by automatically increasing the percentage of oxygen in the air breathed by the flyer from the 21 percent found in free air to 100 percent when he reaches an altitude of 30,000 feet. If his oxygen equipment is functioning efficiently, his arterial oxygen saturation is still normal at 34,000 feet and will remain adequate up to 38,000 feet.

Above 38,000 feet even 100 percent oxygen is insufficient for physiological needs, and the accumulative effects of anoxia will develop with prolonged exposure. The only way to raise the flyer's service ceiling beyond this point is to increase the air pressure at higher altitudes.

This may be done with a pressure demand oxygen mask and regulator, which can increase the pressure of oxygen in the lungs throughout the respiratory cycle by 15 to 25 millimeters of mercury. This will prevent anoxia up to an altitude of 42,000 feet and may be used in emergency between 45,000 and 50,000. By maintaining a positive pressure which prevents any possible air leakage the pressure demand mask at altitudes in excess of 35,000 feet will provide, in addition, a margin of safety not obtained in the diluter demand mask. Nevertheless, it has many of the disadvantages inherent in any closed system of breathing. These include the necessity of maintaining a facial fit of the mask which is virtually sealed, the discomfort attending prolonged periods of wear, the possibility of the mask freezing at the subzero temperatures found at high altitudes, the possibility of small amounts of anoxia developing from malfunction, the general fatigue which frequently results from continued use of an oxygen mask, and the restriction of movement by an oxygen hose of fixed length. Moreover, building up pressure in the lungs exerts pressure on the veins carrying blood to the heart. Pressurization of the lungs alone is therefore not without potential danger.

The ideal solution to all this is the development of pressure cabin airplanes, such as the B-29. In this airplane, the forward compartment and the rear compartments are sealed and pressurized. They are connected

by a pressurized tunnel passageway over the bomb bay. In addition, the tail gunner's compartment, a lonely cupola under the rudder, is pressurized. Up to 30,000 feet it is possible to maintain a pressure which provides adequate oxygenation of the blood without the use of an oxygen mask. Thus the greatest practical advantage of pressurization today is to free the flyer from dependence on the oxygen mask at altitudes below 30,000 feet. If the airplane were to continue to climb above this altitude a point would be reached where the interior pressure decreased to the equivalent of 10,000 feet altitude and the aircrew would then be required to put on oxygen masks. In other words, the effect of pressurization would be to jack up the "floor" of the demand oxygen system from 10,000 to 30,000 feet altitude and therefore raise the theoretical service ceiling for this type of equipment from about 40,000 feet to whatever limit engineering places on the airplane's altitude performance.

The practical and potential values of cabin pressurization are numerous. In addition to freeing the flyer from the necessity of wearing an oxygen mask at moderately high altitudes, pressure disturbances of the middle ear, the sinuses, and the gastro-intestinal tract are greatly minimized. Moreover, pressurization prevents aeroembolism, or "the bends," which becomes a problem at altitudes above 30,000 feet. The sealed pressure cabin acts, to a great extent, as a sound proofer, and hence noise levels within the cabin are lowered. Finally, the heat resulting from air compression reduces the hazard of high altitude frostbite.

Low temperature is second only to lowered atmospheric pressure as a physiological hazard in high-altitude flight. Anoxia is far swifter than subzero cold but no less deadly. High-altitude frostbite, principally of the hands, has constituted a leading cause of battle casualties in heavy bomber operations at altitudes of 25,000 and 30,000 feet where temperatures of 40 to 50 degrees below zero are normal. But having indicated already the successes that have attended our efforts in this direction, I shall not dwell further on this subject except for one further point.

In addition to the electrically heated clothes, shoes, and gloves, to which reference was made at the outset, clothing design for AAF flyers has been a problem of major concern. The magnitude of the problem is suggested by the fact that flying clothing in 1942 was designed for aircraft most of which seldom flew to an altitude of 20,000 feet. The heaviest clothing was very popular; it was leather, lined with shearling. As more and more flyers were trained and as the cold temperatures to which they were exposed became lower when flying went higher, and as the duration of exposure increased, the inadequacies of such clothing soon became apparent. For example, men perspired in shearling clothes at lower altitudes and this imposed the danger of frostbite at higher altitudes. As this clothing was worn, the shearling became packed, and this resulted in a marked decrease in insulating efficiency. In short, demands for increased production, without clothing specifications that fulfilled the desired purpose, created difficult problems. Particularly were shoe and glove designs found to be inadequate.

The problem was approached effectively by the Clothing Branch of the Personal Equipment Laboratory at Wright Field. Through cooperation of a group of engineers, anthropologists, physiologists and clothing and textile experts, clothing of proper and effective design was achieved. On one hand, bulk was reduced and, on the other, improved and variable multilayer insulation was obtained. For example, this was accomplished effectively with respect to gloves. This was based on RAF experience. A rayon inner liner serves to protect the hand if outer gloves are removed for brief but necessary use of the fingers; next, a heavier five-fingered glove gives considerable protection, permits moderate movement and use of the fingers, and over these, a heavy lined gauntlet with the index finger free is provided for the long periods of time when such insulation is necessary and the hands need not be actively used.

The use of manufactured pile on the body clothing itself has marked a great advance over shearling. It is less bulky than shearling and, because it is stronger, it resists crushing in a surprisingly effective manner.

By using outer jackets that can be donned over others, the degree of insulation can be controlled by the demands of a situation in a way hardly possible with a single, heavy leather, shearling garment.

These are not idle considerations to the man who must fly. Consider, for example, the fact that, regardless of the ground temperatures, whether in the tropics, in the desert, or in the far northern latitudes, the air temperature over 20,000 feet is about the same at all latitudes, if certain allowances are made for seasonal differences.

The type of aircraft a flyer will use is of great importance in determining the type of clothing that can be worn. It is well known that fighter aircraft have very small cockpits. The problem is not simplified for a larger than average pilot who must wear a parachute, a life vest, a one-man life raft, a personal emergency kit, and clothing for insulation. It is true that fighter aircraft now have very effective heaters; but in combat these must be turned off as a safeguard against noxious gases. On long-range fighter escort missions at high altitude this has proved to be a real problem. Since operations in the Pacific could be carried out at lower average altitude than the vigor of German resistance permitted, the going with respect to clothes in that theater was easier for both flyers and ground echelons.

I should like to cite two practical instances of the way scientific thought permeates into air forces activities before I leave this subject. On the basis of available data, a chart was prepared by two scientists at Wright Field giving for every latitude and season the type of combination of air forces clothing that will be most effective in combating cold at any altitude up to the ceiling of our present aircraft. This chart, based on the known ambient air temperatures, the insulative characteristics of various combinations of clothing determined by tests made in cold chambers, and upon certain physiological considerations, is incorporated into the Stock List Catalogue of Army Air Forces Clothing.

One of the oldest problems and one of the newest advances in aviation medicine concerns the protection of the flyer against centrifugal force. This problem concerns the fighter pilot rather than the bomber crew inasmuch as it is our fighter and fighter-bomber aircraft which commonly perform steep dives or sharp turns as a part of their tactical maneuvers. The human body is constructed to function at the weight produced by gravity on a body at rest. When the body is projected through space it undergoes acceleration, which means a change in velocity either in magnitude or direction. Decreases in velocity present the pilot with no special problems as long as his airplane continues in a single direction, even if the velocity increases to 500 or even 700 miles an hour. Should something prevent him from continuing at this speed, for example, a mountain side, the problem of deceleration is obvious. There is a less obvious but real difficulty, however, when the airplane is traveling at high speed and merely changes its direction. This may occur in a loop or turn, when the pilot's body tends to continue in a straight line. This centrifugal acceleration is similar to a gravity at right angles to the tangent to the path the airplane is traveling; it will be toward the right if the airplane is turning toward the left and downward if the airplane is straightening out and turning upward from a dive. This centrifugal force, as it is commonly called, may be greater than the force of gravity. Ordinarily a centrifugal force four or five times gravity is about all a man can take without "blacking out." Blacking out is a visual phenomenon due to an interruption of the blood supply to the brain. The pull of centrifugal force operates from head to feet, and when the blood becomes four or five times heavier and tends to pool in the abdomen and legs, it becomes increasingly difficult for the heart to deliver blood to the brain. The heart, after all, puts out no more blood than it receives. Vision is affected first, being so sensitive to loss of oxygen supply that it will black out within four to eight seconds after the blood has ceased to circulate in the retina. At this point the pilot is still conscious, but if the excessive force continues a few more seconds, he will lose consciousness. When the centrifugal force ceases, it takes a pilot only three to five seconds to regain his vision, but if he has lost consciousness, it takes as

much as 15 to 60 seconds to recover. In dive-bombing or dog-fighting either a momentary loss of sight or loss of control can be disastrous. Early in the game, "hot" pilots learned to crouch, tense their abdominal muscles and yell or growl when they pulled back on the stick for a quick change in direction. These reactions helped to increase g-tolerance by making the body more rigid and thus restricting the pooling of blood in the abdominal and leg regions. These maneuvers not only had practical drawbacks but were extremely fatiguing and, through experience, fighter pilots learned not to exceed their individual blackout threshhold. Due to the great advances in aeronautical engineering, this put them far behind their airplanes in performance under the stress of centrifugal force.

Air-minded physiologists have experimented for many years in an effort to develop anti-g protective devices. Prior to World War II the Royal Canadian and Australian Air Forces and the United States Navy developed workable g-suits which in one way or another aimed at the restriction of the movement of blood from head to feet by the application of pressure on the legs and abdomen. In these suits, pressure during exposure to excessive g-forces was produced with water, carbon-dioxide gas, or air. The Navy suit was essentially a pair of tight, high-waisted pants and girdle containing 17 air bladders. It was supplied with a gradient of three pressures. In 1943 this suit was adopted by the Aero Medical Laboratory at Wright Field as the most practicable for tactical use, and experimentation with it was begun. This suit, which weighed 10 pounds, was found to be wholly effective in flight tests but too hot and cumbersome.

The eventual result of experimentation with the centrifuge for humans and in battle tests was a two-pound, single pressure g-suit which became standard wear for Army Air Forces fighter pilots in the European Theater of Operations. It was recently described in the *Readers' Digest* as the Air Forces *zoot suit*. This g-suit, which resembles a cutaway version of a pair of pants, contains only five air bladders placed over the abdomen, the thighs, and the calves. Operated by compressed air from the airplane's vacuum instrument pump through a gravity valve, these bladders automatically inflate or deflate when centrifugal force rises above or falls below 2 g. While the extra g-tolerance provided the pilot averages only about 1.5 g, fighter pilots wearing the g-suit have never reported a complete blackout. The g-suit has made an important contribution to fighter tactics as reflected in the statement of one fighter pilot: "I was never able to turn inside a Jerry before, but I did it today." The tactical advantage applies not only to plane-to-plane dog-fighting but to fast maneuvers in tight formation and to sustained deflection firing while circling. Evasive action during low-level strafing and dive-bombing, particularly to avoid flak, can be carried out more rapidly with the g-suit. In all these maneuvers the pilot can sit up and look around to the side and rear without fear of blacking out. The ability to keep his eye on the enemy is the fighter pilot's best life insurance, and many pilots have provided enthusiastic case histories of kills resulting from the protection provided by g-suits. Some have actually stated they owed their lives to it. The g-suit, which had its first general combat application in the Army Air Forces, constitutes a direct contribution of the physiological branch of experimental medicine to the superiority of our fighting men.

The sudden and intense forces of acceleration pose a number of problems for those charged with the care of the flyer. These are, generally speaking, the problems attributable to the impact of forced landings on land or sea, those due to bailing out of fast moving aircraft, and those due to landing by parachute.

How is the flyer protected? This depends upon the type of aircraft to which he is assigned. If seated, like a fighter pilot, or the pilot and co-pilot of a multiplace aircraft, the flyer uses a shoulder harness, tightened just prior to impact. This braces the man securely, making him for the moment of impact a part of the seat and even of the cockpit. This not only increases the duration of time over which the impact force acts but it increases very much the area of the body over which these forces act.

It can be demonstrated mathematically that, given a set of hypothetical circumstances, a flyer using a shoulder harness will experience a force of about five pounds per square inch, whereas in the same accident, using the customary lap safety belt, a flyer thrown so that his head hits a gun sight or other projection of about one square inch will receive a blow approaching 2000 pounds per square inch. Despite this advantage in favor of the shoulder harness, there are still flyers who would rather risk the latter consequences than use a shoulder harness with a quick-release device on it.

For the aircrew men for whom no seats and shoulder harnesses can be provided, other protective measures are necessary. The solution is to find a position for every man in which he can brace himself securely. For example, it is desirable to have the man seated with back and head held firmly against a bulkhead. The number of such positions available is limited, and second best positions must be taken. These are established for all air crewmen in every type of aircraft.

The dangers incurred in bailing out of aircraft moving at high speed are considerable. If, for example, a flyer who escaped from an airplane moving at 300 miles per hour pulled the ripcord immediately upon clearing the aircraft, the force acting in the direction of the aircraft would be far greater than when deceleration in this direction is over. In the former situation the parachute may tear; if it does survive the opening shock, a force of considerable magnitude is transmitted to the body by the supporting harness. If this has been improperly fitted, bodily injuries are sure to result. The means of effectively combating these dangers lie in two directions; one by training and the other by assuring that every parachute harness is firmly fitted at the proper points on the body. The latter is a responsibility of the Personal Equipment Officer. Without going into detail, it may be said that these and certain other measures have resulted in a very marked decrease in injuries sustained by our flyers who bail out of aircraft. At one time, statistics show, about 40 percent of our men sustained some sort of injury; now the percentage of accidents—and the general severity of them—is but a small fraction of this number.

I SHALL mention in passing an urgently pressing problem, and one which as yet has defied satisfactory solution despite extended study by the experts. I refer to the subject of seat comfort in long range fighter escort missions. The air crewman in a multiplace aircraft can stand up and move about during flights of many hours duration. The fighter pilot, however, is obliged to sit in the confines of a single seat, always clothed and equipped for bail-out, perhaps over enemy territory, over the jungle, or over water. He is obliged to remain in this fixed position for three to four hours at a time, on some missions. As yet, there is no way to provide even reasonable comfort, and the effect on bodily efficiency has yet to be measured under these circumstances.

IN another direction, the AAF medical services have established methods for testing and improving the flyer's night vision. Whereas the *cones* in the central portion of the retina are the principal organs for distinguishing color and detail in daylight, night vision is largely a function of the *rods* in the outer area of the retina. These rods are 1,000 times more sensitive in dim light than the cones. For most efficient function at night, however, the flyer must adapt his eyes to darkness by protecting them from light for 30 minutes prior to use—a protection which must be continued during the period of flight operations. This is accomplished without the necessity of remaining in darkness by the use of dark adapter goggles containing red lenses. Furthermore, the flyer must learn the off-center method of gazing at objects at night to bring his rod vision into full use and to avoid the night blind spot presented by the center of the retina when he gazes directly at the object. This is done by looking 15 degrees to one side of the object he wishes to see. The flying candidate's night vision is tested as a part of the physical examination for flying. Night vision indoctrination, including simulated firing at targets under moonlight and starlight conditions, is especially valuable in aerial gunnery training.

ANOTHER problem of interest is airsickness. A history of swing, train, sea, or other types of motion sickness is sought in the physical examination for flying, and the aviation cadet is continually observed during training for susceptibility to airsickness. Actual flight constitutes the best index. Study conducted at the School of Aviation Medicine based on the use of a swing test and upon reports of the relationship between airsickness and individual fear of flying have disclosed that the syndrome is a product of two factors: motion and emotion. Airsickness primarily due to motion has important differences from the preponderantly emotional type. In the former, which occurs only during rough weather or acrobatics, nausea is relieved by vomiting, symptoms disappear upon landing, and an immunity is acquired. The emotional type may occur before or after take-off and produce nausea not relieved by vomiting, sickness during smooth flight, headache, and symptoms after landing. Sixty-five percent of navigation cadets and 30 percent of pilot cadets experience airsickness early in training, but only about 10 percent of all navigation cadets and 1 to 3 percent of pilot cadets are eliminated because of airsickness. The symptoms of those eliminated fall predominantly in the emotional category and are linked with fear of heights, or of flying and other factors impinging upon emotional stability. Conditions commonly associated with airsickness, such as current diet, aircraft odors, temperature, vibration, and the sight of others being sick, have been discounted as having any stimulus in airsickness other than lowering the threshold at which disturbances of the sense of equilibrium may produce symptoms.

Space is lacking to go into several interesting subjects, e.g., the efficiency with which research engineers of some well-known manufacturing concerns have constructed oxygen regulators that take into account individual variations in rate and depth of respiration; how they have allowed for the limited tolerance of the human body to overcome resistance during inspiration in order to operate a demand valve for hour after hour, whether resting or periodically active; how they have provided oxygen of increasing richness, in accordance with the necessity to maintain a given partial pressure of oxygen in the lung alveoli. Our engineers, working in accordance with the specifications laid down by physiologists, have produced a regulator that meets the rigorous requirements for maintenance of bodily efficiency and that satisfies the aircraft designers by providing a system which, for the amount and pressure of oxygen carried, is of minimum weight and adaptable to such distribution of weight as the engineers require.

It would be interesting to discuss the progress which is being made in the field of cockpit design and standardization, and the mutually advantageous cooperation between our armed services by the setting up of Army-Navy Standards. It is true that some of these developments are aimed at simplification of manufacture and procurement, but whenever these engineering developments are considered, a staff of medical officers, aviation physiologists, and other specialists are consulted in order that human capabilities, reactions, and limitations will be evaluated. And so it is with considerable satisfaction that many biological scientists, both medical and nonmedical, can survey the success of the application of their work in the notable combat record of the Army Air Forces in the face of novel circumstances.

FUNCTIONS AND OPERATIONS OF THE NATIONAL ROSTER[1]

By ROGER C. SMITH[2]

PROFESSOR AND HEAD OF DEPARTMENT OF ENTOMOLOGY, KANSAS STATE COLLEGE

A GENERAL paper about the National Roster by President Leonard Carmichael of Tufts College (for more than four years Director of the Roster) was published in this Journal in February, 1944. The purpose of this article is to give a more intimate account of the actual operations of the Roster and the changes which have taken place in its functions during the five years of its existence.

The first purpose of the Roster was that of an inventory or registry that, as completely as possible, would record and classify the scientific, technical, and professional personnel of the nation. This purpose has been accomplished to as great a degree as could have been hoped for, and the National Roster became and remains a repository of more complete and up-to-date information on such personnel than is available from any other single source. The file of the Roster contained on December 31, 1944, approximately 440,000 registrations. The distribution by broad general fields is given in percentage in Table 1.

A second function of the National Roster from its inception has been placement. It is estimated that during its five years of activity the National Roster has made referrals of 170,000 registrants for commissions in the Army and Navy and for civilian positions in government and war industries. No detailed attempt was made to follow up these referrals, but it is estimated that about 50,000 of these positions were filled by the efforts of the Roster. This type of work still continues. The reconversion period will probably see a considerable increase in the placement activities of the Roster. All members of the Armed Forces who are qualified in the fields covered by the National Roster and who are seeking assistance in locating suitable employment are to be referred to the Roster as they pass through the separation centers and return to civilian life. They will be registered with the Roster if not already enrolled, and their records will be made available to employers if they are seeking employment.

A third function of the Roster may be summed up as fact-finding. As indicated in Dr. Carmichael's article, the Roster has been called upon to prepare reports on the personnel situations in physics, engineering,

TABLE 1

THE PERCENTAGE DISTRIBUTION OF ROSTER REGISTRANTS BY BROAD GENERAL FIELDS

Engineering		46.2
Mechanical	13.2	
Civil	10.7	
Electrical	9.5	
Chemical	5.9	
All others	6.9	
Physical Sciences		24.5
Chemistry	16.2	
Mathematics	3.3	
Physics	2.6	
Geology and Geophysics	1.9	
All others	0.5	
Management and Administration		13.2
Social Sciences		6.9
Agricultural and Biological Sciences		5.9
Architecture and Planning		2.1
Languages		1.2
		100.0

chemistry, geology, mathematics, and the agricultural and biological sciences. It has also evaluated the needs of industry for engineers and scientists and has surveyed educational institutions to acquire data necessary for the training programs of the Armed Forces. Findings of the Roster have been used by the Essential Activities Committee in connection with Selective Service deferments. In addition to several bulletins giving results of surveys, there have been written in the Roster a number of descriptions of

[1] Published by permission of the Director of the National Roster of Scientific and Specialized Personnel, War Manpower Commission.

[2] Former Professional Specialist for the Agricultural and Biological Sciences, National Roster of Scientific and Specialized Personnel.

professional fields, and handbooks outlining the detailed duties in each profession.

The Roster has had a varied but important part in advising the Selective Service System regarding the deferment of professionally trained men necessary to essential and critical activities. Approximately 17,000 such cases were referred to the Roster for recommendations either to State Directors or Local Boards of the Selective Service System. A like number of student deferment affidavits were processed, examined, and approved by the Roster during the short-lived deferment program of 1944.

The mechanics of National Roster operations indicate a systematic and thorough utilization of professional personnel records. It is of interest to trace the progress of an individual record as it is recorded and used. The original questionnaire is exceptionally complete and thorough to meet every possible need and to answer every possible question of those seeking specialized personnel. In addition to approximately two dozen specific pieces of information, the registrant gives on a detailed check list not more than four special fields in which he has competence. Each profession is divided into a number of main groups and these in turn subdivided into specialties. For example, a registrant indicates that his principal professional field is Organic Chemistry (Code Number 408). Under that heading the major division 4, Theoretical Organic Research, is selected and under that heading Organometallic Compounds (40.8.84) is checked.

When an individual questionnaire with its check list is received, all data are coded by well-trained operatives and are then transferred to a special 80-column punch card (Holerith card). From these cards two index cards are made and from one to four qualifications cards. One index card is put in a statistical file, and this file is used only for compiling general statistics as to registration, distribution by fields, extent of education, age, etc. The other index card goes into an operating field and is used to locate and assemble individual records according to any coded item. The qualifications cards are identical with the other index cards except for the order of the coded fields of proficiency. For example, if an individual is primarily an organic chemist in the field of synthetic drugs but has had some experience in polarigraphic methods of analysis, two qualification cards would be punched so that this man's record could be located under either heading. A log card shows the registrant's name, address, and serial number and these cards are filed alphabetically. Whenever an inquiry is made about the registrant, his name and address are sent to the "log room" and the number of his record is obtained. This is sent to the questionnaire file room, and the folder, containing the individual's questionnaire and all subsequent correspondence and records, is received.

When a call is made by a government agency or other organization for an individual of certain qualifications (age, education, experience, foreign language, etc.), the index cards are mechanically searched and a complete list of registrants meeting these qualifications is printed. The folders for all these individuals are sent to the placement staff for briefing and for use in communicating with registrant and prospective employing agency for arranging interviews and the usual placement procedures. In the case of requests from industry or other private employer, the only records selected are those of registrants who have indicated that they are available for employment.

Statistical information from registration records is gathered mechanically from index cards without dealing with any registrant as an individual. Only in cases of detailed study are the questionnaires examined. For example, the question may have been raised as to whether foresters are employed in the aircraft industry because opportunities in their own field were limited or because their special knowledge of wood was being utilized.

The Roster plans to carry on the functions outlined in this article, with continual adaptations to the peculiar needs of the period of reconversion and the succeeding peace and to render the greatest possible service to government, education, and industry and to the professions on which they depend.

THE LOGIC OF THE LOST YEAR

By THOMAS H. HAZLEHURST
PROFESSOR OF CHEMISTRY, LEHIGH UNIVERSITY

In the August issue of the Scientific Monthly there appears an article by Professor A. G. Keller of Yale University presenting a strong case for a national, compulsory, peacetime service bill. It seems only fair that some rebuttal be offered. In summary Professor Keller's arguments are as follows: The training will be military, but not narrow or harsh. It will be physically beneficial for the trainees. It will toughen but not brutalize their mental and moral fiber. It is essentially education—education in discipline and democracy. The so-called lost time for formal education can be avoided by deleting unnecessary training in grade school, high school, or college, or by accelerating. The training is necessary for defense. Without it, "we deserve what is coming to us."

Military training need not be harsh but it is necessarily narrow. It is a training in the tactics of offense and defense in organized warfare and is as specialized in its approach to a trainee as a master mechanic in his approach to an apprentice. Moreover, as has been abundantly pointed in numerous articles and speeches, the training received during the next few years will be outmoded when and if the next war comes. If we may assume that it will take at least as long after this catastrophic disturbance to get ready for a new war as it took after World War I, the proper time to start training young men for defense is in 1962. Had we begun compulsory military training in 1937, 23 years after the start of World War I, we would have been prepared for World War II when it came, except that we should have had to revise all the tactics after we recognized the new weapons and methods employed by the Germans.

If, on the other hand, the training is not to be for organized warfare, it is difficult to see how it contributes very directly to defense. If it is to be "vocational," it is a far more expensive method than that of providing federal scholarships to vocational or professional schools. Our experience with the misfitting extensively practiced by the army at present lends little encouragement to the notion that men will be treated individually or assigned to tailor-made or even altered-to-fit jobs.

Military training, largely physical, would undoubtedly improve the health and physique of most trainees. It is to be expected, however, that the details of a service act would provide for a physical examination before induction and a subsequent weeding out of the "physically unfit." Only the good will be accepted and made better, whereas those who really need corrective measures will have to rely upon whatever advice and assistance is afforded by government agencies for civilians or upon such aid as the man or his parents may have the desire and the money to employ. As for the "limitation of intoxicants," army life at present seems to indicate that those who wish to drink will find the means to do so, temperately or otherwise. Has the army reformed the less desirable citizens who have been inducted in this war? Would the army accept trainees from a reform school then any more willingly than it accepts convicts now? Aside from obvious and admitted crime such as the murders and the large-scale racketeering which have occasionally been outrageous enough to crash the censored headlines, is it not true that army life teaches a man to provide for himself by becoming an accomplished scrounger? Is there any reason to believe that a peacetime service corps, without the patriotic stimulus of "saving the homeland," will be any less addicted to skimping, shirking, cheating, passing the buck, and, very occasionally, in addition to these normal rear-area practices, indulging in a spot of drunkenness, murder, or rape?

And, of course, discipline. Appalling as is the lack of discipline in modern education there seems to be a swing away from the extreme laxity of a few years back. It

would be futile to deny that 20 years in the army would impress upon a man a strong habit of obedience to those authorized to order him about. It would be equally futile to maintain that a single year of army life would establish a firm habit of submission to authority, one which would survive the man's discharge from the corps and make him a less rebellious workman, student, or citizen. The imposition of unaccustomed discipline and restrictions almost invariably produces a rebellious reaction and a determination to make the most of freedom when it comes, in this case at the end of twelve months. How do furloughed soldiers behave now? Those who used to celebrate by painting the town red still do so, insistently. Those whose habits were originally less exuberant and more law-abiding celebrate inoffensively. However, we must agree that a little training in discipline is better than none and that some training in it, either in civilian education or in a service corps, is clearly indicated.

Like Scrooge's sister, education in democracy has always been a delicate flower that a breath might have withered, but, also like Scrooge's sister, it usually survives its frailties long enough to produce a hearty and likeable offspring,—but not if regimented. Indoctrination (nee Propagandizing) in Democracy could and, of course, would be carried out efficiently and well in the training camps—far more intensively and successfully than it could in the civilian educational system. We should remember, however, that democratic procedures are just what the army does not employ. The authorities which must be obeyed (and with no right to strike) are not elected and are under no supervision by those who must take the orders. Army life stresses uniformity, conformity, and unquestioning obedience to authority imposed from above. Democracy is moribund unless differences, heretics and cranks, and constant criticism of delegated authority abound.

There are many brands of Democracy. More than superficial differences distinguish the Democracy of which Soviet Russia boasts from that in France, England, or Sweden, and even these last are not identical with our own. Our Democracy calls for a minimum of federal supervision. The current army manual is always *right,* and the Democracy taught by army methods would be unlikely to permit, much less to encourage, questioning and differences of opinion. Teaching millions of young men a selected system of democratic principles is getting close to that regimentation we wish to avoid. All-in-all, it is very questionable whether the trainees would emerge from their army boarding school with a much more trustworthy political philosophy than they had when they entered it, and it is certain that civilian education, with its discussion groups and wide variety of opinion, has a far better chance than the army has of imparting to the citizenry a genuinely democratic viewpoint.

The mingling of young people of different social and racial backgrounds *should* certainly promote understanding and cooperation, which are the essence of successful practical democracy. But we must not forget that the terms "kike," "wop," "nigger," "hunky," and the like are used just as freely in the public as in the private institutions and that race riots take place as frequently in the North and West where segregation is not semilegal as in the South where it is. There are plenty of cliques and snobbish clubs in public as well as in private schools. In any large mixed group each person mingles by preference with those of most congenial background. Companionship in a fox-hole or a small army post is one thing; mingling in a large training camp is quite another. The possible benefit to Democracy derived from the heterogeneity of the group is probably vastly overrated.

To justify his thesis that the "lost year" could be made up without genuine loss Professor Keller presents a rather scathing criticism of the state of American education. It is pseudocultural. Some courses are boring. Much time is "wasted." If these items of little or no value were omitted, a year might easily be saved. (Having saved it, of course, the proper procedure is to spend it at once upon a year of compulsory training.) As Professor Keller says, no educator

is ever satisfied with the way in which education in general and his own part in it in particular are being handled. Any educator would admit that some useless material is taught and that some material is presented without being taught at all. It is the effort to decide just *what* is badly taught or superfluous that causes bitter feuds within the academic walls. No schoolboy, no college student, knows *certainly* what he will be called upon to do in life. His well-laid plans may be the very proposals of which God will dispose by filing them in the cosmic waste basket. Neither the pupil nor the teacher can tell definitely just what is superfluous, although any pupil will state categorically what he *considers* useless and any teacher can list certain courses (taught by others) which are mere hangovers, with academic inertia as their only excuse for existence. Certainly for those students who enter college with only a vague notion of what they intend to do with their lives, or whose plans involve college solely as a cultural experience, it would be entirely impractical to select the "must" subjects.

Army training would not be faced with this difficulty. The army knows precisely what it wants the man to be trained for: a specific job in a combat team, a supply depot, a communications group, and so forth. The army spokesmen say categorically and with feeling that they do not want that twelvemonth of specific training to be split up into four three-month summer camps. They want one continuous period of twelve months because that is the efficient way to train men. This is precisely the argument which the educators of preprofessional and engineering students have been urging. To interrupt the man's technical training, which is arranged in a carefully integrated and sequential whole, is to make that training far less effective. Also, engineers, doctors, and scientists are admittedly far more useful to a general defense program when they work in their own fields and utilize their specialized education than when they are shifted to a new technique for which their training has given them no background. It is surely more wasteful to snatch them from their valuable education for a year of vegetation than it is to pad college curricula with 15 or even 30 semester-hours of superfluous or worthless material.

It could be argued that *all* formal education at the college level, which would be the level most affected by the proposed program, aside from preprofessional training, is wasteful or at least a mere superfluous luxury in a social sense. There are certainly many men in college who, through lack of natural ability or lack of motivation, have already reached the limit of their formal education and are so much deadwood on the campus. And there are many who use their college courses to prepare them for creative work in literature or art, activities the intrinsic value of which is sometimes denied by the "practical" man. But these students too have a strong urge to obtain a diploma, even if some of them decline to do more than the absolute minimum of work required to obtain it, and to remove them from formal study for a year is to risk ending their college careers. Study habits are very easily lost and are regained only with difficulty. Men who have been forced to interrupt their education and subsequently try to resume it find the going very hard. Many of them fall by the wayside. Where is the farsighted wisdom in making these men and others forfeit their diplomas and what those diplomas stand for on the ground that they will be serving their country by undergoing a training in techniques which will be outmoded before they are used? It would be a tragic and enormous waste of a somewhat rare commodity, intellectual ability.

If, in spite of the weighty arguments against it at this juncture, a compulsory training bill is inevitable, at least let us avoid the ruinous loss of trained and trainable minds which we have suffered in this war by our short-sighted policy of avoiding the appearance of class distinction and inducting our preprofessional students as though the greatest service they could perform for the war effort was to fire a gun or drive a truck. Let there be exceptions made for college men with a certain standard of scholastic attainment. And that does not mean proficiency on the gridiron. Nostalgic references to the coach as the man who

taught me the most that was really worth knowing are good inspirations for sentimental doggerel, but a man's ability to earn the leisure in which to reminisce depends upon his performance in the classroom and study-hall, rather than in the game in which he helped to trounce the traditional rival.

Whatever may be the moral effect upon the trainees who pass through the program, the mere institution of the plan will have a profound effect upon the morality of the general public. Under the stress of perfervid wartime patriotism, we have submitted, for some five years now, to a long list of restrictions. We have been drafted, we have been rationed, we have been taxed. Even in this period of submission we have had much draft-dodging (some half-million cases are said to have been investigated to date), plenty of black market, and ample tax-evasion. When it comes to donating a year of one's own or one's child's life to one's country, not in an emergency but just as a general precaution, who can estimate the extent of the lying, evasion, string-pulling, and skulduggery that will be evoked? Even now we know of draft boards rather softhearted about granting deferments, a bit lenient in interpreting directives, or a little forgetful, always of particular names on the list. What will it be like then? Or are Americans so superior to the citizens of those nations which have struggled with compulsory service for so many years that every young American of 17 will draw himself to his full height, look the Procurement Bureau in its collective eye, and say in a voice hoarse with emotion: "I regret that I have but one year to give to my country?" Of course most American boys would comply voluntarily, if grumblingly, but it would be just those actual and potential juvenile delinquents, the dead-end kids and the like, the ones that are to be benefited by the program, who would find ways to dodge.

The logic of the lost year is that, having allowed our emotions to assume command and having instituted immediately a training program vastly expensive in years as well as dollars, after some five to ten years of peace and moderate prosperity, no one will want the program any more except the Army and those Cassandras who foresee the next war. By popular demand it will be discontinued at the very epoch when it might begin to become useful. Wars do not come overnight. It is only the final act of violence that arrives with startling suddenness. If our Cassandras could become sibyls and command the faith of the public, our training program could begin years hence but in ample time. The tragedy of it is that if the program is not put through in the heat of war it will probably take the commencement of violence in the next war to produce it.

The fact remains that to pass a national, compulsory, peacetime service bill at this juncture would mean a lost year for every one of the millions of trainees, and, especially for those of college caliber, it would threaten the stultification of several subsequent years. Not one lost year, but many!

A PHILOSOPHER'S REPLY TO A SCIENTIST'S ETHIC

By PATRICK ROMANELL
DEPARTMENT OF PHILOSOPHY, COLUMBIA UNIVERSITY

THE leading article of the April 1945 number of THE SCIENTIFIC MONTHLY (Vol. LX, No. 4, pp. 245–253) is entitled "Ethicogenesis" by Chauncey D. Leake, vice president of the University of Texas Medical Branch in Galveston. This well-documented and thought-provoking paper is "a deliberate attempt to establish an ethic on the basis of scientific knowledge and in scientific terms." (253)* To substantiate his thesis for a scientific ethic, Dr. Leake uses two interrelated arguments which we may call and state summarily as follows:

The Methodological Argument. The "descriptive" approach of modern science should replace the "normative" approach of traditional philosophy in the field of ethics if the possibility of ethical science is to be realized. (247)

The Darwinian Argument. Goodness should be measured in terms of its "survival" value to the human species. (248)

Leake starts off rather cavalierly by charging that, "to the professional scientific worker, discussions on morals by professional philosophers usually appear to be metaphysical irrelevance in the face of our present knowledge" (245), and by making an earnest plea that philosophers henceforth follow the example of Spencer, James, and Dewey, and apply the scientific method to all fields of human endeavor.

In view of the general fact that most philosophers have been seriously challenged by Leake as a professional scientist, and in view of the particular fact that the article, "Philosophy in a World at War," published in the March 1942 number of *Fortune* by my former teacher and present colleague William P. Montague, Johnsonian Professor of Philosophy at Barnard and Columbia, is specifically accused by implication of "metaphysical irrelevance" (245), I therefore think it is our duty to answer the plaintiff without apology and fear of becoming a *persona non grata* to the republic of science. As devil's advocate, I shall speak for Montague and try to show that each of the foregoing arguments for establishing an ethic on the mere basis of "biological evidence" (253) is subject to serious and relevant criticism. Since my subsequent remarks constitute a "rejoinder," I shall concentrate on the polemical angle of the questions at issue and only touch lightly on those points about which I am in substantial agreement with the author.

CRITIQUE OF THE METHODOLOGICAL ARGUMENT

To begin with, we wish to side wholeheartedly with Leake's plea that we should apply the scientific method to "all fields of human activity and interest" (245) as much as possible. No thinking individual today can deny that our robust faith in science means that we ought to make the most out of it and not just be half-scientists. However, our support of this needed extension of the scientific method in practice does not commit us at all to his peculiar emphasis as to what the method of science is in theory nor to his over-confident trust in its all-sufficiency. *Caveat lector!*

According to the author, the general procedure of science may be defined essentially as the "descriptive approach." It is strongly urged throughout the article under consideration that such approach should be applied to ethics and be substituted for the "normative" or metaphysical approach of the "classical ethics of philosophical thought." (247) This conclusion may be taken as the initial phase of his methodological argument for a scientific ethic.

To the above phase of the methodological argument there are two logically possible

* The number in parenthesis following a quotation from "Ethicogenesis" is the number of the page of the SM from which the quotation was taken.

answers, one from science and the other from ethics. Let us now consider them in turn.

The scientific answer to the author's explicit claim, which appears at the close of the methodological preface to his moral analysis, to wit, that the "scientific method of arriving at the 'truth' is now well defined in theory and practice" (248), is our counterclaim that, in spite of the fact that he knows what the scientific method is in *practice,* he does not have a clear and distinct idea as to what it is in *theory.* He senses, to be sure, that science is not merely an empirical *description,* however accurate, of the so-called "raw" facts, but a rational *explanation,* however tentative, of them as well. He knows almost by instinct that the scientific method in general is *rational observation;* that is, neither observation nor reason *alone.* (248) Yet, he keeps putting the accent on the "descriptive" approach throughout his whole discussion and talks as if the observational or the experimental element were the only "characteristic of scientific effort" (247) or the only factor of importance in the process of science. Of course, such Baconian emphasis is psychologically intelligible as a critical weapon to combat blind authoritarianism and idle speculation, but it is not logically justifiable, since a good definition of the scientific method must include *all* its essential attributes.

Moreover, Leake is aware of the importance of *"experimental reasoning"* in the "way of mathematics," but at the same time he isolates the quantitative type of analysis from the "way followed in the natural sciences" (248), and thereby fails to appreciate that "modern science really dates from the union of the experimental and mathematical methods of procedure,"[1] as Montague insists. In other words, the author keeps the two ways of knowing so separated that he does not seem to realize the important role of the mathematical chains of reasoning *within* the very realm of the natural sciences. A mere glance at the mathematical structure of modern physics should convince one of the intimate *union* of the two ways. Whether we accept or not Galileo's metaphysical faith that "the Book of Nature is written in mathematical language," modern science at least accepts the workable methodological faith that reason is written in mathematical language. Although it may be difficult for a pharmacologist like Leake to swallow such mathematical pill, the relentless logic of modern science, which he enthusiastically professes, may well nigh compel him to take his own medicine for consistency.

To summarize, our first criticism of the author's methodological analysis is that he pays too much attention to just one aspect of the general pattern of science and thus tends to commit the fallacy of exclusivism. We say "tends" deliberately in fairness to the fact, which we have already admitted, that he senses the necessity of a broader definition of the scientific method.

As the first objection points to a deficiency of commission, the second points to one of omission. The author omits a discussion of the connecting link of the two alternating stages of the empirical-rational and the inductive-deductive cycles of scientific inquiry, namely, "constructive imagination," whose important part in the formation of the hypotheses of science was appreciated in the last century by John Tyndall in his famous lecture on "The Scientific Use of the Imagination." Despite its evident importance, Leake dismisses intuition as "little more than logical inductions made so rapidly that the maker is not conscious of the steps in the process, until they are analyzed." (252) The fact that this doctrinaire dismissal occurs in a passage concerning religious "revelation" may throw some light on why intuition is scientifically suspect to him and why constructive imagination is the missing link in his conception of science.

The omission of the imaginative factor in scientific theory is rather surprising for a professional scientific worker like Leake who admires Charles Darwin so much. Didn't Darwin arrive at the epoch-making theory of evolution through his constructive imagination "brooding over the facts" of biology and working on his memories? The history of scientific discoveries, both great and small, shows in the concrete and in varying degree that constructive imagination (not the irresponsible and idle kind, of course) is, so to speak, the bridge between the empirical and

rational poles of scientific procedure. To be sure, all imagined hypotheses in science must be tested by experimentation, whenever and wherever possible, if they are to lay any claim to intellectual validity, but they must *first* be *proposed* before they can later be confirmed or refuted by experiment. This initial step is precisely the work of constructive imagination. As Montague is fond of putting it: "In short, we may say that imagination proposes and reason disposes."[2]

What we have previously called the initial phase of Leake's methodological argument for a scientific ethic can be divided into two parts. The first part *affirms* the validity of the "descriptive" approach in general; the second part *denies* the validity of the "normative" approach to ethics in particular. The foregoing brief survey of the fundamental components of the general method of science constitutes our answer to what he affirms. We shall now present in what follows our answer to what he denies. If we succeed in indicating the exact nature of the "normative" in the field of ethics, we shall thereby rescue this most basal of the categories of moral understanding from his irrelevant attack. Our subsequent task is to demonstrate that it is impossible to talk ethics without the category of the "normative."

In any search for knowledge there are two distinct problems to be granted priority: one refers to the purpose of the investigation and the other to the class of objects to be investigated. In other words, any intellectual injury involves a *for what* and a *what*, respectively.

From the standpoint of aim, inquiries may be divided into two general groups, *existential* and *normative*. An existential inquiry, by definition, is interested primarily in studying objects as they *actually are;* a normative inquiry is concerned mainly with studying objects as they *ought to be* or with evaluating objects. Ethics, by definition, is necessarily a normative pursuit because it aims at determining systematically whether a special class of non-sensory objects of human conduct or character has or has not the specific type of value designated as moral. Whereas, for example, biology is a study of life as it exists in nature, ethics is an examination of the *good* life, whether it exists or not.

From the standpoint of content, objects as such have a determinate structure to which the mind in its cognitive effort directs its attention. Now just as there are two groups of inquiries with respect to aim, so we find on examination that there are correspondingly two general classes of objects, *fact-objects* and *value-objects*. The objects of existential inquiry are simply called facts, those of normative inquiry are called values. The essential difference between biology and ethics, with respect to subject matter or object matter, to be more exact, is that the former studies life as a series of biological facts and the latter studies the good life as a series of moral values. Although it is possible to investigate moral matters as facts, they then fall within the scope of the social sciences and not strictly within the scope of ethics proper. In brief, it is only when moral facts are analyzed and appraised as values that ethics is appropriate to them. This conclusion establishes the *autonomy* of ethics as one type of normative inquiry.

Before going any further, it is important to note that the previous distinction between the existential or "is" kind of inquiry and the normative or "ought-to-be" kind is based on the classification of *aim* and has nothing to do with the question of approach or the *how* of inquiry. There is no such thing as the "normative" approach implied by Leake's whole methodological argument against "the classical ethics of philosophical thought." (247) The normative is relevant to the *for what* and the *what* of inquiry but irrelevant to its *how*. Hence, the author's conclusion that the "descriptive approach" should replace the "normative" one is based on the false premise that there is a normative method to replace in the field of ethics. As a matter of fact, the descriptive approach is common to both existential and normative inquiries. The difference between the two does not lie in that one describes and the other does not, but in the *what* each specifically describes. The fact that the descriptive approach has yielded better results in existential inquiries than in normative ones has

no bearing on the present point at issue. Moreover, when he observes that classical ethics is "dogmatically 'normative' and has scarcely considered the 'descriptive' approach characteristic of scientific effort" (247), he is guilty of committing not only the fallacy of false premise, but that of misplaced status in shifting the locus of the "normative" from the teleological and ontological contexts, where it properly belongs, to the methodological context, where it does not. This confusion of contexts, which is found in writers who classify inquiries on the basis of aim into "descriptive" and "normative" instead of into existential and normative, is the cause of the dislocation of the proper perspective on the moral situation and is responsible for Leake's not seeing that the normative is *intrinsic* to ethics. For ethics without the "normative" end in view and "norms" as content is a contradiction in terms. Philosophical ethics, if you like, may be bad on other grounds, but to indict it for being "normative" is similar to indicting science for being "scientific."

If our reasoning has been cogent, the conclusion follows that ethics *ut sic* is a normative affair with respect to both aim and content. This conclusion denies the validity of C. H. Waddington's position, which is accepted by Leake, namely, "that ethical judgments may be statements of the same kind as scientific statements" (249), since ethical judgments are normative statements concerning values and *not* of the same kind as scientific statements concerning facts. A principle of morals differs from a principle of nature as gravitation, for example, in two major ways: (1) a moral principle such as justice can be violated without destroying its validity, but a physical principle cannot; (2) a principle of nature tells us what is so, but a principle of morals tells us what is desirable and not necessarily so.

Lest our defence of the normative character of ethics be misunderstood as "metaphysical irrelevance," let us hasten to note that the radical difference between moral and scientific statements does not imply that ethics should formulate its principles of conduct independently of the available knowledge of human nature supplied by psychology and other pertinent sciences. It is obvious that a sound ethic for mankind must come to terms with the basic facts of human nature, but it should be equally obvious that all such facts put together do not add up to the system of morality we should live by. We should begin, of course, with the facts of human nature, but not end there. This empirical starting point is to act as a springboard to the vital business of ethics. The *ideal* ought to be certainly connected with the *real* in order to be relevant to the human scene, but ought also to be carefully distinguished from the real in order to be valid. As Montague expresses the relation of the two realms of discourse: "Existing situations do, of course, determine which values are relevant to their time and place, but success or failure in realizing the appropriate ideal neither creates nor destroys the validity of that ideal."[3]

The foregoing passage also serves to discredit the author's misinterpretation of Montague's profound belief in the Platonic vision of absolute values. Leake may be right in affirming that science "finds no evidence for the objective existence" (245) of the absolute principles of Truth, Beauty, and Goodness, but he is wrong in implying that Montague believes in their "objective existence." Now the latter's argument for their absolute value is not based on their alleged objective existence or *reality* in the ordinary sense, but rather on their genuine objective validity or *ideality*. After all, Montague tempers his Platonism with common sense, and his faith in absolute ideals not only prevents him from making believe that "what is is right," but also prevents him from making believe that "what is right is."

To return to our analysis of the good from the normative standpoint, the "ought" and "ought not" (or their moral equivalents) are not in truth the wishful "rationalizations" sanctioned by traditional philosophers, as the author suspects (247), but rather the axiomatic polarity of any ethical inquiry. Morris R. Cohen is right in holding that any moral system must by definition start with an "indemonstrable assumption", in terms of which everything else within the system is demonstrable, "since we cannot

have an *ought* in our conclusion unless there is an *ought* in one of our initial assumptions or premises."[4] The essential difference between Kant's "categorical imperative" and what we may term Leake's "biological imperative" lies precisely in the *specificity* of the "ought," not in that the first has an "ought" and the second does not have it, because the Leakian conclusion that we "ought" to survive is just as much an "ought" as the Kantian conclusion that we "ought" to universalize our conduct. To be sure, the sceptics and their not so extreme brethren the relativists, like the author, in their "natural reaction to the absurd claims of moral absolutism," are justified in "insisting that there is an arbitrary (in the sense of volitional) and indemonstrable assumption in every moral system," but from this it by no means follows that "moral systems contain nothing but assumptions or that all assumptions are equally true or equally false."[5] We still have the important job of determining *which* of the possible imperatives is the best guide for mankind.

Moreover, we may or may not agree with Kant's monumental "categorical imperative" as a moral principle, but we certainly cannot dismiss it as "religiously a matter of persuasion, of exhortation." (247) For there is a big difference between the "exhortations" of a Moses and "the great intellectual effort of Immanuel Kant." (247) As we might put it in language familiar today, Moses worked for Jewish *morale* and Kant wrote for a universal *morality*. And speaking in behalf of a Moses, don't we need morale and exhortation at times when things are going tough? Be that as it may, the real problem of reflective morality is not to do away with the "normative," which is a logically impossible undertaking, but to determine as adequately as possible what we "ought" and "ought" not to do *concretely* in any given ethical situation. And as there is no royal road to truth, there is less of a one to goodness.

So much, then, for what we have called the initial phase of the author's methodological argument in behalf of a "descriptive approach" for a scientific ethic and our two answers to it, (1) the scientist's answer derived from the general nature of the method of science and (2) the moralist's answer derived from the general nature of the field of ethics. Let us now proceed to examine its ultimate phase.

The ultimate phase of Leake's methodology is expressed in the belief which he shares with Julian Huxley and A. J. Carlson that "the answer to the question of the insufficiency of science is more science." (245) From this basal premise it is inferred that the scientific approach needs no supplementation of "metaphysical or supernatural considerations" (245), that philosophy is alas! dead, and the "devotions" of metaphysicians, in the funeral speech of Hugh Miller, "are a wake, administered to a corpse." (246)

Apart from the rejoinder that theologians may offer to the above indictment of superfluity, we as philosophers must ask: How can the body of philosophy be resurrected from the apparent death imposed by the successful invasion of the natural sciences which have been developed so impressively in the modern world?

To meet this death charge, let us consider, first of all, the logical problem of the definition of terms. If "science" is extended to mean "knowledge," then, of course, the original premise turns out to be a truism, to wit, the answer to the question of the insufficiency of knowledge is more knowledge. There's no argument about that! Nevertheless, this still leaves open the problem as to whether "science" in the modern sense of the term and "knowledge" are completely identical in connotation and denotation. Although "science" literally (from the Latin) means "knowledge," it is misleading to identify the two terms outside of the etymological context, especially since the history of modern science has prided itself so much in its persistent efforts to distinguish its type of verifiable knowledge from that of so-called common sense and philosophy. All this may seem rather trite hairsplitting on our part, but it is important for what is to follow to have disengaged Leake's contention from its truistic form, about which there obviously can be no argument.

In order to prove that science in the modern sense is not enough in our quest for truth, let us consider what would be the ideal of knowledge. Perfect knowledge, like an organism, would have a double aspect. On the one hand, it would be as deep and as broad as possible, and on the other hand, it would be as detailed and as particular as possible. The former properties constitute the *whole-aspect* of the body of knowledge, the latter its *part-aspect*. In other words, our intellectual goal is to know both *everything* as a whole and *every thing* down to its minutest detail. To be sure, that is what we would like to have, but we hardly need a reminder of how far we are from such goal. We fully realize that the deeper and broader our knowledge of things is, the less are the chances of its being certain. As the extensive element of richness of content increases, the intensive element of certainty decreases. But we should likewise admit the reverse of this principle. The more detailed and particular our knowledge is, the less are the chances of its being significant. As the intensive element of certainty increases, the extensive element of richness of content decreases. The intellectual life, like any life, must sacrifice something and pay its price for what it holds dear. In brief, selection always implies exclusion, whether in extension or intension.

Returning to our biological metaphor, knowledge-of-the-whole,—that is, the deeper and broader kind of knowledge, is not the simple sum of knowledge-of-the-parts, the detailed and particular kind, just as a living organism is not merely the simple sum of its parts. Integration in the calculus of knowledge is not ordinary addition. Moreover, the tragic element in the human drama of knowledge lies in the fact that growth in one aspect of knowledge is not usually accompanied by a corresponding growth in the other. The intellectual story of mankind is a revelation in the concrete of a "cultural lag," to speak in the sociological language of W. F. Ogburn, between the two aspects of the cognitive situation.

We usually give the name of *wisdom* to the deeper and broader type of knowledge and the name of *information* to the detailed and particular type. Now, how can we characterize the man of wisdom? Vergilius Ferm aptly states: "A person is said to have wisdom when he is able successfully to peer beneath or beyond the superficial and seemingly self-evident, to see subtle relationships, and when he knows how to co-ordinate seemingly isolated items and view them in their true perspective and to bring this insight to bear upon the problems and tasks which confront him. A man may have much knowledge and not be wise, however, he could hardly be wise without adequate knowledge. Wisdom is more than information; it is information crowned with understanding."[6]

From the standpoint of our intellectual heritage, science is the discipline whose aim is to give us information and philosophy is the discipline whose aim is to give us wisdom. Science is the search for organized and tested information about the *parts* of the universe in which we live. Philosophy is the search for the best wisdom about the whole universe and man's place in it. Wisdom, by definition, is not *less* than but *more* than information, and "touches *all* the scientific interests and *all* experience."

Philosophy literally (from the Greek) means: the love of wisdom. Several years ago I published a paper[7] where I attempted to develop the far-reaching implications of the etymological definition of philosophy, which is immortalized in Diotima's Tale of Plato's *Symposium,* and suggested therefrom the difference between philosophic knowledge or wisdom and scientific knowledge or information. This difference in *type* of knowledge implies a methodological dualism or a difference in *method.* To acquire scientific knowledge we require scientific method; to acquire philosophic knowledge we require philosophic method. Since wisdom is a knowledge with two dimensions, breadth and depth, we need insight or *vision* for its broader dimension and reflection or *understanding* for its deeper dimension. A two-dimensional type of knowledge necessitates a two-fold approach for its attainment, one for each of the dimensions. Vision and understanding are to philosophic method what observation and proof are to scientific method. As science proceeds, in John Dewey's termin-

ology, by "analytic observation," so philosophy proceeds by reflective vision.

Leake practically agrees with the logical positivists that philosophical propositions or propositions about wisdom are nonsense (he is a bit kinder in accusing them of "metaphysical irrelevance"), since they can neither be confirmed nor refuted by the strict and restricted canons of science. However, this negativistic claim in itself does not prove that they cannot be tested *by other means* nor does failure to prove mean necessarily disproof. If we make a blanket assumption from the start that philosophical theories are not verifiable at all, we are committing ourselves unduly to an intellectual defeatism which is not warranted by the actual and possible facts of the case. For just the verification of a *single* philosophical theory would be needed to destroy the validity of that blanket assumption. And if this possibility were ever to occur in actuality, it would be an unfair change of names to contend that, *before* the test of verifiability that theory was philosophical and that *after* the test it became scientific. After all, we should not put all our eggs in one basket and leave the poor philosopher "holding the bag."

Moreover, the evident fact that philosophical propositions are, on account of their more comprehensive and fundamental nature, much more difficult to verify than scientific propositions should not serve as an obstacle to putting them to as many tests as we can possibly muster. Since the organism of knowledge implies a *uni*verse or one world, there is no *a priori* reason to believe that the methods we find so successful in explaining the parts of this world, cannot be applied with some degree of success at least to the explanation of the whole. In a monistic world, the parts must reflect the whole to which they belong, like Tennyson's "flower in the crannied wall," and the whole must in turn be reflected in its parts. Hence this parts-whole connection in *reality* makes possible a legitimate transfer of *method*.

The possibility of the transfer of the canons of science to the field of philosophy suggests that the various scientific tests at our disposal can and should be employed to eliminate the latter's unfit hypotheses and thus indirectly pave the way for establishing the survival of the fittest ones. This eliminative function of the different scientific tests in the realm of philosophy, though negative in character, is highly important because, by rejecting the false alternatives to any philosophical issue, we have solved at least one half of the problem of attaining wisdom. The final and positive test for determining the validity of philosophical propositions or theories, after the rigid preliminary tests of science have performed their function of elimination as effectively as possible, is or should be the universal experience of the ages and the sages which is embodied in man's deepest understanding and broadest vision. Such ultimate test is the *something more* which science as such can in no wise furnish.

Philosophy and science, in being concerned with two different aspects of the cognitive situation, are mutually supplementary rather than logically incompatible, just as an organism as a whole and its particular organs are not necessarily in conflict. The fact that philosophy and science differ in content and method does not make them mutually incompatible, just as the fact that man and woman differ in sex does not make them incompatible with each other. This is the logic of the situation. Now the historical fact that some philosophers and scientists have been at war, as is so poignantly manifest in the article under consideration, means simply that they have not appreciated the powers and limitations of their distinct business. Philosophers and scientists should not be at war, especially since their common spirit, the critical attitude, and their common logic to which they make constant appeal, are by their very nature *self-correcting*. All of which history reveals the comical side of the human drama of knowledge. The fault, of course, lies not with philosophy and science as such, but with some mentally myopic philosophers and scientists.

Philosophy needs as little justification as science. They are both human business in this precarious life of ours which demands some kind of intellectual adjustment to a baffling environment of problems, old and

new. "Metaphysics," in Montague's beautiful words, "whether a good or a bad thing, is at least a necessary thing in the sense that the urge to it is irresistible. Accepting humbly and gratefully from the sciences their verified discoveries of the truth of this and this and that and that, we must still ask, *What is it all about?* What is the nature of things, and where do we come in? To most of the people some of the time and to some of the people most of the time, such queries make an appeal that will not down. And not until the noble capacity for *wonder* has deserted the human soul will metaphysics cease. And after all, what is there to be afraid of? A metaphysician has nothing to lose but his claims, and a world to gain—a world in which there is the certainty of that solemn joy which comes only from meeting and grappling face to face and hand to hand with the greatest problems of existence; a world, furthermore, in which there is always the possibility of extracting from the products of creative imagination some nuggets of deep and precious truth."[8]

In accepting Hugh Miller's funeral pronouncement on the status of metaphysics, Leake is consciously or unconsciously committing the generalization fallacy. To argue that *all* metaphysics is dead because *some* metaphysical systems are dead is like arguing that *all* physics is dead because *some* physical systems are dead. A thing perennial as metaphysics may be pronounced dead prematurely, but it certainly does not stay dead. Whether we like it or not, metaphysics will out, even in the minds of those who without serious reflection pronounce it a "corpse." Philosophical systems may come and go, but philosophizing as an intellectual activity will continue so long as man retains his sense of wonder about the whole scheme of things. And as long as man is faced with those fundamental problems which affect his weal and woe, his thirst for ethical wisdom will never be quenched.

In the light of the above considerations, we wish to bring to a close our critique of the ultimate phase of the author's methodological argument for a scientific ethic by affirming that "the answer to the question of the insufficiency of science" is not only, as he over-confidently believes, "more science," but also *more wisdom,* the aim of all philosophy worthy of that name. For scientific information, no matter how complete and certain, is no real substitute for philosophic wisdom, no matter how incomplete and uncertain. Wisdom is irreplaceable. As is unfortunately confirmed by our daily experience, more science does not necessarily bring more wisdom. We certainly have more experimentation and gadgets at our control than the ancient Greeks ever dreamed of, but are we wiser? Man does not and shall not live by science *alone.* To be sure, we need more science, but we need more wisdom too, in order to live *significantly.*

To avoid possible misunderstanding of our position, we must bear in mind that we have not really attacked the author's faith in science as a noble pursuit of truth. *De scientiā non disputandum.* What we have in essence found fault with is not so much his *science* but his *scientism,* the "ism" which makes science the all-sufficient tool of knowledge and regards all else as "metaphysical irrelevance." In other words, we have criticized what he *denies,* not so much what he *affirms* with respect to the scientific position and its implications. Scientism, a new name for the old positivistic way of thinking popularized in the last century, is both an inadequate philosophy of science and an incomplete philosophy of values. In short, our objections have been more to Leake as scient-*ist* than to Leake as *scientist.*

Critique of the Darwinian Argument

We would like to begin our commentary on Leake's Darwinian argument for a scientific ethic by stating that we fully agree with his general thesis that a system of ethics to be sound and relevant to human conduct must have a *naturalistic* foundation. Nevertheless, this agreement does not commit us to his Darwinian brand of naturalism. As I have defended elsewhere,[9] a thoroughgoing naturalistic philosophy does not lead to the popular conclusion usually drawn from it and implicitly expressed in the article under consideration. The incorporation of man into nature, to be sure, naturalizes man, but this very incorporation also humanizes na-

ture. Leake quotes in agreement with Darwin that the difference in mind between man and the higher animals in the scale of nature is "one of degree and not of kind." (246) His acceptance of the Darwinian statement indicates that he is missing the whole point, which is that this difference of *degree* makes all the difference in the world between man and the rest of nature. The continuity of man with nature, his home, means not only the alliance of ethics with physics and biology, as John Dewey insists (250), but also its alliance with metaphysics and theology. For how can we envisage an ideal of human life without the illumination of a philosophy and the inspiration of a religion? All the talk about the atoms and the monkeys, however enlightening and quickening, is certainly not enough to guide mankind. In short, as F. J. E. Woodbridge observed, a critical and a discriminating naturalism demands not only an "emancipation from a traditional conception of man," but also an "emancipation from a traditional conception of nature."[10]

Moreover, to formulate a "naturally operative ethical principle," which is Leake's aim, "independently of metaphysical implications or considerations" (252), which is his claim, is logically impossible. Any naturalistic interpretation of man's place in the universe, whether valid or not, is as metaphysical in scope as anything can be, since it involves the general question of man's relation to the universe as a whole. The author seems to be so afraid of "metaphysics" that he prefers to call it by another name, "science." He appears to be a victim of the nominalistic fallacy in the literal sense, and what he impatiently kicks out of the front door of his mind surreptitiously slips in from the back door.

It is appropriate to recall at this point that Leake is engaged in a "deliberate attempt to establish an ethic on the basis of scientific knowledge and in scientific terms." (253) In other words, what he proposes to give us is a scientific ethic. Now, since every *giver* of knowledge has a *given,* let us examine briefly what is assumed in his analysis.

Two interconnected assumptions, one major and one minor, are made throughout the article under consideration with respect to the formulation of an ethical principle. In the first place, the author assumes that the "scientific knowledge" relevant to ethics is "biological knowledge" (248) and, in the second place, he assumes that the "biological basis for ethics" is a "corollary of the Darwinian principle of evolution." (252)

Both of these assumptions are subject to the internal criticism of science itself, not to speak until later of the external criticism of philosophy. On the one hand, the major assumption will not be acceptable to that group of non-biologically minded scientists who would prefer to establish a scientific ethic on the basis of physical or chemical knowledge or on their combination rather than on the basis of "biological knowledge," and would claim for their argument that, if one is going to "approach the problem of ethics in a scientific manner" (245), one might as well go from the biological level down to the bottom of things. This first group of critics constitutes the "fundamentalists" of science. And on the other hand, the minor assumption will not be acceptable to that growing group of non-Darwinian biologists who would prefer to establish a scientific ethic on the basis of a non-Darwinian principle of evolution. This second group of critics constitutes the "protestants" of contemporary biological theory, since the mechanists still predominate in biology. The present disagreement among the biologists themselves on whether Darwinism is a complete explanation of evolution, J. B. Pratt warned,[11] should caution us against accepting it as gospel.

What is most surprising in the article under consideration is that Leake as a present-day physiologist should accept the nineteenth-century Darwinian interpretation of evolution without taking into account the various modifications it has been obliged to undergo under the impact of recent findings in the field of biology. Such omission tends to over-simplify evolution as a law of biology, with the fatal result that a ready-made and sterile formula to fit all the cases is substituted for a flexible and fruitful search to discover its empirical evidence. And the fact that "evolution" has been in vogue is

no prior guarantee of its scientific infallibility.

Of course, any appraisal that may be submitted of the *given* in Leake's ethical system does not impugn at all his right of making assumptions on which his conclusions depend. No thinker can work without presuppositions. However, this logical right of the free thinker involves the corresponding duty of making the most valid assumptions. In order to prove that Leake's Darwinian assumption is not the most valid one for ethics, let us now state his argument and consider its moral consequences.

The author's Darwinian argument for a biological ethic is that, since any living thing to survive must adapt itself harmoniously to its environment, the "good" for human life is precisely whatever promotes such "harmonious adaptation." (250) In the words of S. J. Holmes: "Morality becomes just one phase of the adjustment of the organism to its conditions of existence." (250) On this view called the "harmony theory" (248) of ethics, the moral enterprise is simply one of discovering those human relationships which are "mutually satisfying" (252) to people and consequently have "survival" value. "Adaptation toward the goal of mutual satisfaction" is recommended as the basis of an effective system of "biological engineering." (250)

The "naturally operative ethical principle" is stated in terms of a concomitant variation of "survival" and "mutual satisfaction," as follows: *"The probability of survival of a relationship between individual humans or groups of humans increases with the extent to which that relationship is mutually satisfying."* (251–2) This "proposed statement," which is claimed to be "derived from objective and agreed upon biological evidence" (253), is admittedly a "corollary" or "special case" of the more general Darwinian principle of evolution: "The probability of survival of individual, or groups of, living things increases with the degree with which they harmoniously adjust themselves to each other and their environment." (252)

Before examining the moral implications of the Leakian outlook, it may be questioned whether its ethical principle is a legitimate "corollary" of the Darwinian theory of evolution. If evolution is conceived strictly in Darwinian terms as a "struggle for existence," then it follows that the ethical principle most compatible with it is not the "Golden Rule" of Jesus and Buddha, which seems "to have been devised in appreciation of the naturally operative ethical principle" (252–3) defended by Leake, but what we might christen the "Iron Rule" of Nietzsche and his followers. Leake is too much of a Spencerian to draw the right conclusion from his Darwinian premises. His compromising type of morals is too tame to be consonant with the Darwinian picture of Nature "red with tooth and claw." Hence we contend that the "Iron Rule" of the "Superman" theory rather than the "Golden Rule" of the "harmony theory," is more in keeping with what the Darwinian Huxley[12] of the last century figuratively called "the gladiatorial theory of existence."

Now if we do not accept the ruthlessness of the "Iron Rule" on moral grounds, then we should not logically accept the Darwinian principle of evolution in its original form on metaphysical grounds. We cannot have our cake and eat it. Herein lies precisely the dilemma of Darwinian ethics: Nietzsche's "Iron Rule" is *immoral* and Leake's "Golden Rule" is *incompatible* with its metaphysical foundation. If "the ape and tiger methods of the struggle for existence are not reconcilable with sound ethical principles," as Huxley[13] vigorously charged, then we must do what he failed to accomplish, namely, find a better explanation of evolution which would fit *all* the facts of life and would make intelligible the difference between the life of man and the rest of living things. And in order to achieve a sound conception of nature and man, it is not enough to argue with Spencer that, if the ethical process is "part and parcel" of the cosmic process, then the two cannot be put in opposition, because we must still explain their evident opposition within the single continuum of evolution.

However, it is at least to Huxley's credit that he saw the tremendous incompatibility between the ground and consequence of the

ethics of evolution from a Darwinian standpoint, even though his Promethean defiance of Nature was evidence of his moral sensibility rather than proof of his ability to solve the problem on the intellectual plane. In any case, no matter what form the ethics of evolution may take, the statement of its principle can hardly be "emotionally neutral" (253), as Leake claims for his own case. Neutrality is no more possible or even desirable in ethics than it is in politics, where vital issues are often at stake. In such delicate matters we have to choose sides, and it is to be hoped that all available knowledge be our guide in choosing the better side, if we would live like rational animals.

There are three principal and closely connected moral consequences to the author's Darwinian thesis concerning a "biological basis for ethics." (251) They are (1) ethical relativism, (2) ethical egoism, and (3) ethical practicalism. What we propose to do in what follows is to indicate the weaknesses of each of the foregoing doctrines. Let us examine them in the order mentioned with primarily this end in view.

Leake prefaces his conclusion that our moral categories are "relative" by agreeing with C. H. Waddington's discussion of four anti-intellectualistic tendencies which have contributed to the prevailing and highly fashionable belief in relativism. These "four lines of thought" (249) are psychoanalysis, comparative anthropology, Marxism, and logical positivism. In spite of his uncritical acceptance of these four "attacks on the intellectual validity of ethics," Waddington has faith in the possibility of a science of ethics based on "the course of evolution," since he "feels that ethics is based on facts of the kind with which science deals." (249) Leake "feels" likewise but supplies no proof. And what is worse, both men fail to realize that the logical outcome of the fourfold attack robs not only "ethical statements of any claims to intellectual validity" (249), but eventually robs all scientific statements of such claims as well. For the logic of relativism inherent in the four attacks, which have penetrated deeply into our contemporary climate of opinion, is more fatal to the intellectual validity of science than to that of ethics. To argue with the relativist that ethical beliefs "have no general validity" (249) is bad enough as a guide to morals, but to argue that scientific beliefs have no general validity is even worse as a guide to science. Of course, Leake is too much of a good scientist to be an extreme epistemological relativist, even though he ambiguously identifies "the scientific and relativist approach" (245) at the beginning of his paper and later says that "truth" is "relative" in the sense that it is continually "subject to revision," adding that "goodness" is also "relative" in the same sense. (251) In spite of the fact that his terminology at times is misleading, he does not defend the relativist conception of truth when he affirms: "What we mean by the truth is the opinion *agreed to by all who investigate,* and it is the object represented in this opinion that is the real." (247) However, if we are not mistaken, he does not seem to realize the chaotic and defeatist implications for both science and morals of the four anti-intellectualistic or relativistic "lines of thought" with which he sympathizes.

The irony of it all is that he "feels" protected from the cancerous growth of relativism in our midst today by holding fast to the Darwinian theory of evolution, when it is precisely the extension of that theory to knowledge and conduct which has been partly responsible at least for such growth. Historically speaking, the tragedy of Darwinism is that it has contained the seeds of its own destruction because it has not only given birth to a legitimate child, the science of biology, but also to an illegitimate one, the "Frankenstein" of relativism. In other words, Darwinism as biological relativism is sound in so far as we possess a sufficient amount of empirical evidence on the evolutionary character of the realm of nature, but it is unsound when it is extended to the realm of logical and moral meaning and is transformed respectively into epistemological and ethical relativism. In their half-ardent and half-cynical misapplication of evolution from the realm of existence, where the evolutionary process properly belongs, to the realm of essence, where it does not, the relativists hopelessly confuse not only the variant

things of nature with the *invariant* relations of logic, but also the changing content of moral rules with the relatively permanent form of moral principles.

Since we have just indicated the weakness of relativism, let us pause for a moment to consider its strength.

The general value of relativism, whether applied to knowledge or conduct, is a *negative* one. Epistemological relativism is a good antidote to dogmatism and authoritarianism in matters of opinion. As we had occasion to point out in the first part of our paper, the strength of moral relativism lies in its acute awareness of the variable character of human creeds and codes, and thus constitutes a sound reaction to the inconsistent claims of moral absolutism. This experiential fact of the variations and inconsistencies of our moral judgments is what makes the debate of "absolute or relative criteria" for "goodness" and "right" so acute and not "irrelevant," as the author summarily dismisses the issue (251), and hence calls for a reasonable way out. What reconciliation of this old but crucial debate is possible, is out of place in our rejoinder.

We turn now to an appraisal of the second of the moral consequences to Leake's Darwinian argument for a "biological basis for ethics," which we previously called ethical egoism.

Leake states: "From a consideration of our biological knowledge, the implication is clear at once that survival for an individual living thing or for a particular living species, is 'good' for that individual or that species." (248) This statement is an expression of biological egoism, the doctrine that self-preservation is the first law of nature.

Such conception of self-preservation looks self-evident but is really misleading when applied to human beings. M. R. Cohen remarks: "Men generally have a positive preference or urge to live and want to postpone the pain of death. But they also want certain things for which they willingly shorten their lives by hard work, risks, etc."[14] This means, from the positive side, that mere "survival" is not so "good" if it holds no promise in store and, in fact, could get very boring, and from the negative side, it means that "non-survival" is not so "bad" if it makes for achievement and significance. From the foregoing it follows that survival and non-survival, strictly speaking, are *morally neutral,* meaning that biological categories as such are irrelevant to ethical appraisal. Thomas H. Huxley appreciated the difference between the biological and moral set of categories when he pointed to the fallacy of ambiguity in the Darwinian phrase "survival of the fittest," about which "hangs a moral flavour."[15]

Biological egoism leads naturally to ethical egoism, the view that self-interest is the first law of morals. This view on examination suffers from a tautology. If we admit that "survival" or self-preservation is "good" for the human organism, the question is still left open as to what *kind* of "self" is to be preserved. Montague clarifies this issue in the following way: "Let us suppose that we have each of us decided that the highest moral good means *my* highest self-interest; what kind of self shall I become? Shall I make my ego the sort of ego which finds its attainment of power or life-fulfillment in narrow or physical happiness, or in broad spiritual happiness? In domination over others, or in cooperation with others? Admitting that the pig at the trough and the martyr at the stake are each actuated by self-interest, which of these self-interests shall I prefer and strive to attain? It is no answer to say, 'Whichever is to my greatest interest,' for that is like saying, 'It is to my interest to seek what is to my interest.' What is required is a criterion or principle for deciding what kind of self-interest is the best."[16]

The above logical difficulty would still hold if we were to shift from "personal" to "social satisfaction." (250) The change would be quantitative and would not thereby affect the qualitative problem underlying all hedonism, whether egoistic or altruistic, whether of a single individual or of a whole group. In the latter case we would have to decide what kind of group-interest or what kind of "social satisfaction" is the best, independently of its "survival" value. Furthermore, the fact that a human relationship, like the

husband-wife partnership, is "mutually satisfying" (252) to the parties involved, is no final guarantee of its moral worth, no matter how high its "survival" value. Once more we would be obliged to determine what kind of "mutual satisfaction" is the best.

And finally, let us consider the third of the moral consequences to the author's Darwinian argument in behalf of a "biological basis for ethics," namely, ethical practicalism.

Any appraisal of the "adaptive factors" (248) of moral life in particular necessarily involves an evaluation of the Darwinian conception of life in general.

We believe that Montague presents a valid criticism of this view in his essay entitled "A Materialistic Theory of Emergent Evolution," in which he holds: "It seems to me that it is a great mistake to measure vital excellence by degree of adjustment to environment. If life be at all as we have described it, its business is not to adjust itself to the environment, but to adjust the environment to it, to impose its pattern upon its surroundings and increasingly inform them. It is the inorganic systems that tend toward conformity and orthodoxy and approximate in their increase of entropy more and more to undifferentiation and equilibrium, thermodynamic or otherwise, with their environment. The deader a thing is, the more stable its adaptation to its *milieu.* A block of granite, a diatom, a clam, an ape, a Socrates, embody in increasing measure an aggressive and rebellious power to impose their retrospective and impliedly prospective patterns upon a neutral or more or less hostile world. This invasive, insurgent, and heterodox temper of life does not, of course, preclude- on the contrary, it necessitates- a certain modicum of adaptation. Life must stoop to conquer; but unending conquest, not conformity, is its goal."[17]

If Montague's position is true of "vital excellence," it is even more true of moral excellence. Moral life as *moral,* by definition and observation, cannot be measured by its degree of adjustment to environment, but rather is or should be measured the other way around. And the fact that moral life as *life* requires a certain amount of adaptation to environment is no reason for making a virtue, as Leake insists on doing, out of the necessity of our organic predicament. To be sure, we need *natural* life to achieve the *good life,* but to this truism we are tempted to reply with a "so what"?

Ethical practicalism, the doctrine defended by Leake and other Darwinists that "our moral systems represent ways by which we adapt ourselves to our environment" (249), is in reality the ethical correlate of biological practicalism, the doctrine that moral customs originated as useful instruments of adaptation to environment and therefore, as S. J. Holmes holds, "one potent reason for their adoption is their conduciveness of survival." (250) However, in spite of the important contribution of biological practicalism to "ethicogenesis," ethical practicalism suffers from the genetic fallacy in evaluating moral systems in terms of their historical origins as ways of coping with the environment. The common origin of moral codes does not determine their validity. And the fact that human beings began to be good in order to live is no reason why they should not later be willing to die in order to be good.

Leake's so-called "harmony theory" of ethics is anxious to preserve the "balance of nature" by means of a biocentric type of equilibrium, in which the function of morality is to help the whole human species adjust itself harmoniously to its environmental conditions. In view of the various difficulties of his position, we should like to suggest the opposite kind of "harmony theory" in ethics, based on an *anthropocentric* type of equilibrium, in which the function of morality is to help make the environment of fact conform to the will of men so as to achieve the greatest possible good for all. If the "good" life is to mean more than the "practical" life, it is necessary to reverse our usual tactics of coping with the environment and reach a stage of interaction opposite to the biocentric type of adaptation.

One final comment in closing our reply. The author recommends that we extend the "burnt hand example" (251) to the whole field of morality. To be sure, the practical demands of daily life oblige us to obey the commands of our environment and learn

from experience the lesson of "The burnt child dreads the fire." But, we may ask: Is the *burnt hand* example enough for the *good* life? Tell it to Socrates! Tell it to Jesus Christ! Tell it to John Huss! Tell it to Giordano Bruno! And last but not least, tell it to the Marines!

In short, our most serious objection to Leake's "harmony theory" of ethics is not so much that it "will tend eventually to lead to a *status quo*" (253), as he anticipates, but that it lacks a Promethean spark and is not of the stuff out of which our moral dreams and heroes are made. Without this Promethean or heroic spark to illuminate and inspire the dream of a better world on earth, man may very well survive in the flesh at the terrific cost of perishing in the spirit.

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[12] Huxley, T. H. *Evolution and Ethics*, p. 33.
[13] *Ibidem*, p. 7.
[14] Cohen, M. R. *Op. cit.*, p. 432.
[15] Huxley, T. H. *Op. cit.*, p. 32.
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EVOLUTION

In chaos, primeval chaos,
Came a shifting, a stirring,
The making, the forming,
But in darkness, steaming darkness.

Through the void, formless void,
Quivered a breaking, a rending,
A seeking, a searching,
Then flashed light, living light.

Through the marshes, slimy marshes,
Came a creeping, a growing,
A moving, a spreading,
For there was life, teeming life.

In rolling time, boundless time,
With struggling, with changing,
Out-reaching, up-looking,
Forth strides man, creative man.

—Forest C. Dana

NATURAL HISTORY IN THE ARMED FORCES

A RÉSUMÉ OF SOME RECENT LITERATURE, MOSTLY BOTANICAL, OF INTEREST TO SERVICEMEN

By EGBERT H. WALKER
SMITHSONIAN INSTITUTION

THE plants and animals of the regions which our armed forces occupied or expected to occupy in World War II were of much concern to both those planning operations and the personnel who carried them out. Hence, many books and articles concerning them were prepared officially and privately. Some have never been made public, others are available to servicemen only, and still others are available to the public but are scarcely known outside the offices where they were prepared. Because many of these publications could be of greater use to the public and to the servicemen than they are now, the more useful ones are discussed here along with the various trends in interest which brought them about.

The trends in military interest in botany and zoology varied with the changing aspects of the war, but the first concern was for the protection of the men from the dangerous elements in nature, as the poisonous plants, snakes, and other pests of all sorts. Simultaneously, knowledge was sought of the emergency foods that nature could provide for castaways, and the shelter and needed emergency equipment that could be improvised from plant and animal materials. To supply these needs there have been issued several books and literally hundreds of articles and booklets, comprising what is commonly called survival literature. A list of these has been issued under the title *Survival—A Selected Bibliography,*[4] in which are given data on their character and general contents and the issuing and distributing agencies.

Gradually the demand for this type of literature has been supplied. One of the first and most comprehensive in the botanical field was *Emergency Food Plants and Poisonous Plants of the Islands of the Pacific*[21] by E. D. Merrill, director of the Arnold Arboretum of Harvard University. Probably the most widely distributed of these publications was *Survival on Land and Sea,*[12] prepared for the U. S. Navy by the Ethnogeographic Board and the staff of the Smithsonian Institution. Nearly a million copies were printed, but it has only recently been made available to civilian institutions.

Coordinate with this concern about aspects of nature in connection with survival was interest in plants for camouflage to hide military equipment and movements, and in the identification of plant formations, especially from the air, in order to determine the nature of the terrain, a matter of great importance in carrying on military operations. Secret files doubtless contain much information on these subjects, but little has been made public. The Smithsonian Institution published in 1942 *The Natural History of Camouflage*[13] by H. Friedmann, curator of the Division of Birds, U. S. National Museum, in its War Background Series. Here are discussed the ways in which nature uses concealment and the principles which can be applied in warfare.

A little later our military forces became interested in the exploitation of the natural resources of occupied and to-be-occupied areas, and sought information on economic plants which could provide construction and other needed materials. At first almost all such information, when issued, bore the designation "restricted" or even more confining terms, but gradually these limitations to general distribution are being removed and such publications are becoming available. The Bureau of Yards & Docks, Navy Department, issued in 1944 *Native Woods for Construction Purposes in the Western Pacific Region,*[15] by J. H. Kramer, a Yale forester on the staff of the Foreign Economic Administration in Washington. It is a manual containing descriptions and illustrations of important trees and is intended for the use of servicemen engaged in supplying our

forces with construction materials in the Solomon Islands, the Bismarck Archipelago, the Molucca Islands, Celebes, and the Philippine Islands. A second book by the same author, recently issued, is *Native Woods for Construction Purposes in the South China Sea Region,*[16] covering the rest of the East Indies and the countries around the South China Sea.

Much interesting and valuable information is available in *Notes on Forests and Trees of the Central and Southwest Pacific Area*[28] by W. N. Sparhawk, mimeographed by the U. S. Forest Service. A sketch of the forests and principal useful trees of Japan is to be found in *Japan—Forest Resources, Forest Products, Forest Policy,*[29] compiled by the same author and issued in the same way. The Philippines are similarly treated in two as yet restricted publications of the Allied Geographic Section, Southwest Pacific Area: *Special Study of Timber Resources of the Philippine Islands,*[1] and *Vegetation Study of the Philippine Islands.*[2]

Besides this official interest in survival and the natural resources in the war area, there has been much personal interest in the plants and animals on the part of the men and women in the army and navy. Because their deep-seated cravings to know about the things of nature around them could hardly be met officially by the army and navy, scientists in various other organizations have come forward and provided many books, booklets, pamphlets, circulars, and articles on various aspects of the natural history of the Pacific region. The Smithsonian Institution early in the war initiated its well-received series of War Background Studies, a few of which are on natural history subjects. Most of them deal with geography and peoples, with incidental mention of plants and animals. Already noted is one on camouflage in nature. Another is *Poisonous Reptiles of the World: A Wartime Handbook*[9] by Doris M. Cochran, which bridges from the survival literature. The last of the series so far issued is *The Aleutian Islands: Their People and Natural History (with keys for the identification of the birds and plants)*[10] by H. B. Collins, Jr., A. H. Clark, and E. H. Walker. It was designed to interest and inform the servicemen in the Aleutians concerning the wild life of their environment and has already greatly stimulated natural history study and collecting. Recently it has been partly supplemented by the issuance at one of our bases in the Aleutians of a mimeographed booklet *Flowers of Island X*[34] by Lt. G. B. Van Schaack, USNR, an enthusiastic amateur botanist. It contains descriptions of many of the commoner plants based on the author's collections and field notes made to assist in the preparation of the botanical part of the Smithsonian's study on the Aleutians. We can well expect an increasing number of publications by servicemen in various parts of the world, recording their natural history observations. A list of the plants and animals observed by the naturalists in the Army on Kiska Island in the early days of occupation was issued there in mimeographed form by Capt. G. A. Ammann and is now being extensively revised for issuance by the Department of Botany of the University of Michigan. Other notes by servicemen have been published in various periodicals. In the Marianas the military government is reported to be definitely promoting natural history collecting and study as part of the recreational and morale-building program. Such activities are sure to play a part in the programs of our troops in occupied countries.

The U. S. National Museum has issued a series of seven mimeographed circulars on the natural history of the southwest Pacific.[33] The groups treated in these unpretentious circulars are plants, birds, butterflies, mollusks, reptiles, starfishes, and sea urchins and their relatives. Although intended originally to supplement the answers to the many letters received by the Museum asking for information on the flora and fauna of this area, these circulars have filled a real need for general information basic to a pursuit of more detailed information.

Still another valuable series of natural history books and booklets was initiated and sponsored by the American Committee for International Wild Life Protection, commonly called the Pacific World Series. The staff members of the American Museum of Natural History in New York contributed most of the contents of the first book entitled

merely *The Pacific World.*[26] The material was brought together and edited by Fairfield Osborn of the New York Zoological Society. It concerns mostly the geography and peoples with some mention of the plants and animals, containing especially a tabulation of the distribution of the mammals and birds. Others of this series already issued are *Animals of the Pacific World*[8] by T. D. Carter and G. H. H.Tate and *Peoples of the Pacific World*[14] by F. M. Keesing. Others to be issued soon are *Fishes and Shells of the Pacific World*[25] by J. T. Nichols and P. Bartsch, *Insects of the Pacific World*[11] by C. H. Curran, *Plant Life of the Pacific World*[22] by E. D. Merrill, and *Reptiles of the Pacific World*[18] by A. Loveridge. All these have been or are being issued in cloth-bound editions by commercial publishers for the general public and in cheaper paper-bound editions by The Infantry Journal, Washington, for servicemen only.

The birds of the Pacific region have been treated in numerous serial articles, especially in the popular *Audubon Magazine.* Three outstanding books with excellent illustrations have been issued, *Birds of the Southwest Pacific: A Field Guide to the Birds of the Area between Samoa, New Caledonia, and Micronesia*[20] by Ernst Mayr, *Birds of the Central Pacific Ocean; A Popular Account of the Sea-birds and Shore-birds of the Central Area of the Pacific Ocean*[7] by T. M. Blackman, and *Birds of Hawaii*[24] by G. C. Munro. These leave little to be desired in the matter of popular bird books on those areas, so far as available data are concerned.

The fishes have also received treatment. In 1944 in response to the popular need there appeared *Hawaiian Fishes: A Handbook of the Fishes Found Among the Islands of the Central Pacific Ocean*[32] by S. W. Tinker. It has been very popular with the fishermen in our Pacific forces.

These several publications and many others on the natural history of occupied lands were made necessary by the lack of available published works suited to the needs of servicemen. Many scientific treatments of the plants and animals of the Pacific region have been issued in the past century, as well as a very few more popular ones, but most are out of print and not available for distribution. Some reprinting and translating of older works has been done. The Netherlands Information Bureau has duplicated an article revised from the *Handbook of Netherlands East Indies* (1930) entitled *Flora and Fauna of the Netherlands East Indies*[3] and has also duplicated an article by E. C. J. Mohr, *Climate and Soil in the Netherlands Indies,*[23] which is a chapter in his book on soils of equatorial regions. If the need could have been foreseen and the funds had been available at the beginning of the war, many other works could have been profitably reproduced. The Arnold Arboretum has recently published a description of the vegetation of New Guinea entitled *Fragmenta Papuana [Observations of a Naturalist in Netherlands New Guinea]*[17] by H. J. Lam. This is a translation of a series of Dutch articles originally published in 1927–29. It is of special interest to those naturalists of our forces so fortunate as to be stationed in this botanically rich and little-known country.

The collecting of scientific material is another aspect of natural history with which many men in service are concerned. In 1944 the Smithsonian Institution issued *A Field Collector's Manual in Natural History,*[27] which has been widely distributed and used. A group of American botanists have formed an informal organization aimed to encourage Army and Navy personnel to keep up or develop an interest in scientific subjects by collecting natural history specimens. Specialists in various groups are identifying material and corresponding with enthusiastic naturalists, and already some fine collections have been received. Besides the Smithsonian's collector's manual there have been issued by the Department of Botany of the University of Michigan several pamphlets in a series of *Instructions to Naturalists in the Armed Forces for Botanical Work.* Those issued to date are *Seaweeds and Freshwater Algae*[31] by W. R. Taylor (no. 1), *Fungi and Lichens*[19] by E. B. Mains (no. 2), *Mosses and Liverworts*[30] by W. C. Steere (no. 3), and *Wood-destroying Fungi*[6] by D. V. Baxter (no. 8). These appear as Supplements to Company D Newsletter. "Company D" was a group of AMG men in train-

ing at the University of Michigan, who were specially instructed and encouraged to maintain their contact with natural science by engaging in scientific collecting whenever opportunity permits. This program of collecting by servicemen is interinstitutional. Collectors are being invited to send their collections to the Smithsonian Institution, attention of the writer of this article, whence they are being distributed to various specialists for identification. Many botanists and institutions the country over are receiving scientific specimens directly from servicemen, but in some cases the men find specimens can be sent to a government institution more readily than to others. Some men are asking that their collections be held for their own study on their return to civilian life and resumption of their interrupted training. Professor H. H. Bartlett of the University of Michigan, one of the sponsors of this interinstitutional program, has described it in an article *Gathering Scientific Materials—How Alumni Can Help*[5] in the *University of Michigan Official Publication* sent to all alumni in service. A similar article by him will soon appear in *Chronica Botanica.* The writer issued an article in 1944, *Plant Collecting by the Armed Services,*[35] which went to all medical officers in the army, and was republished for "Seabees." Hence, the invitation to collect scientific plant material has reached many men in practically all branches of the service. A private's wife reports that her husband's letters improved greatly in interest when he responded to the invitation to collect plants in New Guinea, and that he is looking forward to a lifetime of botanical exploration.

It is important to keep alive and develop the scientific interests of our young men while they are in service, in order to offset the danger that our country may emerge from this conflagration with world-wide obligations and opportunities for leadership in science, but without the properly qualified men needed to assume leadership in the geographical aspects of botany and zoology. The currently prepared literature mentioned in this article will go a long way toward assisting and maintaining the interest of our potential scientific leaders now in service and will give them essential training in observing and collecting material, which will at the same time increase the collections on which our leadership in natural science must be based.

WORKS MENTIONED AND WHERE THEY MAY BE OBTAINED*

1. Allied Geographical Section, Southwest Pacific Area

Special study of timber resources of the Philippine Islands. 113 pp. illus. map. Sept. 1944.

Mostly concerned with lumbering, but contains general information on forest types and individual trees with a 6-color folded vegetation map. A "card sorting key to the identity, properties and uses" accompanies this work with a supplementary pamphlet of directions. This is a "punch card" method for identifying timbers. This is a restricted publication prepared in Australia and not yet available for distribution.

2. Allied Geographic Section, Southwest Pacific Area

Vegetation study of the Philippine Islands. 120 pp. illus. map. 1944.

Contains much valuable botanical information. The colored map is the same as in the preceding work. Restricted and not yet available for distribution.

3. Anonymous

Flora and fauna of the Netherlands East Indies. 23 pp. 1944.

A general description. A mimeographed revision of part of the *Handbook of Netherlands East Indies* (1930). Issued by the Netherlands Information Bureau, Dupont Circle, Washington, D. C. in their Weekly Survey, no. 28.

4. Arctic, Desert, Tropic Branch of the Army Air Forces Tactical Center

Survival: A selected bibliography. 21 pp. Jan. 1945.

A source of information on the more important publications and their availability. "Distribution restricted to organizations or individuals working in cooperation with the armed forces, hence not generally available to the civilian public." (From an official letter.)

5. Bartlett, H. H.

Gathering scientific materials—how alumni can help. Univ. Mich. Official Publ. 46 (107): 10–15. April, 1945.

A description of the informal serviceman's collecting program. Obtainable without cost from the Bureau of Alumni Relations, U. of Michigan, Ann Arbor, Mich.

6. Baxter, D. V.

The collecting of wood-destroying fungi. Instructions to naturalists in the armed forces for

botanical field work. No. 8. Suppl. Co. D Newsletter, 23 pp. illus. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.
Free to servicemen.

7. **Blackman, Thomas M.**
Birds of the Central Pacific Ocean; a popular account of the sea-birds and shore-birds of the central area of the Pacific Ocean. 70 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.00.

8. **Carter, T. D., and G. H. H. Tate**
Animals of the Pacific world. 227 pp. illus. Macmillan Co., New York. 1945. $3.00.†
A popular account of the principal mammals.

9. **Cochran, Doris M.**
Poisonous reptiles of the world: A wartime handbook. Smithsonian War Background Studies, no. 10, 37 pp. illus. Mar. 1943.
Obtainable from the Smithsonian Institution, Washington 25, D. C. $.25, free to servicemen.

10. **Collins, H. B., Jr., A. H. Clark, and E. H. Walker**
The Aleutian Islands: Their people and natural history (with keys for the identification of the birds and plants). Smithsonian War Background Studies, no. 21, 131 pp. illus. map. Feb. 1945.
Prepared primarily to interest the servicemen in their environment. For availability see preceding entry. $.25, free to servicemen.

11. **Curran, C. H.**
Insects of the Pacific world. 331 pp. illus. Macmillan Co., New York. Not yet available. $3.75?†
A popular account, probably available in October.

12. **Ethnographic Board and Staff of the Smithsonian Institution**
Survival on land and sea. 187 pp. illus. 1943; revised 1944.
An emergency manual with much botanical information, distributed widely to service personnel. Available to servicemen on application to the Division of Naval Intelligence, U. S. Navy, Washington 25, D. C., and in limited quantities to nonprofit institutions, such as colleges, schools, and libraries.

13. **Friedmann, H.**
The natural history of camouflage. Smithsonian Institution War Background Studies, no. 5, 17 pp. illus. 1942.
Concerns concealment in nature and the principles applicable in warfare. Obtainable from the Smithsonian Institution, Washington 25, D. C. $.25, free to servicemen.

14. **Keesing, F. M.**
Peoples of the Pacific world. 144 pp. illus. maps. Macmillan Co., New York. 1945. $3.00.†
A general account with incidental natural history references.

15. **Kramer, J. H.**
Native woods for construction purposes in the western Pacific region. (Preliminary edition, covering the Solomon Islands, Papua, northeast New Guinea, and the Bismarck Archipelago.) 197 pp. illus. map. May, 1944; (Revised edition covering the Solomon Islands, New Guinea Island, the Bismarck Archipelago, the Molucca Islands, Celebes, and the Philippine Islands.) 384 pp. illus. 5 folded maps. Sept. 1944.
A manual for identifying the principal commercial species. Obtainable by or for navy personnel without cost from the Bureau of Yards & Docks, Department of the Navy, Washington 25, D. C. For army personnel write to A. G. O. Publications Division, Executive Office, Room 2-C-966 Pentagon Building, Washington 25, D. C. Attention of Lt. T. Zbin. Civilians can now obtain it from the Superintendent of Public Documents, Government Printing Office, Washington 25, D. C., for $1.50.

16. **Kramer, J. H.**
Native woods for construction purposes in the South China Sea region . . . Burma, Malay Peninsula, Thailand, Sumatra, Java, Borneo, French Indo-China, and southeast China, including Hainan and Formosa. 277 pp. illus. map. 1945.
For nature of contents and availability see preceding entry. Price to civilians $.75.

17. **Lam, H. J.**
Fragmenta Papuana [Observations of a naturalist in Netherlands New Guinea]. Sargentia, no. 5, 196 pp. illus. 1945. $3.00.
Accounts of the author's travels with descriptions of the vegetation, plants, peoples, etc. and a bibliography. An English translation by L. M. Perry from the Dutch of a series of articles published in Dutch serials, 1927–29. Obtainable from the Arnold Arboretum of Harvard University, Jamaica Plain, Mass.

18. **Loveridge, A.**
Reptiles of the Pacific world. 272 pp. illus. Macmillan Co., New York. Not yet available. $3.00?†
A popular account, probably available in October. The Infantry Journal edition, 243 pp. illus., is already available.

19. **Mains, E. B.**
The collecting of fungi and lichens. Instructions to naturalists in the armed forces for botanical field work. No. 2. Suppl. Co. D Newsletter, 8 pp. illus. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1944.
Free to servicemen.

20. **Mayr, Ernst**
Birds of the southwest Pacific; a field guide to the birds of the area between Samoa, New Caledonia, and Micronesia. 316 pp. illus. Macmillan Co., New York. 1945. $3.75.

21. **Merrill, E. D.**
Emergency food plants and poisonous plants of the islands of the Pacific. War Dept., Technical Manual, 10–420, 149 pp. illus. 1943.
Intended as a pocket manual. Contains illustrations and brief descriptions of the principal species with numerous vernacular names. Obtainable

from the Superintendent of Public Documents, Government Printing Office, Washington 25, D. C. $.15.

22. **Merrill, E. D.**
Plant life of the Pacific world. 336 pp. illus. Macmillan Co., New York. Not yet available. $3.50?†

An account of the vegetation and principal species, probably available in October. The Infantry Journal edition is already available.

23. **Mohr, E. C. J.**
The soils of equatorial regions with special reference to the Netherlands East Indies. 766 pp. illus. maps. Edwards Brothers, Ann Arbor, Mich. $7.50.

An English translation by R. L. Pendleton from the Dutch of *De Bodem der Tropen in het Algemeen, en die van Nederlandsch-Indië in het Bijzonder*, duplicated from typescript. The Netherlands Information Bureau, Dupont Circle, Washington, D. C. has reproduced the chapter "Climate and soil in the Netherlands Indies," 7 pp. [1944].

24. **Munro, G. C.**
Birds of Hawaii. 189 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.00.

A popular manual.

25. **Nichols, J. T., and P. Bartsch**
Fishes and shells of the Pacific world. 185 pp. illus. Macmillan Co., New York. Not yet available. $3.00?†

A popular account, probably available in October.

26. **Osborn, Fairfield** (editor)
The Pacific world: Its vast distances, its lands and the life upon them, and its peoples. 164 pp. illus. maps. W. H. Norton Co., New York. $3.00.†

A general account, largely geographical, with incidental references to natural history.

27. **Smithsonian Institution, Staff of**
A field collector's manual in natural history. 118 pp. illus. 1944.

Intended to assist servicemen in collecting animals of all sorts, plants, anthropological materials, fossils, rocks, etc. Obtainable from the Smithsonian Institution, Washington 25, D. C. $.50, free to servicemen.

28. **Sparhawk, W. N.**
Notes on forests and trees of the central and southwest Pacific area. 78 pp. Mimeographed. U. S. Forest Service. [1945].

Brief descriptions of the various island groups, with a bibliography. Obtainable from the author, U. S. Forest Service, U. S. Department of Agriculture, Washington 25, D. C. without cost.

29. **Sparhawk, W. N.**
Japan—forest resources, forest products, forest policy. 89 pp. Mimeographed. U. S. Forest Service. May, 1945.

Contains descriptions of forest regions and the principal trees with much information on forestry. For availability see preceding entry.

30. **Steere, W. C.**
The collecting of mosses and liverworts. Instructions to naturalists in the armed forces for botanical field work. No. 3. Suppl. Co. D Newsletter, 13 pp. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.

Free to servicemen.

31. **Taylor, W. R.**
The collecting of seaweeds and freshwater algae. Instructions to naturalists in the armed forces for botanical field work. No. 1. Suppl. Co. D Newsletter, 17 pp. Dept. of Botany, U. of Mich., Ann Arbor, Mich. 1945.

Free to servicemen.

32. **Tinker, S. W.**
Hawaiian fishes: A handbook of the fishes found among the islands of the central Pacific Ocean. 404 pp. illus. Tongg Publishing Co., Honolulu, Hawaii. 1944. $3.50.

An excellent guide.

33. **[U. S. National Museum, Staff of]**
[Natural history of the southwest Pacific.] 1944.

This is an informal, unnumbered, mimeographed series of circulars with titles as follows:

Birds of the southwest Pacific. 6 pp.
Birds of the western Pacific (Philippines, Formosa, and Ryukyu Islands). 7 pp.
Plants of the southwest Pacific. 9 pp.
Butterflies of the southwest Pacific. 6 pp.
Mollusks of the southwest Pacific. 4 pp.
Reptiles and amphibians of the southwest Pacific Islands. 8 pp.
Some common fishes of Australia and the South Pacific Islands. 7 pp.
Southwest Pacific starfishes, sea urchins and their relatives. 6 pp.

Each is a general account. Available on request to the U. S. National Museum, Washington 25, D. C. without cost.

34. **Van Schaack, USNR, Lt. G. B.**
Flowers of Island X. 38 pp. illus. 1945.

Descriptions and notes intended to supplement the botanical list and keys in "The Aleutian Islands: Their peoples and natural history." (See entry 10.) Published by the Welfare and Recreation Dept. [U. S. Navy?]. Probably not generally available.

35. **Walker, E. H.**
Plant collecting by the armed forces. Bull. Army Med. Dept. 85: 55–56. Dec. 1944; republished in Construction Battalion Activities 2 (3): 2–3. Mar. 1945.

A general account aimed to promote scientific collecting. Reprints obtainable from the author, Smithsonian Institution, Washington 25, D. C. without cost.

* Most of the government publications were intended for official use, hence stocks are limited. However, most of these are generously made available to the public as long as the supply lasts. Mention of restricted items 1, 2, and 4 has been approved by the Bureau of Public Relations, War Department.

† Special editions of these works may be obtained by or for servicemen only from The Infantry Journal, 1115 17th St., N. W., Washington, D. C. for $.25 each.

RELATIVE HEATING REQUIREMENTS FOR AMERICAN HOMES

By STEPHEN S. VISHER
PROFESSOR OF GEOGRAPHY, INDIANA UNIVERSITY

THE recent shortage of fuel has increased the interest in fuel requirements of all people whose homes are heated in the winter. Millions of families have wondered whether enough fuel could be obtained to carry them through the cold weather.

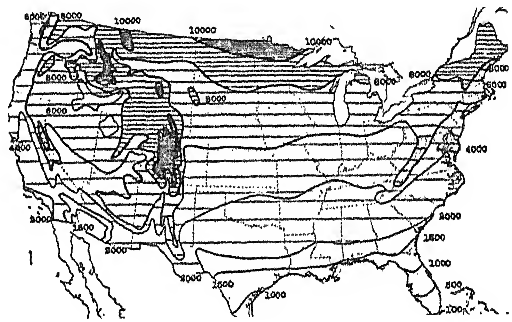
The amount of fuel required for adequate home heating depends, of course, upon several factors beside outdoor temperatures. One of these is the wind. However, since outdoor temperature is a major factor, more adequate information about what temperatures may reasonably be expected is increasingly desired.

Certain data recently released by the U. S. Weather Bureau have permitted the construction of the accompanying original maps. These data were compiled by, or under the direction of, J. B. Kincer, long chief of the Climate and Crop Weather Division of the Weather Bureau, in the few years before his recent retirement. They were made available at his special request in the hope that they would become more widely useful.

These data deal with a special sort of temperature description called a "degree-day," which is defined as a day having an average temperature one degree below 65° F. Thus all days with temperatures of 65° or higher are ignored in this discussion, on the reasonable assumption that house heating is not needed when the average daily temperature is 65°, for on such days the daytime average temperature normally is somewhat above 70° and the nighttime minimum not much below 60°. On days when the average temperature is 55°, ten degree-day units are registered; a day when the average temperature is zero has 65 degree-day units.

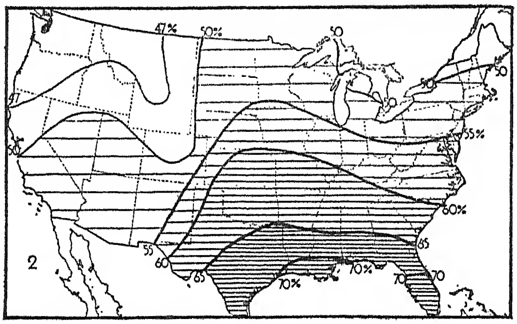
Data on the average number of degree-day units occurring at each of 208 "first order" Weather Bureau Stations by months permitted the construction of Maps 1–5. The variation from year to year in the number of degree-day units is shown, by sections of the country, in the graphs of Figure 1, prepared by Mr. Kincer.

Map 1, of the average annual number of degree-day units, shows that the range is from 100 in extreme southern Florida to 10,000 in northern North Dakota. Thus there is a hundredfold range. In the densely populated part of the country the average number of degree-day units lies mostly between 4,500 and 6,500; for example, it is 4,500 in the District of Columbia, 5,400 in New York City, 5,900 in Boston, 6,800 in Buffalo, 6,200 in Cleveland, 6,300 in Chicago, 4,600 in St. Louis, and 8,000 in Minneapolis. On the Pacific Coast the average is

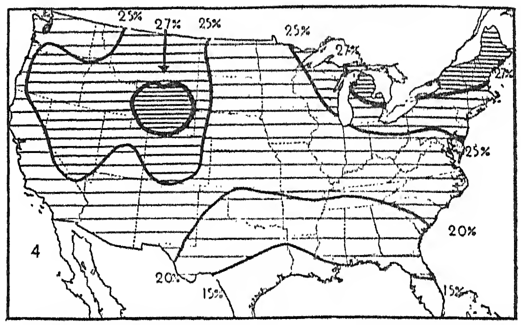


MAP 1. THROUGH THE YEAR
THE AVERAGE ANNUAL NUMBER OF DEGREE-DAY UNITS.

1,400 in Los Angeles, 3,100 in San Francisco, and 4,900 in Seattle. These wide contrasts imply a vast difference in fuel requirements. For example, Philadelphia has more than twice the degree-day units that Birmingham or Atlanta has, Milwaukee has one-eighth more than Chicago, and Cleveland has one-fourth more than Cincinnati. These percentage variations do not mean quite so great a difference in the fuel supplies needed, however, because of differences in insulation of houses, for example, but they reveal an important range in an appreciable item of the cost of living.



MAP 2. IN WINTER
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.



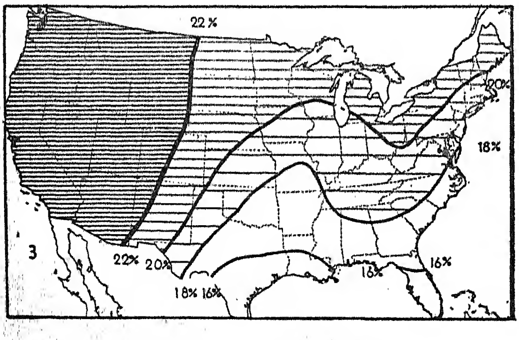
MAP 4. IN SPRING
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.

Map 2 shows the fraction of the degree-day units which occur during the winter (December–February). It reveals that winter has about 70 percent of such units in the Deep South but that the Northwest and extreme Northeast have less than half of their cold weather units in winter. For most of the people of the United States, the three winter months require only slightly more than half of the annual fuel needs. It is interesting to note, for example, that Washington, D. C., Columbus, Ohio, and Chicago are nearly alike in this percentage.

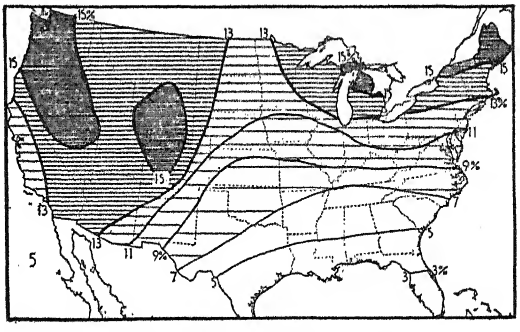
Map 3 shows that autumn is relatively cooler in the West than in the South; that is, in the West the autumn has a larger share of the annual total of degree-day units, between 22 and 24 percent as compared with about 16 percent in the South. The northeastern part of the country has about a fourth more, relatively, than the southeastern. The more rapid cooling off of the western third of the country is associated with its higher elevation above sea level and its greater dryness. In the humid East the considerable contrast between North and South, despite similarity of elevation and humidity, is largely associated with the cooling effects of the snowfall of the North. (The South seldom receives snow in the autumn while in the northern part of the North snow is general in November.)

Map 4 shows that the spring quarter year in much of the northern half of the country has about a quarter of the year's degree-day units, but near the Gulf of Mexico the spring has only about one-sixth of the annual total. The South is warmed sooner in spring than the North, partly because the sun is notably higher in the sky at noon and partly because a snow-cover often persists in the North during the early weeks of spring.

Map 5 shows the percentage of the annual number of degree-day units which occur after March 31. In other words, it shows approximately the fraction of the year's fuel which will be needed to maintain comfortable indoor temperatures after April 1. The variation in the more heavily populated parts of the country is less than might be



MAP 3. IN AUTUMN
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.



MAP 5. AFTER MARCH 31
PERCENTAGE OF THE TOTAL ANNUAL DEGREE-DAY UNITS.

expected; most housekeepers need approximately one-eighth of their house-heating fuel supply after April 1. In Washington, D. C., the percentage is 10, in New York City 12, in Chicago 12, and in Los Angeles and San Francisco also about 12.

Three additional maps, not here printed, may be briefly described. The fraction of the annual total of degree-day units which occur before November 1 range from about 3 percent in the Deep South to 7 to 10 percent in the Northeast and 11 to 13 percent in the Northwest.

Half of the annual total of degree-day units normally have occurred by January 15 in the Deep South, by January 18 in most of the western half of the country, and also in the middle South, but not until January 24 in the Northeast. In other words, midwinter occurs, so far as degree-day units are concerned, on January 13 in Louisiana and New Mexico, on January 15 in Florida, South Carolina, and Arkansas, on January 17 in most of the western states and in Tennessee, and on January 20 in Maryland, Ohio, and northern Illinois. The midwinter date comes latest in New England, Michigan, and New Jersey where January 24 marks the accumulation of 50 percent of the total seasonal degree-day units.

Throughout the southeastern third of the country the summer quarter year normally has no degree-day units. However, about one percent of the annual total of such units occur along a line extending from New Jersey to Iowa and thence southwest to New Mexico. About 3 percent of the year's total occurs in summer in New England, Michigan, and North Dakota, and 4 or 5 percent in the Northwest, from Colorado to Oregon and Washington.

Fuel requirements vary not only from place to place but also from year to year. Although all parts of the United States experience variations in amount of cold, a careful study of the departures from the normal, or average, conditions from 1898 to 1942 shows considerable regional contrast in variability. The variation is least in the Northeast (New England and New York) where only 5 years of the 42 studied departed as much as 12 percent from average, and the greatest departure was only 14 percent. The South Atlantic States, by contrast, had a greater than 15 percent departure from average or normal; two seasons were more than 30 percent colder than normal, and 1930–1931 was 39 percent warmer than normal. Of the 10 regions of the United States studied by J. B. Kincer, the average percentage variation from normal was approximately as follows (estimated from his graphs by years): Northeast 10.0; East Northcentral States 11.8; Northern Interior (North Dakota and adjacent states) 12.3; Central Interior (Kansas, etc.) 12.3; Rocky Mountain States 13.0; North Pacific States 15.3; California 23.4; Middle Atlantic States 12.9; South Atlantic States 28.6; Southcentral States 22.1. These data show that the average range along the southern border of the country is more than twice as great as along the northern border.

The extreme variation experienced in these 42 years was somewhat more than twice as great as the average variation. It was least in the Northeast (26 percent from normal, 14 above, 12 below); it was 30 percent in the Northern Interior, about 32 per cent in the belt that extends from New Jersey to Kansas and from Kentucky to the Great Lakes; on the Pacific Coast the extreme range at the North averaged 41 percent, while in California it averaged 60 percent. The increase in range southward from Virginia and Oklahoma is sharp, an extreme range of 60 percent occurring in South Carolina and central Texas and one of more than 70 percent near the Gulf Coast.

Thus, although the winters of the North average much colder than those of the South, a long-time average reveals that the cold of the North is more regular and predictable than that of the South.

A climatic problem of widespread interest—how our climate is changing—is illuminated by the Kincer data on the departures from the normal of the averages of annual degree-day units. Kincer's studies reveal that the last half of the period 1898–1942 has been warmer than the first half in all regions of the United States. The warming has been greatest in California; next comes the Southeast, despite two exceptionally cold seasons. The Northeast has experienced the least change toward warmer weather.

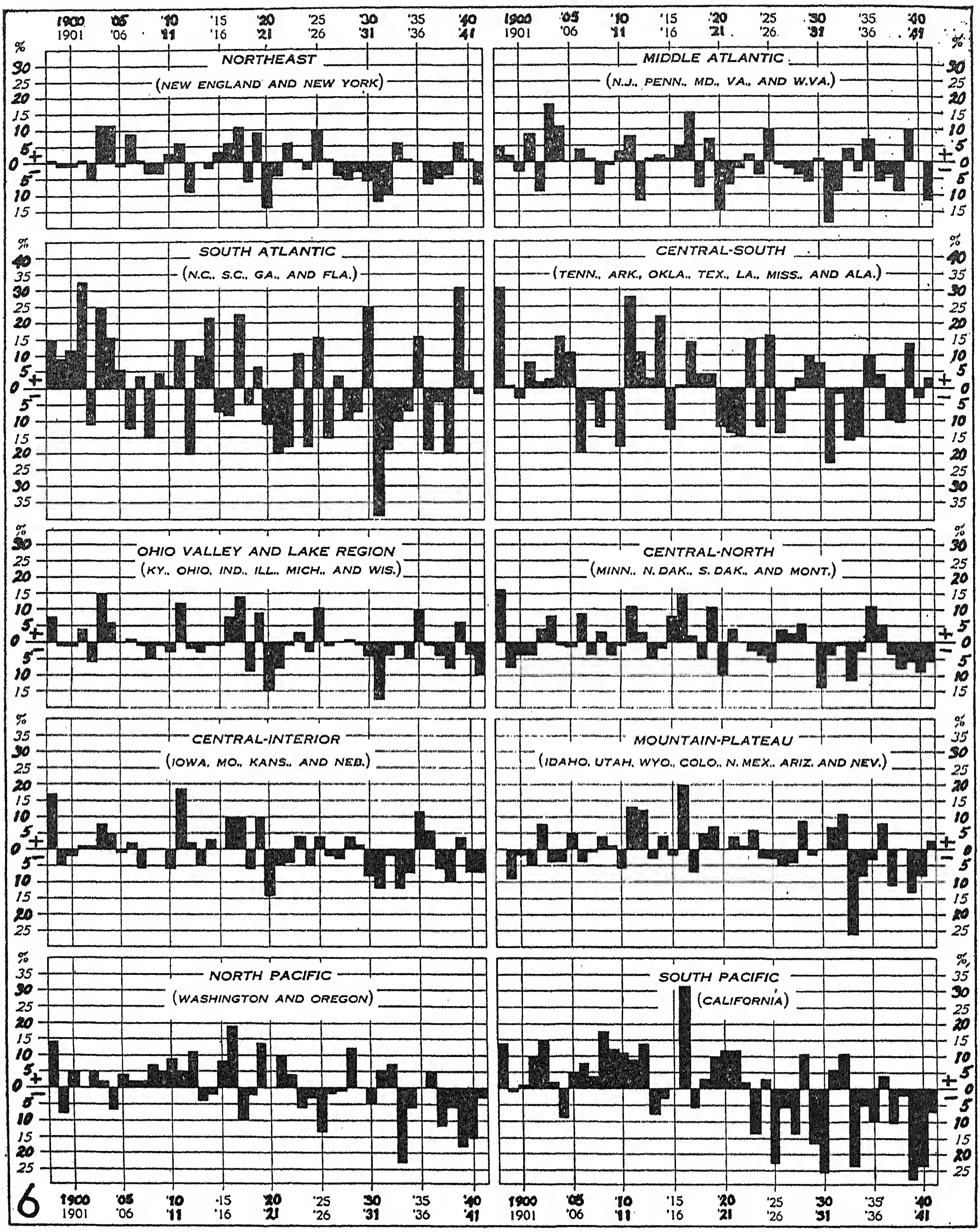


FIG. 1. DEPARTURE FROM NORMAL OF DEGREE-DAY UNITS

BAR GRAPHS BY J. B. KINCER OF THE U. S. WEATHER BUREAU, SHOWING PERCENTAGE DEVIATIONS, 1898–1942.

Figure 1 shows detailed bar graphs for each of the years (centering in winter) 1898–1942 for each of the 10 regions studied by Kincer. The downward bars indicate fewer than the normal number of degree-day units, and hence fewer cool or cold days.

SCIENCE ON THE MARCH

THE BERLIN CONFERENCE

The Report of the Berlin Conference, released on August 2, 1945, is more than a political document. It obviously reaches into every phase of German life and thought, and it affects the Reich's geography, technology, industry, and economy. What is less obvious but no less true is the fact that the document as approved by J. V. Stalin, Harry S. Truman, and C. R. Attlee is a blueprint for the future of central and southern Europe. From this standpoint some of the provisions of especial interest to scientists invite comment.

Provision is made for the control of "all German public or private scientific bodies, research and experimental laboratories, et cetera, connected with economic activities." Scientists will immediately think of German contributions in the fields of medicine, psychology, chemistry, and all the other sciences, pure and applied. They will not forget the commercialization of nitrogen fixation which served both as a boon to agriculture and as a scourge to man when the process was turned to the manufacture of explosives in 1914. They will recall the development of metallic magnesium, which gave industry its lightest metal but likewise burned much of London in the fire raids of 1940 and 1941. They will remember hydrogenation and the correlative processes whereby Germany's lignite was converted into petroleum products and synthetic rubber—basic raw materials for peacetime industry, but at the same time the means of motorizing the German military machine and of bringing the *blitz* to Poland, Denmark, Norway, Holland, Belgium, France, Yugoslavia, Greece, Russia. They may wonder whether the stipulated controls will smother inventive genius, as well as the Prussian penchant for war.

The Conference Report decrees the elimination of Germany's war potential, specifically by restricting the "production of metals, chemicals, machinery and other items . . . to approved postwar needs," and by the removal of "productive capacity not needed for permitted production . . . in accordance with the reparations plan. . . ." It is further stipulated that "the German economy shall be decentralized for the purpose of eliminating the present excessive concentration of economic power;" that "primary emphasis shall be given to the development of agriculture and peaceful domestic industries;" and that "measures shall be taken promptly to enlarge coal production."

The terms of the document prepared at the Conference leave no doubt about responsibility for the holocaust which the Germans brought upon the civilized world in 1939, or about the utter defeat which they have deservedly suffered. But the provisions suggest that preoccupation with Germany's war potential may have led inadvertently to the adoption of measures which, while chaining the criminal, will also cripple his unfortunate victims.

Germany's prewar economy was adjusted to a population of 60,000,000 in Germany proper and of 80,000,000 in "Greater Germany." Despite its northerly latitude, its mediocre to poor glacial soils, and its extensive upland terrain, Germany cultivated intensively every available acre of its 181,000 square miles, leading the world in crop yields per acre. Soils and climate compelled her to concentrate on root crops, of which potatoes and beets are outstanding, and upon the lesser grains—rye, barley, and oats. It will be difficult, and perhaps impossible, to increase agricultural production beyond the point reached by the Germans themselves; yet peak agricultural production was never sufficient to provide either a livelihood or food for the entire population. The best the Council of Foreign Ministers or its agents can hope for in administering the agricultural provisions of the Conference Report is the restoration of German farm production to the maximum achieved in the decade of the thirties.

The Conference Report also stresses the mining of coal, which is the Reich's greatest natural resource. A question inevitably follows: To what use will the coal be put? The Report provides one answer—" peaceful

domestic industries." But such an answer demands analysis. As war broke out, German coal production reached an all-time high of 200,000,000 metric tons of bituminous coal and 230,000,000 metric tons of lignite. Roughly one-third of the bituminous coal came from Silesia, which has been awarded to Poland. Saar tonnage was relatively small, and the quality of Saar coal placed severe restrictions upon its use for industrial purposes. The Ruhr yielded approximately two-thirds of the gross production and a still higher proportion of the high-grade industrial fuel.

In the January issue of THE SCIENTIFIC MONTHLY Chauncy D. Harris pointed out that the coal mines of the Ruhr have supplied continental Europe, except the U. S. S. R., with half of its energy requirements. He concluded that, "barring unforeseen discoveries, the Ruhr district is likely to remain the most important European source of power."

It may be planned to distribute Ruhr coal among Germany's neighbors to rehabilitate industries that were crippled by the military regime and war economy imposed upon these nations by the Germans. But the arbitrary destruction of installed industrial capacity which draws directly on local supplies of high-quality fuel is a dubious expedient, particularly when equivalent capacity is not available elsewhere to produce the iron, steel, and other durable goods which central Europe so sorely needs. Not only does such a policy flout sound economic practice, but it seriously threatens effective economic recovery within the entire central and southern European trading area, with a gross population of nearly 200,000,000.

Germany's lignite, or brown coal, has been skillfully utilized in steam plants for the generation of power and as raw material for the manufacture of oil, rubber, and other synthetic products. The power generated, supplemented by modest supplies of hydroelectric energy, has operated reduction plants for aluminum, magnesium, and other metals, as well as factories for the manufacture of textiles and similar peacetime products. Available power from lignite far exceeds the requirements of the strictly "peaceful domestic industries." The physical properties of lignite are such as to preclude shipment—it must be used at the mines or not at all. Hence curtailment of industrial use means a drastic reduction of output and a substantial net loss in the power potential of central Europe. It has been proposed to generate the power and to transmit the energy to surrounding countries. This proposal, however, blandly ignores the fact that Germany's immediate neighbors, which lie within economic transmission distance, lack the industrial plants to use the power and the skilled labor to apply it even if the plants were built.

The peoples of the United Nations are in no mood to worry about a balanced economy for Germany or about employment for 60,000,000 Germans. But the Report of the Berlin Conference suggests that the interdependence of several national economies is being overlooked. French and Swedish iron ore; French and Yugoslav bauxite; Danubian, Dutch, and Swiss foodstuffs; Baltic lumber and many another raw material were traded for German machinery and for manufactured materials which these countries cannot make from native raw materials with native labor. The industrial economy of continental Europe exclusive of Russia is a delicate web, of which the Ruhr is the only available center. For the sake of the 135,000,000 non-German peoples involved in the web, the center must be preserved, even though the 60,000,000 Germans will necessarily benefit. For this reason, retention of industrial capacity with rigid controls appears to offer a sounder and less hazardous long-range program than the restriction, removal, decentralization, and elimination prescribed at the Berlin Conference.—HOWARD A. MEYERHOFF.

BOOK REVIEWS

A SCIENTIST'S EXPERIENCE IN MEDICAL RESEARCH

The Way of an Investigator. Walter B. Cannon, M.D. 229 pp. 1945. $3.00. W. W. Norton and Company, New York.

COLLEAGUES who have followed Dr. Cannon's outstanding work in medical research, medical education, and in other public service, in peace and in war, for 50 years, will expect something more than a traditional autobiography in this volume. This they will find in most of the chapters of *The Way of an Investigator.* The first chapter gives us a glimpse of Dr. Cannon's ancestry (French-Canadian and Scotch-Irish), his boyhood days in Wisconsin and Minnesota, and his student adventures at Harvard University and the Harvard Medical School 50 years ago. He grew up close to the soil and was conditioned early to the necessity and the dignity of labor. Dr. Cannon may be said to be, through the Harvard Professor of Physiology, Dr. Bowditch, the grandson of the great physiologist, Ludwig, of Leipzig.

The meat of the narrative, the timeless, simple and clear story of Dr. Cannon at work in the research laboratory, in the lecture halls, in scientific assemblies at home and abroad, starts in the second chapter (p. 22) and covers most of the book up to p. 174. Dr. Cannon pioneered in many fields of fundamental biology. The story is clear, even to laymen, and there is little or no bypassing of the author's own errors in experiment or in interpretation. Pointed, and at times humorous, illustrations and quotations from great men of science of the past illuminate and enliven the pages. These chapters should be *must-reading for all medical students* if not all students in science in college and high school. Here Dr. Cannon will be teaching by a great example (I hope) for another 50 years. Not the least of these teachings by example is Dr. Cannon's deeds and words touching our fellow men in other lands, that is, Dr. Cannon's international experience and outlook.

As evidence of the directness, clarity and simplicity of style, the essential impersonality and universality of *The Way of An Investigator,* I quote the opening paragraph of the second chapter (p. 22):

> Investigators do not march straight to their goal with ease and directness. In their imagination they see a possible fact and they set forth to learn whether their foresight can be realized. Or they come upon something which is puzzling and challenging and which they wish to explain; then they try in various ways to relate it to other phenomena that would solve the riddle. Obstacles and difficulties are sure to be encountered. The search for understanding is an adventure or, more commonly, a series of adventures. If an attempt in one direction fails, the failure is not discouraging to an eager explorer. There are other possible approaches to the end in view and relentlessly, one after another, these are tried. When the goal is reached, there is occasion for joy and exultation. A conquest has been achieved. New knowledge has been gained which deeply satisfies both the explorer's adventurous spirit and his persistent curiosity.

Dr. Cannon was President of the American Association for the Advancement of Science in 1939. The Association has (I think wisely) selected *The Way of An Investigator* as one in its nontechnical science series. This volume is not above the comprehension of laymen, and wide reading of Dr. Cannon's story would broaden the base of real understanding of science in our beloved land.

A. J. CARLSON

DEPARTMENT OF PHYSIOLOGY
UNIVERSITY OF CHICAGO

BOTANY AND AGRICULTURE IN LATIN AMERICA

Plants and Plant Science in Latin America. Edited by Frans Verdoorn. 384 pp. Illus. 1945. $6.00. Chronica Botanica Company, Waltham, Mass.

THIS interesting volume is No. 16 of *A New Series of Plant Science Books* being issued by the Chronica Botanica Company. One's first impression is of its comprehensiveness, and he regrets that in this volume, as in its associates, the type is too small to attract the general reader. For, while many chapters are specialized, there is much here that should receive wide attention. Again, the confused sequence of themes will be discouraging to some, but the consultant is directed to the "Complete and Detailed Table of Contents" at the rear which gives a topical analysis of each paper. The sub-

jects considered are so diverse as to defy easy grouping.

Plants and Plant Science in Latin America contains papers prepared by as many as 85 authors. Some dissertations are of general scope geographically, while others are devoted to particular countries. Among the many themes discussed, most pertain to botany and its applied phase, agriculture. Dr. Verdoorn has coordinated the whole, and written a stimulating "Introductory Essay." To it and its message we shall return after attempting to show in more detail what the whole work comprises.

Much of the book recalls the *Naturalist's Guide to the Americas*[1] of nearly twenty years ago. To that some 125 authors contributed, and both works show a resultant inequality in the value of the individual sketches. Only the smaller portion of the *Naturalist's Guide* was devoted to Latin America, and then south only to the equator; *Plants and Plant Science* covers all territory from the southern boundary of the United States and from the West Indies to Cape Horn. The *Naturalist's Guide* considered both plants and animals, but these only as they occur in nature; the later work plants only, but these both in nature and in cultivation. A main feature of both is the often admirable sketches of the vegetation (or, for the earlier work, the biota) of particular countries. These are quite comparable and often supplemental in the two works, sometimes with a fuller and clearer summary in the one and sometimes in the other. (A surprising comment is how rarely these sketches in the later make any reference to those in the earlier work.) But the later work carries much that the earlier lacked.

Such are the series of general sketches, discussing certain themes for Latin America as a whole. Of most value to naturalists will be the "Phytogeographic Sketch of Latin America," and there are summaries of the climatology and mineralogy, of the ethnobotany, of the history of botanical exploration, and of such practical subjects as food aspects and soil conservation.

[1] Naturalist's Guide to the Americas. Edited by Victor E. Shelford. Illus. 761 pp. 1926. Williams and Wilkins Co.

It is in its trend toward applied science that the later differs most strongly from the earlier work. Its first contribution essay is on "Some Problems of Tropical American Agriculture," which is followed (but not immediately) by a comprehensive summary of the "Principal Economic Plants of Tropical America." For many of the countries there are special articles on the "Plant Resources," while not geographically limited are articles on "The Production of Essential Oils in Latin America," "Notes on Cinchona Culture," and an especially detailed account of "Hevea Rubber Culture in Latin America, Problems and Procedures."

The progress of pure science also holds its place in such themes as "Plant Pathology in Latin America," "Paleobotanical Work in Latin America," "Plant Breeding, Genetics and Cytology in Latin America," and "The Location of Botanical Collections from Central and South America."

Topics are far from exhausted, but one must pass from such listing to see what the editor himself has put into the volume. Even though he has taken credit for none of the special discussions within the body of the book, his has been no mean contribution. One senses Dr. Verdoorn's historical interest in the attractive illustrations reproduced from various works of travel, as well as in "A Selected List of Travel Books of Botanical Interest," further bibliographic lists, a list of plant science institutions and societies, etc. His most significant contribution is his introductory essay, entitled "The Plant Scientist in the World's Turmoils." Its message is timely, and gives his reason for undertaking the present volume.

Dr. Verdoorn is a true internationalist. Readers will recall that he commenced his *Chronica Botanica* on such a basis in Holland, and, because of the increasing exigencies there, moved the enterprise before this World War to this country. In the present essay he discusses the growth of nationalism over the World since the first World War and shows how it has superseded the actual move toward internationalism that had been gaining headway in the first decades of this century. He now pleads that science with its broad outlook should aid the latter all-essential movement. The incentive to the

present task has been the desire to further international understanding throughout the Western Hemisphere. Perhaps by such regional understanding and cooperation can the internationalism of the future be best developed. A step lies in such undertakings as the present, for not only does this volume include many countries in its view, but the contributing authors are from various Latin-American as well as Anglo-American countries, and they have written in English, Spanish, Portuguese, and French.

In opening his introductory essay, Dr. Verdoorn quotes from Carl Lumholtz' *Unknown Mexico*, published in 1902, and this reviewer is glad to pass on the selection to his readers now: "It is unnatural to be without a special love of the country of one's birth, just as a man has more affection for his family than for other families. But let our allegiance extend to the whole globe on which we travel through the universe, and let us try to serve mankind rather than our country right or wrong."

F. W. PENNELL

CURATOR OF PLANTS
THE ACADEMY OF NATURAL SCIENCES
OF PHILADELPHIA

MATHEMATICAL METHODICITY

How To Solve It. George Polya. 204 pp. Illus. 1945. $2.50. Princeton University Press, Princeton, N. J.

THE author of this little volume is a distinguished professor of mathematics at Stanford University, formerly dean of the school of mathematics and physics at the Federal Institute of Technology in Zurich. In this book he presents in very simple and detailed form an analysis of steps to be taken in solving problems. The illustrations are mainly from the field of mathematics, and the writer had this field principally in mind, but it is to be noted that the range of applications is far wider—to problems in general.

In brief summary, he states:

First. You have to *understand* the problem.
Second. Find the connection between the data and the unknown. You may be obliged to consider auxiliary problems if an immediate connection cannot be found. You should obtain eventually a *plan* of the solution.
Third. *Carry out* your plan.
Fourth. *Examine* the solution obtained.

He then analyzes in some detail how these steps are to be taken, and how a teacher may unobtrusively aid the student in carrying out the program. Illustrations from the domain of anagrams, puzzles, elementary geometry, algebra, elementary calculus, and number theory, carried out in part as a dialogue between student and teacher, make the analysis concrete.

Most of the book is devoted to Part III, "Short Dictionary of Heuristic," which contains sixty-four articles on alphabetically arranged topics, from "Analogy," "Auxiliary Elements," and so on, to "Working Backwards." To the professional mathematician this presents a lively summary and analysis of methods which he habitually uses in his research work, couched in everyday language. To others there may well be here suggestions and illustrations of methods which are unfamiliar or novel; which should be given careful consideration and tried in the solving of problems that otherwise appear untractable.

Though fundamentally serious, the book is lively, entertaining, and not without humor. We may relish the introductory paragraphs to certain topics in his Dictionary:

> *Rules of discovery.* The first rule of discovery is to have brains and good luck. The second rule of discovery is to sit tight and wait till you get a bright idea.
>
> *Rules of style.* The first rule of style is to have something to say. The second rule of style is to control yourself when, by chance, you have two things to say; say first one, then the other, not both at the same time.
>
> *Rules of teaching.* The first rule of teaching is to know what you are supposed to teach. The second rule of teaching is to know a little more than what you are supposed to teach.
>
> *The traditional mathematics professor* of the popular legend is absentminded. He usually appears in public with a lost umbrella in each hand. He prefers to face the blackboard and to turn his back on the class. He writes *a*, he says *b*, he means *c*; but should be *d*. Some of his sayings are handed down from generation to generation.

"In order to solve this differential equation, you look at it till a solution occurs to you."

"This principle is so perfectly general that no particular application of it is possible."

"Geometry is the art of correct reasoning on incorrect figures."

"My method to overcome a difficulty is to go around it."

"What is the difference between method and device? A method is a device which you use twice."

While the book may properly be called a mathematics book, it is quite clear that it bears little resemblance to textbooks with which the reader is familiar. I recommend it highly to any person who is seriously interested in finding out methods of solving problems, and who does not object to being entertained while he does it.

E. J. MOULTON
DEPARTMENT OF MATHEMATICS
NORTHWESTERN UNIVERSITY

PLANNING A WORLD FOOD SUPPLY

Food for the World. Edited by Theodore W. Schultz. 353 pp. Illus. 1945. $3.50. University of Chicago Press.

DURING the past fifteen years considerable attention has been given to the existing paradox of widespread undernourishment and malnutrition in a world burdened by disturbing regional food surpluses. This problem and its implication for postwar food policies furnished the central theme of discussion at the Twentieth Institute of the Norman Wait Harris Memorial Foundation, held at Chicago, September 4–8, 1944.

Food for the World is a valuable record of the program of that Institute, which was planned to bring to focus upon the subject of public food policy the knowledge and adjudgment of leading students in the fields of nutrition, population, agricultural economics, and international relations. In addition to the 23 essays presented, the book contains an illuminating introduction by Theodore W. Schultz and a report of the interesting informal discussions that followed the six major groups of essays.

The reviewer knows of no other book from which a reader can get so complete and authoritative a summary of the important background factors of the world's food problem as is given here. If, however, one seeks and hopes to find close agreement among the participants as to appropriate national and international food policies, he is likely to be disappointed. Yet, even the discussions on food policy contain a significant core of agreement.

Part I of the book consists of essays by Frank G. Boudreau and John D. Black on the development of the international movement to raise levels of food consumption—a movement that first became important in the mid-thirties.

Part II, by Frank W. Notestein and Frank Lorimer, directs attention to the currently different rates of population growth in various countries and to the changes likely to be witnessed over the next few decades. In the densely settled and undernourished Orient, domestic food production shows no signs of being able to keep pace with the rapidly growing population. In the Western World, on the other hand, declining rates of population growth and continuing technical improvements tend to be reflected in chronic surpluses of farm products.

Part III, on Nutrition, is one of the highlights of the book. Here one finds a valuable survey of present knowledge in the field of nutrition, outlined by such authorities as C. A. Elvehjem, L. A. Maynard, Paul R. Cannon, Ancel Keys, and Lydia J. Roberts. These experts (especially Maynard) put needed emphasis on the unknowns, as well as the knowns, in this relatively new science. And the "government planner" should note that even within the known sector of the field, students of nutrition disagree as to whether "recommended dietary allowances" should be set at levels "only sufficient to prevent clinical or biochemical signs of deficiency" or (like the recommended allowances of the Food and Nutrition Board) at appreciably higher levels that insure "a fair degree of tissue stores."

The three remaining groups of essays—on Food Supplies, International Relations, and Consequences and Policy—are devoted primarily to economic facts and analysis. Among the contributing economists are many

well known for their work in the fields of agricultural economics and international relations: Theodore W. Schultz (Editor), Paul H. Appleby, Percy W. Bidwell, Karl Brandt, Allan G. B. Fisher, E. W. Gaumnitz, Edward S. Mason, Margaret G. Reid, Leroy D. Stinebower, Henry C. Taylor, Howard R. Tolley, Walter W. Wilcox, and P. Lamartine Yates.

Among these authorities there is close agreement in analysis of the world's food problems, but considerably less agreement with regard to proposals for policy solutions. It is therefore noteworthy that a substantial amount of accord is registered on the following important points bearing on food policies:

(1) Some measure of official direction of food production and distribution in the postwar period is inevitable and probably desirable.

(2) Any satisfactory agricultural and nutrition policy must include provision for the maintenance of high levels of national and world employment, for continued increase in economic output per man-hour, and for expansion of international trade.

(3) The pressure of population against food supplies in India, China, and similar areas cannot be relieved quickly and permanently through importation of foodstuffs from abroad. Solution of the food problems of these countries must be based upon improvements in domestic agriculture, industrialization, and mass education—developments that may be expected to raise the socio-economic status of the people and eventually reduce the rate of population growth.

(4) There is no easy solution for the agricultural surplus problems of the United States and other Western countries, though these problems would be minimized in an expanding world economy, under conditions of practically full employment and freer international trade.

(5) The agricultural price policy recently pursued by the United States has protected high-cost farmers, raised prices to consumers, postponed the shifting of agricultural resources from products in chronic surplus to foods needed to raise the nutritional status of the population, and curtailed the flow of American farm products to foreign markets.

These accepted ideas are probably more important than the expressed differences of opinion on policy, which, in any case, are too numerous to outline here.

HELEN C. FARNSWORTH
FOOD RESEARCH INSTITUTE,
STANFORD UNIVERSITY

SCIENCE A LA MODE

Science Today and Tomorrow: 2d Series. Waldemar Kaempffert. 279 pp. 1945. $2.75. The Viking Press, New York.

Science Year Book of 1945. Edited by John D. Ratcliff. 224 pp. 1945. $2.50. Doubleday, Doran & Co., New York.

Careers in Science. Philip Pollack. 222 pp. Illus. 1945. $2.75. E. P. Dutton & Co., New York.

KEEPING abreast of science these days is no child's play, and the science writers who attempt to keep us well informed are to be encouraged. These three books, though not written with identical aims by any means, have this in common: they review current scientific progress for the layman and indicate a few of the opportunities in science and a little of the "shape of things to come."

Mr. Kaempffert's book is the most stimulating of the three. His 24 chapters cut a wide swath, and their appeal lies not only in the factual accounts of recent scientific discoveries and developments but also in the author's speculations for the future. It is easy enough to say that the world of tomorrow will be violently changed by science, but it takes a Kaempffert to visualize these changes, to dare to make rather imaginative but definite predictions, and to render them convincing to the reader who may be inclined to pooh-pooh this H. G. Wells sort of stuff. When we start reading about such possibilities for the future as rocketing-through-space-to-Mars, psychosurgery, teletaction, teleolfaction, telegustation, etc., etc., we instinctively exclaim, "Tell it to Sweeney!" But then, when we finish reading we become apothegmatic. *Today's romance is tomorrow's reality. Only dolts disbelieve in miracles. There are more things in heaven and earth, Horatio. . .*

Mr. Kaempffert believes in the efficacy of science as a world-unifying agency. "Science and technology," he says, "are the most potent influences in the world today," and they are "about the only unifying forces on which all men are agreed." In his final

chapter, "Through Science to World Unity," he pleads for a World Scientific Commission to organize and direct science toward this international unity and to educate the masses in the fundaments of science. We agree with him that the experiment is worth making, for if Science is to become king the people everywhere ought to begin knowing something about their new ruler. "It may take decades, even generations," says Kaempffert. It will, so far as America is concerned, unless our education is raised considerably above the present donald-duck level.

Science Year Book of 1945 is the fourth such anthology to be edited by Mr. Ratcliff. He has aimed to reprint the latest, most exciting, and most readable developments in the fields of medicine, physics and chemistry, aviation, and other sciences. The articles have appeared in such popular magazines as *Reader's Digest*, *Collier's*, *Saturday Evening Post*, and *Hygeia*, among others. Penicillin, blood derivatives, DDT, buzz bombs, electrons in medicine and industry, high-vacuum physics, and industrial research are featured. The selections are conspicuously of uneven scientific value, and it is hard to see how some of them make the grade. Several of the sciences, such as anthropology, botany, and zoology (except for an article on animal migration) are unrepresented, which indicates, I suppose, not that these branches have made no noteworthy progress since 1944 but that writers in these fields have not attained the level of popularization that Mr. Ratcliff's yearbooks require.

Careers in Science is written especially for young people who may be thinking seriously of entering scientific work as a livelihood. It is a book of vocational guidance and is of a series of "career" books. The plan throughout is to try to tell the young prospective scientist what he is getting into, what the opportunities are in his chosen field, what he may expect to earn, the directions that science is taking. Chemistry, physics, biology, and geology seem to offer the best prospects. Although rather indifferently written and although not all the author's facts and figures are strictly up to date (such as the names of and salaries offered by various United States Government agencies), the book contains much good advice and valuable information. One recurrent query is asked the would-be scientist: Have you got what it takes? Have you the price of admission? Besides Mr. Pollack's contribution, which was made in collaboration with Vocational Guidance Research, the book has an introduction by H. C. Madsen, of Westinghouse; a chapter on "The Outlook for the Physicist and Prospective Physicist in Industry," by Dr. Albert W. Hull, of General Electric; and a chapter on "How Can We Develop Inventors?" by Dr. Charles F. Kettering, of General Motors. The volume is abundantly illustrated and is complete with bibliography and index. This book should find its way into all our secondary-school libraries. If compulsory military training for our youth comes, science and technology will be the losers, and it therefore behooves science to look now to her training grounds and garnishee all the latent scientific talent anywhere in sight. This book may help a little.

PAUL H. OEHSER
SMITHSONIAN INSTITUTION,
WASHINGTON, D. C.

CRUSADING MOLDS

Microbial Antagonisms and Antibiotic Substances. Selman A. Waksman. 350 pp. Illus. 1945. \$3.75. The Commonwealth Fund, New York.

THE advent of Professor Waksman's book is a very timely one. It has frequently happened in the history of science that, like a good gold strike in new territory, a comparatively simple observation or discovery has set off a mass trek into new fields. Such has been the situation for the past four years with respect to antibiotics, and it is interesting to look back and try to put one's finger on the "strike" which set it off. The conception of antibiosis is not at all new, Pasteur himself having commented on phenomena of this sort as much as seventy-five years ago. Neither is the idea of using antibiotic substances for therapeutic purposes a novel one, the Germans having experimented with pyocyanase and pyocyanine as much as fifty years ago. Moreover, in comparatively recent times, the discovery of the therapeutic activity of gramicidin did not serve to set off the deluge of antibiotic research with which

we are now confronted, and penicillin had already been known for twelve years without having attracted very much attention. In retrospect, it now appears that the spark which started the conflagration was the demonstration of the almost micraculous therapeutic properties of penicillin in the treatment of certain conditions for which there had previously been no effective remedy. The result has been a tremendous burst of activity to find new antibiotic substances, not only in microorganisms but in the lower and higher plants as well.

The author opens the subject of *Microbial Antagonisms and Antibiotic Substances* with a discussion of the association of mixed populations of microorganisms in water, soil, and composts. Although many of these relationships are symbiotic, those which are of an antibiotic nature have here been singled out for special attention. This provides the entree into the discussion of the general subject of the book, which is introduced with sections on methods for isolating antagonistic microorganisms and the measurement of the activities of their associated antibiotics. Following this there are separate chapters devoted to each of the various groups of organisms known to produce antibiotics; both the chemistry and biological properties of known compounds of this class are discussed insofar as it is possible. Finally, there are chapters dealing with the use of antibiotics in the control of animal and plant diseases.

Microbial Antagonisms and Antibiotic Substances is highly accurate, complete, and up-to-date in every respect, including a bibliography of more than a thousand references. However, no critical evaluation is given for most of these papers, and one cannot help wishing that the author, considering his pre-eminence in the field, had done something more than merely list them and allow the reader to decide for himself as to their relative importance. This book is in no sense a popular treatment of the subject; it is definitely a book for the scientific worker in this and closely allied fields, and as such can be very heartily recommended.

ROBERT D. COGHILL
NORTHERN REGIONAL RESEARCH LABORATORIES,
U. S. DEPARTMENT OF AGRICULTURE

DEEP SEA AND ANTARCTIC EXPLORATION REPORTS

The Genus Ceratium in the Pacific and North Atlantic Oceans. Scientific Results of Cruise VII of the Carnegie. Biology V. Herbert W. Graham and Natalia Bronikovsky. 209 pp. Illus. 1944. Paper bound $2.00; cloth bound $2.50. Carnegie Institute of Washington.

The Scientific Results of Cruise VII of the Carnegie. Oceanography II. *Part I. Marine Bottom Samples Collected in the Pacific Ocean.* Roger R. Revelle. *Part II. Radium Content of Ocean-Bottom Sediments.* Charles S. Piggot. 195 pp. Illus. 1944. Paper bound $2.00; cloth bound $2.50. Carnegie Institute of Washington.

Reports of Scientific Results of the United States Antarctic Service Expedition, 1939–1941. 398 pp. Illus. 1945. $4.00. Proceedings of the American Philosophical Society.

ALTHOUGH expeditions to the open ocean and the Antarctic were among the casualties of the war, publication of their results is being carried through in spite of the difficulties which accompany these unsettled times. To be sure, the last major oceanographic expedition, the seventh cruise of the *Carnegie,* ended with the destruction of that fine ship at Apia in 1929, but is was not until 1942 that publication of the results was begun. Expeditions since that time have not been so extensive and have usually been connected with some particular problem, like the antarctic whale fishery, or restricted to a limited area of the ocean.

Hence it is interesting to see in what ways the latest series of oceanographic reports resembles, and differs from, its predecessors. The Carnegie Reports, whose most recent numbers are a monograph on the genus *Ceratium* and a report on the bottom samples, are published in the large quarto form established by the *Challenger* reports more than sixty years ago, but the printing inside the covers is quite different. Instead of fine large type in long lines across the page, there is a double column of photolithed typewriter text, with jagged right hand margins. Perhaps there are economic advantages in this, and it must be said that the method produces satisfactory illustrations at comparatively low cost, but it is hardly a delight to the eye. Nor is it very restful to read, for the unfinished appearance of the margins exaggerates the blurring which too often occurs in this type of reproduction From the viewpoint of the appearance of the series as a

whole, it is difficult to understand why this method was adopted after the first two reports (on copepods and *Tintinnoina*) had been printed in the more formal and legible type.

However, the important thing is that the results are being published. They reveal the change in emphasis which has occurred in oceanography since the days of the *Challenger* and the *Albatross*. That emphasis is now on precise physical and chemical data, and comparatively little biological collecting, except of plankton, is now done. The *Carnegie*, of course, was not equipped for heavy dredging, and its work represents the extreme departure from the earlier program of oceanography. Nevertheless, the plankton reports of the Carnegie cruise are especially valuable because of the detailed physical data which are correlated with them, data which earlier expeditions could not gather because of cruder instruments. It is not enough to know what animals occur in the ocean, or even where; we must also know when, and in relation to which conditions, and such reports as these on the dinoflagellates and copepods are long steps toward the synthesis of hydrography and biology which is still the primary goal of oceanography. It is with regret, however, that this reviewer records this change in the order of things, for he misses the elaborate faunal monographs which were formerly the bulwark of every series of reports. As for physical data, the Carnegie reports leave little to be desired, and in several instances data from other recent expeditions and surveys have been included. An especially valuable feature of the report on bottom samples is the detailed station list with descriptions of samples from nearby stations occupied by earlier vessels, notably those of the *Challenger* and *Albatross*.

In the past an Antarctic expedition has contributed much to oceanography and marine biology, and those lately completed by the British and colonial governments have been no exception, but the second exploring expedition to be authorized by Congress has little to offer to these fields of knowledge other than a paper on Ross shelf ice and some reports on birds, general descriptions of antarctic biology and miscellaneous identifications of marine invertebrates. Unlike the Wilkes expedition of 100 years ago, which was the other authorized United States exploring expedition, the Byrd expedition was devoted primarily to land exploration and geology instead of oceanic exploration. This symposium volume of the Philosophical Society, which apparently embodies all that is to be published on the expedition, contains several noteworthy papers on antarctic geology and geography, and some detailed meteorological observations. The bulk of this work was done on the Palmer Peninsula, and is further located by a series of names which are evidently based on the Dunn and Bradstreet rating of the sponsors, for Congress, as usual, failed to appropriate sufficient funds. It will be interesting to see what names are finally applied to these various features, since English explorers have their own set of names for Graham Land, as they call this region. However, there is something liquid and musical about such a name as Mobiloil Bay. The volume is illustrated with excellent, well-produced photographs which will remind many of a well-known geographic magazine.

JOEL W. HEDGPETH
GAME, FISH AND OYSTER COMMISSION,
ROCKPORT, TEXAS

CHEMICAL DETECTION

Discovery of the Elements. Mary Elvira Weeks. 578 pp. Fifth ed. Illus. 1945. $4.00. Journal of Chemical Education, Easton, Pa.

THE appearance of a revised fifth edition of Dr. Weeks' well-known *Discovery of the Elements* is sufficient evidence of the growing popularity of this work not only among chemists but also with those who are interested in the general history of science. The first edition of the book, compiled in 1933 from a series of articles by Dr. Weeks in the Journal of Chemical Education, has expanded in the course of a few years from a volume of 363 pages and 875 literature references to the present work of 578 pages and 1599 citations; or, differently stated, from a book of 21 chapters and 282 illustrations to one of 27 chapters and 347 illustrations.

Among the new features of the present edition should be noted the list and page

numbers of the many interesting illustrations, the great majority of which are from the large collection of Professor F. B. Dains of the University of Kansas. Additional biographical sketches have been inserted and new information has been made available, particularly upon the history of developments in the countries of the Western Hemisphere. Each chapter is preceded by a brief summary of its contents and by an appropriate quotation from one of the chemical classics. The anecdotal treatment, employed by Dr. Weeks in the previous editions of her book and followed in the present work, gives much of human interest to the unfolding of the story of how each of the elements was discovered—a story which the discoverer, wherever possible, is allowed to tell in his own words.

The book can be enjoyed as much for random as for continuous perusal and can be dipped into with equal pleasure by the young and old. For the beginner in science, there are few books more suitable for collateral reading than the present work with its fascinating and stimulating accounts of how perseverance, or intuition, or accident, has contributed to the making of some discoveries and of how lack of caution, or overcaution, has prevented some investigators from achieving the fame of discovery. To older readers the book will be a veritable treasure-trove of pleasing scientific information gathered by Dr. Weeks in her diligent search of ancient records. The wealth of literature references at the conclusion of each chapter will be a useful guide to librarians and to those desirous of enriching their collections with the rarer works on the history of chemistry. The typography of the new volume is excellent, the reviewer having noted only the peculiar syllabic transposition of "inNasi" for "Nasini" on page 340.

Because of war-time paper restrictions this new edition of Dr. Weeks' *Discovery of the Elements* is announced to be a limited one. It is hoped that the publisher has allowed for its probable popularity indicated by the sale of previous editions.

C. A. Browne

U. S. Department of Agriculture,
Washington, D. C.

EARTH SCIENCES RE-EXAMINED

Principles of Physical Geology. Arthur Holmes. 640 pp. Illus. 1945. $4.00. The Ronald Press Co., New York.

Elementary textbooks are so characteristically pedestrian and dull that it is a relief to come upon one which maintains a high level of interest for more than 500 pages. Geology is a fascinating subject, but the penchant of most teachers and textbook writers to reduce every aspect of the subject to definition and to routine exposition deadens the interest which normal underclassmen exhibit in college courses that preserve some spontaneity and excitement. Arthur Holmes imparts much of the excitement which the subject of geology evidently arouses in him.

The effectiveness of the volume must be attributed in large part to the unity which has been achieved in the development of the subject. A single author has a distinct advantage over collaborating authors in this respect, both because he can maintain better perspective in preparing the several parts of the book, and because, by use of a recurrent theme, he can hold the parts together and even dramatize them. Holmes has made the most of these advantages and has coupled with them the benefits which accrue from good writing. Furthermore, the student who uses the text is likely to be infected with the idea that there are many intriguing problems still to be solved in the earth sciences and in many parts of the earth.

Starting with a rather long preliminary survey of the earth, the materials composing it, the processes at work upon it, and the geologic modifications which these processes effect in the course of time, the author then gives an efficient account of the external agents, wind, water, ice, and life. Noteworthy are the two chapters devoted to the work of running water, if only because they present geomorphic views which are firmly established in Europe, but which are still considered controversial in this country. In contrast to the excellence of these chapters, the treatment of groundwater is more disappointing than it is in most elementary texts. It is obvious that British geologists have but little more than a reading acquaintance with solution phenomena and the solution cycle.

Holmes' chief interest lies in the operations of the internal geologic processes. The minutiae of structural and igneous forms, to which American geologists are prone to devote an inordinate amount of space, are quickly disposed of in the preliminary survey, and in the part assigned to the internal processes the author plunges into the broad aspects of diastropism and igneous activity. His method involves a survey of the major landforms of all the continents, and even an instructor will lay the book down with a better integrated knowledge of the earth, however much he may disagree with some of the author's hypotheses and conclusions.

It is easy to find fault with such a book. The American geologist will wince at the vague and, at times, erroneous references to American geography, forgetting his own imperfect knowledge of European geography in general and British geography in particular. From the practical standpoint of the classroom teacher, however, a book which draws most of its illustrative material from Europe cannot be used very effectively among American students, who know altogether too little about their own country.

Criticism will undoubtedly center upon the meager space which Holmes devotes to rocks, minerals, and structures; to the brevity with which some other subjects are dealt with; and to the wretched reproduction of the well-chosen photographs. Between them the reviewers have had 35 years' experience in elementary instruction, and they are prepared to defend Holmes in his rejection of certain details which can be more effectively taught in the laboratory than in a text or in the lecture room. Most of the textbooks in current use kill student interest in minerals, rocks, fossils, and structures by setting down interminable definitions and diagrams which, to the student, become so many exercises for laborious memorization. Spontaneous interest in the subject is given only a meager chance of survival beneath the avalanche of deadly detail. For this reason the reviewers are more than willing to applaud the omissions, which are obviously judicious and not inadvertent.

For the American undergraduate who is taking his first course in geology, *Principles of Physical Geology* is not an entirely satisfactory text because it assumes a background of mathematics, physics, and chemistry which too few of our students have. In consequence, some of the chapters in Parts I and III are somewhat beyond the reach of the average freshman and sophomore. For an advanced course in physical geology the volume should prove valuable in providing fresh illustrative material, but as a text in such a course, the volume is rather elementary. It should find most effective use among instructors who wish to freshen their viewpoint and among nongeologic scientists who want a knowledge of the subject. For both groups Holmes' book is required reading.

Howard A. Meyerhoff
Elizabeth W. Olmsted
Department of Geology
Smith College

COMMENTS AND CRITICISMS

REACTIONS TO LEAKE'S "ETHICOGENESIS"

THE effort to bring ethics into an immediate causal alignment with knowledge, to make of reason the "good," has everywhere occupied the minds of profound thinkers. Dr. Leake's views on ethicogenesis, in keeping with this tradition and modified by the convention of the modern era, assume the possibility of such a relationship between science and ethics; assume that man need only learn from science what is "good" for him in order to construct promptly for himself an ethical system out of his knowledge. That this pattern has not been followed, at least down to the present, is indicated by the glaring discrepancy existing between the comparatively high level of human knowledge and the still-predatory level of civilization. There is no dearth of historical evidence to show that knowledge reaches the level of practical and ethical attitudes only after the lapse of considerable time; as much time as is required for that knowledge to filter or buffet its way through the barrier of prevailing practical and ethical attitudes. That is to say, knowledge reaches the ethical level only after it has undergone a process in which opportuneness plays the major role; a process related to the interaction of the material and intellectual factors of an era. On this view, ethics, as a product of many factors of which knowledge is but one, appear to be an accretion upon and a phenomenon relevant to the phenomenon of human society.

Ethics are unique in the biological world and, more than any other single characteristic of man, distinguish him from his evolutionary forbears. If the difference between the behavior of man in his social environment and the behavior of lower organisms in their natural environment is merely a matter of degree, as is often affirmed, it is necessary out of scientific scepticism to determine whether or not this difference in degree is sufficient to produce qualitative differences between them. It is a far cry from the "ethics" of biological symbiosis to the self-abnegation of Judeo-Christian ethics. A teleological view of nature might consider both, as kinds of relationship, to be analogous ethical devices; yet it is impossible to measure the survival value of both by the same meaning of the term survival. In its social context survival is often conditioned so that it may appear to modify or even reverse biological law. Here then is one qualitative factor barring the direct application of biological knowledge to ethics. The *amount* of knowledge possessed by man about himself does not necessarily, by its magnitude alone, total up to that precise knowledge required to eliminate his lethal conflicts. The task of science with ethics is the selection and orientation of that knowledge which will enable it to span the qualitative gap between society and the natural world; to create, so to speak, a medium of exchange between them. To my knowledge, except for the figurative abstractions of philosophy and poetry, no valid biological correlate of the phenomenon of ethics has been established. On the asssumption and evidence that man is subject to biological law, his ethics must necessarily be found to have a place in the biological studies of man. I have no doubt that this is possible, but it seems necessary first to read ethics in terms of their social root-source before the problem of ethics can be made commensurate with the method of science.

Moving directly, by figurative analogy, from biological phenomena to society, Dr. Leake loses sight of the fact that the survival of the individual or of individual groups or of the relationships existing between them is no longer ethically relevant in an environment where the survival of a part is dependent upon the survival of a unit beyond itself and by which it is encompassed. In his social context man is no longer a biological unit, and ethics based upon survival value apply no longer to him directly but to the survival of the greater unit. Dr. Leake's principle of harmony in groups adds a foot to the toes for better function, but it seems to do little for the total organism, society. In our present era there is a guarantee written in anguish and degradation that the ethics of the total social level take precedence over isolated individual or group ethics. The physical unity of the world requires nothing less than a total ethical principle, an ethic which, as a social force, will have the power to invite the efforts of science for the welfare of society and the individuals who are its creators and its product.—ANNE ROSENBERG.

Chauncey D. Leake, in his profound search for a law of Ethics (in THE SCIENTIFIC MONTHLY, April 1945) devotes a few remarks to the origin of the so-called Golden Rule. My training in the Jewish faith taught me that what he calls the Buddhistic form of the Golden Rule actually was the reply of Hillel I (died A.D. 5) to the question of a heathen about the teachings of the Hebrew religion. I always held this answer to be genuine, a belief which is strengthened by the fact that the famous phrasing

of the Golden Rule in Matthew is followed by the explanation: "for this is the law and the prophets." We, therefore, must conclude that both the affirmative and the negative formulation of the Golden Rule, were popular expressions in Palestine at the time of Jesus, and no sharp distinction can be drawn between them, as the author seems to think.

The exhortation: "Thou shalt love thy neighbor as thyself," which is often identified with Christian teachings, is wrongly translated from Leviticus XIX, 18. It implies that self-love is the strongest expression of love conceivable, an idea alien to the ancient mind. What the lawgiver wanted to impress is simply: "Love your neighbor, he is like you." Many modern translators, e.g., Buber and Rosenzweig, have recognized this. The fact that the Hebrew language has no verb for "to be" led to the century-old misunderstanding.—L. SELIGSBERGER.

To read your "Ethicogenesis" in the current SCIENTIFIC MONTHLY was thrilling to me. It was like a cool breeze on a hot day. So clearly and beautifully set forth, and so inspiring in its implications, your exposition of rational ethics has placed us all under obligation to you.—ALVIN L. DAVIS.

I have read conscientiously the article "Ethicogenesis" by Dr. Chauncey D. Leake to which you assigned first place in the April issue of your magazine.

The attempt to give ethics a scientific foundation seems to me most praiseworthy since the authority for making good use of knowledge must come from knowledge itself and not from superstition.

What I do not understand, however, is why the *good* must be identified *scientifically* with that which has survival value for the individual or the species, since the test of scientific truth has never been its survival value for the scientist or his human species.

I notice also that it does not require a tedious analysis to discover that if the good is *scientifically* identified with survival value, the net result is that Science is made the intellectual handmaid of Machiavellism. This is so because sometimes it is honesty that has the survival value, but too often survival for the individual or his group lies the way of fraud, deception, deceit. It was looking at the survival value straight in the face that Machiavelli warned his Prince that the practice of virtue is a risk, but the appearance of virtue is safe and commendable. Looking at the survival value he recommended as "useful vices" cruelty, hypocrisy, etc.

Then, too, I doubt very much whether even an ethics for the species could be evolved on the survival value idea. "Survival is 'good,'" Dr. Leake says, "in the very significant sense that if the species fails to survive, 'goodness' has no further meaning for that species." But goodness can have no meaning for such an abstract concept as "species." Any student of semantics would tell us that much. Goodness has meaning only for the individuals constituting the species. Now no individual can be expected to risk his survival in the interest of the species since for each individual the species dies with him and goodness for him is coexistent with his own survival. When he is dead, Dr. Leake reminds us, goodness has no further meaning for him.

Scientific ethics must be something above and beyond this survival value concept which in my estimation debases both ethics and science. A scientific ethics must be an ethics based on the nature of man, not on the conditions for survival. Science could tell us, for instance, that the authority for the *ought* must rest on the nature of man's being. A stone, science could tell us, has no categorical imperative to act as a bee, nor a rat as a man. Science could make it very plain that independent of the *is* the ought is meaningless. And the answer to the question, what *is* the nature of man is certainly science's jurisdiction.

It can be scientifically determined either by historical observation or by laboratory experimentation that man has a double nature:

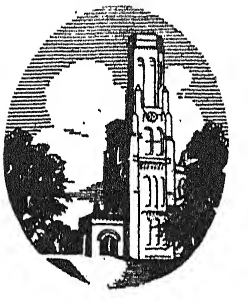
1. Independent of all nurture, man has an affinity for things, which lies back of his specific urges to grab and to hold, and which spells historically class struggle and international wars.
2. Independent of all training, just by pure nature, man has an affinity for correct solutions, just solutions. (A just solution differs from a correct solution merely in that in the case of the just solution the factors of the problem are sensient beings.)
   This affinity for right solution has produced both science and philosophy.

It can be proved scientifically that man has one part of his personality which is not satiated with things but with correct solutions. And it is on this condition of man that ethics has its true scientific foundation.

For the side of man's personality which is not satiated with things, Machiavelli has no message. Hypocrisy has no appeal for this side. As man's stomach hunger is not placated by a cardboard chicken, so this other hunger for correct solutions is not placated with the appearance of right doing.

We have plenty of historical witnesses testifying to the fact that this part of man's nature is no 20th Century addition, that it had always been there. This portion of his personality is what the Greeks glanced at and called Intellect, what Kant peeped into and called Reason, what Christ scrutinizingly observed in all its potentialities and called it the kingdom of heaven—the kingdom of heaven within man, small as the grain of mustard seed.—ANA MARIA O'NEILL.

THE BROWNSTONE TOWER



OCCASIONALLY we receive indignant letters asserting that certain articles we have published do not belong in the SM because they are unscientific propaganda. Our experience shows that such letters are always directed against essays involving human behavior and human problems, and we suspect that our correspondents' indignation has been aroused not so much by the character of these essays as by the views expressed in them. Nevertheless, we are caused to reflect on the possibility that we may be allowing propaganda to creep into the SM.

We could seal the SM against social, political, and religious propaganda by restricting it to the natural sciences. If this were done, we should not only be avoiding our obligations to sections H, I, K, L, and Q of the AAAS but would be doing more harm to the SM than good. We must not expect essays in social science to be mathematically exact and we must bear in mind that in the field of human relations, which interests everyone of us humans, scientists cannot always be scientific; indeed they are usually unscientific. Take, for example, the assertion just made. Is it true? Consider the elaborate and time-consuming philosophical and statistical investigation that would be needed to answer this question; that is, to give scientific validity to our assertion. It is not feasible to confirm the apparent truth of many statements made by scientists on human problems. Their assertions, decisions, and recommendations are based not only upon statistical information but upon experience. To the reader the truth of such dicta must be judged by the extent of their agreement with his own experience. We believe that appeal to general experience in lieu of factual numerical data is entirely justified, for, if scientists refused to express themselves except on numerically predictable subjects, they would leave their fate and that of their fellow men entirely to politicians.

If our reflections are reasonable, we should continue to publish articles on social problems and perhaps to increase the proportion of space devoted to them, not only because of their intrinsic interest but because scientists wish to take greater social responsibilities than they have done in the past. How shall we decide what manuscripts to accept? Let us discuss this question by means of examples from the August issue.

Our leading article by Emeritus Professor A. G. Keller of Yale was on compulsory military training in peacetime, a question of national importance that cannot be answered in the laboratory. We accepted Professor Keller's essay because (1) it was timely, (2) it was well written, (3) it represented the views of a man who was qualified by a lifetime of professional experience to make recommendations on this question of public policy. Because he advocated compulsory training, we knew that some of those opposed would cry propaganda. But Professor Keller cannot be a propagandist. He is retired, has no ax to grind, and speaks up only as an individual scientist and citizen who is concerned about the welfare of his country.

On a controversial subject we think it is better to publish a separate article on each side of the question than to print an apparently scientific and dispassionate article in which the pros and cons are neatly balanced without recommendations. Sometimes we can publish on different aspects of a subject in the same issue. Thus, in the August issue we published Dr. F. B. Sumner's magnificent essay on old age and death—the orthodox point of view of a biologist. Then to show that physical scientists and engineers often have a different outlook on life as old age approaches, we published an unusually pertinent sermon by an engineer, David Moffat Myers, who spoke "the gospel truth." More often we must give a hearing to the opposition in a subsequent issue. Thus, in the present issue the opponents of Leake's "Ethicogenesis" and of Keller's compulsory service present their arguments.

We recognize the danger of accepting unsound manuscripts when scientific standards do not and cannot operate. But we would rather take that risk than shun controversial subjects. We hope that our own opinions on such subjects do not influence our decisions on acceptability of manuscripts; we insist only that the writer express himself clearly and reasonably and with some originality. If we make errors of judgment in our selections, we have some consolation in the thought that misguided and erroneous papers have often done much good by arousing readers to action for the sake of truth.

PARTLY for its intrinsic interest and partly as an illustration of cosmopolitan social wisdom from a scientist who is not afraid to express himself, we publish on page 317 of this issue the thoughts of Dr, Howard A. Meyerhoff, a geologist, on certain provisions of the Report of the Berlin Conference, which will affect the economy of Germany and surrounding countries.

Since July of this year Dr. Meyerhoff has been our nearest neighbor in The Brownstone Tower. Temporarily on the floor above, he comes into our office periodically to sharpen his pencils and his wits. It is time that our readers knew him as the new Executive Secretary of the AAAS, not taking the place of either Dr. Moulton or Mr. Woodley but doing important work that neither of them was free to cultivate. If we were to call Dr. Meyerhoff the Diplomatic Secretary of the AAAS, the reader might guess what he does. Are the bonds of affiliation between the AAAS and other societies in need of attention? Dr. Meyerhoff will strengthen existing bonds or provide new ones. Does pending legislation concerning science and scientists need scientific guidance? Dr. Meyerhoff will provide it. To paraphrase it, he supplies power and oil for the machinery of the AAAS.

Dr. Meyerhoff is on leave of absence from Smith College where he made his reputation in geology and in public service. It was his outstanding performance as Secretary of Section E of the AAAS that caused him to be selected at the age of 46 for service to the Association as a whole. We will not list Dr. Meyerhoff's professional connections nor his services as a public-spirited citizen nor his experience in business and government. It is enough to say that he knows his way around in the world. We are happy to have him with us and hope that he never gets a pencil sharpener of his own.

YOU, kind reader, have received from the Office of the Permanent Secretary a leaflet explaining the need of the AAAS for a home of its own in Washington and appealing to you to contribute to the centennial building fund. Twice before in his scientific career the editor has worked for a new building, but never in his experience has a new building been so necessary as it is to the AAAS. Present personnel is strained to the breaking point by the work that must be done for the Association's enlarged membership. Because space is lacking, additional personnel cannot be brought into present quarters. We have standing room only.

In stressing the gravity of the situation the editor is not speaking for himself. His working facilities are better than those of any other member of the staff, and if he had an assistant editor in another room in The Brownstone Tower, he would be content to remain in the Tower indefinitely. But he can get neither the room nor the assistant, which he must have someday, somewhere, if the SM is to grow with the Association.

Consequently, we must all leave the Smithsonian Institution, but we hope that we shall not have to go far away from its inspiring environment. We are now on the greatest campus in the world, the Washington Mall, which we cross twice a day between the National Museum and the Smithsonian Institution.

To the west stands the glorious Washington Monument, to which our eyes never fail to turn in admiration. We have seen it in many aspects: standing sharp as a sword against an azure sky or mottled with fleeting cloud shadows or shrouded in mist like an enormous ghost. But one night not long after VE-Day we saw it framed in a most memorable manner. We descended the Tower elevator and stepped out into a dark room. Facing the elevator was a narrow window behind which stood the Monument, illuminated again after years of blackout. Our spirits soared to the apex of its gleaming aluminum tip. What a symbol of the highest and purest human aspirations!

At the east end of the Mall stands the Capitol, now also illuminated at night. But we like to think of it as it appeared late one summer afternoon when it was surmounted by a rolling, towering, luminous thunderhead, which gave it superhuman significance.

From our office at night we can see that the light shines again on the great statue of Lincoln in the Memorial. Again facing reconstruction following the devastation of war, we think of Lincoln's second inaugural address carved on the wall of the Memorial: ". . . let us strive on to . . . do all which may achieve and cherish a just and lasting peace among ourselves and with all nations."

We *must* leave The Brownstone Tower, but when we depart for the last time let it be on a windy night with ragged clouds blowing across the face of a full moon behind the Tower.

—F. L. CAMPBELL

THE SCIENTIFIC MONTHLY

NOVEMBER, 1945

THREE AT BIMINI

By E. A. ANDREWS, R. P. BIGELOW, and T. H. MORGAN

PROFESSORS EMERITI: JOHNS HOPKINS UNIVERSITY (ZOOLOGY), MASSACHUSETTS INSTITUTE OF TECHNOLOGY (ZOOLOGY), AND CALIFORNIA INSTITUTE OF TECHNOLOGY (BIOLOGY), RESPECTIVELY

On the maps of the *Encyclopaedia Britannica* some very insignificant fly specks some fifty miles east of Miami, Fla., and on the west edge of the great Bahama Banks bear the name, greatly larger than themselves, "Bimini Islands." Mere points of the vast shallow Bahama Banks that here project above the sea, they form the nearest dry land (and at time of prohibition the nearest wet land) off our Atlantic coast. On North Bimini some people dwell in what is called "Alice Town" but none on South Bimini, though so close, and the big white scavenger crabs taken on the South Island are eaten as coming from a region "where people do not use."

Three students went to Bimini in 1892 to seek traces of relatives of their very remote ancestors. That they went to study marine life in such a remote corner of the world calls for a statement of the aims of zoology in the closing years of the nineteenth century. Naturalists were then eager to test the theory advanced in 1859 by Charles Darwin: that animals had gradually evolved from changes in ancestors. Natural history was becoming a real history of living animals traced back to their presumptive ancestors.

Not only did fossils suggest that some of those now extinct animals might have been like the ancestors of those now living today but embryos and young stages of some living animals led to the inference that such young might be more like the ancestor than are the adults.

In the seas more than on land phenomenal transformations are to be seen, the young becoming adults through changes that would not have been predicted. Young mollusks, worms, crustacea, echinoderms often are quite unlike their parents and must undergo a metamorphosis to become adults. In some instances it seemed probable that the young rather than the adult was like the assumed ancestor. It was said that an animal repeated its ancestral history, the young being like the remoter ancestors: An animal "climbed its ancestral tree." For instance, the barnacle sticking to rock or boat seems in its calcareous shell much like a mollusk, but it proved to be a crustacean that lies on its back and "kicks its food into its mouth," as Huxley expressed it. It was found that the young barnacle is like a water flea and after devious changes finally settles down and becomes fixed. Here the young suggested that the ancestor of barnacles was something like a water flea and nothing like a mollusk.

Vast areas of shallow seas, such as the Bahama Banks, swarm with sedentary animals, corals, sponges, sea fans, mollusks, starfish and many more, which, when young, rise near the surface of the ocean and drift about in ocean currents as swarms of minute living things of often bizarre and remarkable forms. "Plankton" was the term applied to these drifters.

Coming to this country, Louis Agassiz stimulated study of marine life, and from his seaside laboratory on Penikese Island, Mass., one of his students, W. K. Brooks, came to Baltimore in the early days of The Johns Hopkins University and, aided by contributions from citizens of Baltimore, started study of life in Chesapeake Bay.

But, though so important for its oysters and crabs, the life of the Chesapeake proved limited for solution of ocean problems, and so the work was extended to Beaufort, N. C., and later to Green Turtle Cay, off Little Abaco, Bahamas, where Brooks and his students spent the memorable summer of 1886. Here was an island that should have been surrounded by pelagic life in abundance; however, either the coral reef animals ate out all the plankton, or the prevailing currents of water and wind failed to bring plankton to the nets of the visiting naturalists.

Where was a bit of land from which to catch the floating animals of the ocean? Alexander Agassiz, the son of Louis Agassiz and a world-wide voyager, said that the Bimini Islands, on the edge of the Gulf Stream, would be a good place to try.

One afternoon in June, 1892, we loaded our baggage and apparatus with some needed furniture onto a Merchant and Miners steamer in Baltimore and started that night for Savannah, Ga. Thence by train we made a daylight trip through desolate country to Tampa, Fla. We continued on a few miles further to Port Tampa, which consisted of a trainmen's hotel and a pier extending far out into the harbor. Armed with shovels, nets, buckets, and jars of Bouin's fluid, we proceeded on our hunt for the elusive Amphioxus that we heard was to be found in Tampa Bay. One of us, at least, never had seen a living Amphioxus and was thrilled to the core at the prospect of finding this most primitive type of cordate animal in its native habitat. At length from a shovel full of wet sand a silvery, slender animal was seen to wriggle swiftly over the edge of the shovel and disappear in the sand below. More digging brought to light many specimens, a few were captured, but almost all escaped by swimming with incredible speed into wet sand before one could pounce upon them. As the tide ebbed, patches of sand became dusty under the hot sun, and one of us had the brilliant idea of throwing the wet sand onto these dry spots. That was too much for the Amphioxus. He could not penetrate dry sand, and soon our collections became so abundant that we grew reckless and threw specimens of this unique species into the capacious mouth of a tame white pelican that waddled alongside to see what we were doing and bit at our legs when we attempted to limit his consumption of his own food, our long-to-be-preserved specimens of *Branchiostoma caribaeum.*

A day or two later aboard a small steamer bound for Havana we came to Key West. Eventually we found a small sloop that could be hired and a captain who said he knew the way to Bimini. Having made our bargain, we visited markets and stores to purchase a supply of provisions to last through our projected stay of three months at Bimini. The crew consisted of the captain and owner of the boat (No. 1); another "captain" (No. 2), who came along as guest of No. 1 to help with the navigation; and a boy who served as cook, cabin boy, and crew. Captain No. 2 was described by No. 1 as a "great reader." We found out later that his reading consisted of dime novels.

With a fair wind and a smooth sea we sailed along happily within sight of the Florida Keys until late afternoon. We passed Key Largo, and the captain said it looked like rough weather and we had better seek shelter. Whereupon he turned toward shore and anchored in the lee of a small island some distance from a lighthouse to the south. After supper the wind became stronger, and another anchor was put out. Finally the drifting ceased.

The next morning we found our ship to be lying on a soft mudbank and tilted over to an angle of perhaps 30 degrees. At high tide we tried to haul the ship off the bank, but without effect. By days of hard work we took ashore everything, including the rusty mass of iron ballast, and finally she floated on a high tide. We lost no time in getting under way and sailed out into the deep blue of the Gulf Stream. We drifted swiftly to the north while headed for the Bahama Banks. Thus we sailed all day on a smooth sea with the sheets slacked well off to port while we lounged on deck, resting from our arduous labors of the previous days. But the captain took us to the wrong side of a lighthouse near Bimini, and the ship fetched up with a jerk on the Bahama Banks.

Our distress signal in the rigging brought no help from the lighthouse nor did our flares after dark. Later the lightkeeper told us he had seen our Roman candles, but could do nothing to help us. A rising tide at dawn gave courage for renewed efforts, and at extreme high water they were crowned with success. The ship undamaged was once more afloat. We got her headed toward deep water, hoisted sail, and proceeded well off shore past the lighthouse and along the coast of South Bimini until we reached the entrance to Bimini Harbor. We passed in through the narrow entrance against a swift tide and anchored near shore off Alice Town. This was June the sixteenth. When the sails had been furled and all made shipshape, we saw an approaching boat rowed by four men. Seated in the stern was an official in a beautiful white uniform. He was the Resident Magistrate acting in his capacity, seldom exercised, of customs officer but obliged, as he said later, to pay his rowers from his own pocket for each such rare occurrence. A former lawyer of Jamaica, B. W. I., representing the Queen in this small part of her domain, this coffee-colored official came over the side and at first was inclined to be very pompous and haughty, regarding with evident suspicion our leader's explanation of the purpose of our visit. But his attitude changed quickly when he saw our credentials. This impressive scroll, inscribed with our names, titles, and degrees, signed by Mr. Gilman, and sealed with the large red seal of the University, made a great impression. He at once became affable and cooperative. Our goods were admitted free, and we were asked our needs. Our first need was a house, and he pointed to one we might have beneath a grove of coconut palms. We were taken ashore, the owner of the small house was found, and he was told by the Magistrate that he was to rent it to these gentlemen. A bargain soon was made, and a week's rent paid in advance. (From that moment until our departure our landlord was seldom sober.) Meanwhile the Magistrate had hailed a small colored boy: "Hey, Johnnie, go tell your mother to come and cook for these gentlemen." She came and served us well. Also with the help of our friend, the Magistrate, we secured a small boat and a white boatman. We found North Bimini a small area, narrow and about 5 miles in length, somewhat horseshoe-shaped with South Bimini lying quite near as if in the arms of the horseshoe, leaving an entrance to the harbor between them through which the tide flowed so fiercely—about 3 miles per hour—that when anchored in it we let the towing nets drag out and had no need for the usual strenuous rowing to keep the nets full. In the incoming tide floated many minute members of the Gulf Stream fauna, and with the outgoing tide came many different creatures derived from the vast areas of coral and sea fan bottom of the great Bahama Banks. The harbor opened wide and very shallow to the east but was narrow and deep enough for small vessels to the west. There were plenty of sand flats and some lagoons with mangroves along the south island, so that animals had manifold opportunities for finding fit environments.

The Bimini Islands are almost like vessels anchored in the Gulf Stream as they lie on its edge where it is most narrow and hence most swift. The northern, inhabited island has considerable elevation, sloping from a bluff on the west down to the harbor. At the highest point the top of the bluff is perhaps 50 feet above sea level. At its base the bluff is bordered by a broad sandy beach extending a mile or more in a northerly direction from the harbor mouth. On the harbor side of the island there is a ledge of coral near the mouth of the harbor, and then a narrow beach where small boats may be hauled up. The south island is low, sandy, and barren, with low shrubby growth and coarse grass. It is bordered on the harbor side by broad, sandy shoals that are exposed at low tide. On the north island there were two settlements. The white people lived in Alice Town in a grove of coconut palms near the entrance to the harbor.

One day we asked our boatman how many white people lived at Bimini. After some thought he said, "Ninety-nine," and on second thought added, "No, if you count the magistrate and the school teacher there are a hundred and one." They all lived in Alice Town. There were perhaps 500 Negroes in

the other settlement adjoining Alice Town on the east.

The people of Bimini were a law-abiding lot in their scattered houses along the grassy road that led down to the west end cemetery, which was kept in repair by the labors of the occasional culprit who as a prisoner broke stone for the road while the constable sat watching him and then joined in the fellowship of lunch. This road had its daily uses for here the sea was bordered by mangroves standing out over the water to supply a rustic latrine where evidences of infection with large entozoa were observed.

Our laboratory was a house (Fig. 1) of two rooms and a closet, all with windows that could be closed by solid wooden shutters but were innocent of sash or glass. So we had plenty of light for using the microscope, and we worked several feet above the ground because the building was elevated on its wooden legs. One small room served as kitchen, and there the native genius with some sand and few stones, bits of charcoal and a tin can performed the skilled operations needed in making coffee and soup and preparing fish and canned goods for our hungry party of three.

Our dormitory was an old warehouse (Fig. 2) still cluttered with tackle and parts of machinery once used in a failing attempt to raise sugar cane profitably. We avoided the mosquitoes at night by keeping the wooden shutters closed till dark and then sneaking in, lighting candles, and hurriedly tumbling into our hammocks before we were detected.

FIG. 1. A HOUSE ON BIMINI USED AS LABORATORY, DINING HALL, AND KITCHEN

FIG. 2. OLD SUGAR HOUSE USED AS DORMITORY

The people of Bimini lived chiefly on and through the sea. Sailing vessels sometimes brought mail, and there were visits from the Bishop in his black piratical-looking schooner fitted with a cabin for ceremonial marriages and baptism. The vocations were chiefly three: sponging, turtling, piloting; with interspersed salvage of shipwrecks.

Sponging was conducted as follows: two men would paddle a dugout to a favorable location on the Banks near the south island where, over water many feet deep, one man with a long swaying pole armed with a fork managed to spear the little black object on the white sands of the bottom and wrest it from its firm attachment to the ocean floor. With difficulty the other man held the boat still in the current. The rippled surface would obscure objects far below till the spearer held overboard a bucket or box with glass bottom; then he would get a startlingly clear view of what was on the white sand. One by one the black, wet sponges would accumulate in the middle of the canoe. (Some that we at once thrust into strong alcohol even yet show the labyrinthine structure, the intermingling of horny skeleton and living cells that bring in food and water to the whole system of internal canals.) Having been brought home, these sponges were heaped on shore to die and later soaked in the sea to rot, protected by a net to keep them from drifting away. After that they were beaten to remove the dark flesh and reveal the brown horny skeleton, which was dried and taken to the great sponge market at Nassau to be sorted and sold. Ultimately, some appeared in our drug stores, and at that past period they were used by students in elementary schools for cleaning slates.

The hunt of the hawk-bill turtle, the source of the precious tortoise shell, was carried on mainly by Negroes on two-masted schooners that ranged far and wide for this valuable

material. Voyages were so long that little pigs, taken aboard to eat the offal, grew fat before the return home. The shells of the much larger loggerhead turtles were not valued, but their flesh was the chief supply of meat at Bimini. Poultry there was, and fish were abundant. Female loggerheads came to North Bimini to deposit their eggs on the broad sandy beach, high up near the cliff. One bright Sunday morning, it was said, a small boy, playing hooky from church, found a large female loggerhead crawling toward the cliff. His excited shouts were heard in the little Wesleyan Chapel perched above the edge of the cliff, and one by one men and boys of the congregation edged out and then rushed to the beach and joined the fray. At last the preacher too joined the party that finally overcame the turtle and once more Bimini had a taste of fresh meat. Sharks crowded to shore to get their share.

Piloting was exclusively in the hands of white men. For that purpose about half a dozen small sailboats were kept moored in the harbor close to the shore of Alice Town. Each boat was fully equipped with food, water, and a supply of "curiosities." Its sails were in stops, and its mooring could be slipped in an instant. These boats were never used for any other purpose. On the highest point of the cliff was a flagpole with a crow's nest. Here a boy was posted to watch for approaching vessels. From his perch the boy had a magnificent view over the water—golden yellow near shore, bright green farther out, and then deep blue in the Gulf Stream. His interest, however, was in detecting the first appearance of some vessel far to the north, bound south and hugging the shore to avoid the strong current. As soon as he saw a sail or the smoke of a steamer, the boy blew his horn and the fun began. All Alice Town turned out. Pilots and their crews came running past our laboratory, turning up their trouser legs as they ran. At the water's edge one man of each crew would stoop to allow his pilot to jump on his back, and with this burden start wading to the boat. The other crew men would plunge in, wade quickly to the boat, and hoist sail. The pilot on reaching the boat took the helm, his bearer went forward and slipped the mooring, and they were off. The crowd gathered at the shore to watch the start and then all rushed off to the cliff edge to get a good view of the race to the ship. It was an exciting race. The first skipper to reach the ship was taken on as pilot. His rivals and their crews swarmed aboard, nevertheless, hoping to exchange their curiosities—dried sea fans, staghorn corals, conchs, and other shells—for much needed cash. From time to time, a vessel coming by at night or in bad weather, missing the pilot, would strike a shoal and be wrecked. When we were there, the inhabitants of Bimini complained bitterly that they "had not had a good wreck in two years." To be sure, one or two coal schooners had gone ashore along their coast, but what could one do with coal? However, that wreck of two years before had been really a "good one." It was a fair-sized ship with a cargo of all kinds of merchandise. The British Government has rigid laws concerning the salvage of a wreck. A wrecking master takes charge to prevent looting and to see that the owner's rights are respected. But if a package should happen to break open, then its contents are free to all, and the Bimini wreckers know how to profit by such an accident. That wreck of two years before had been very profitable, and some of the loot was still on the market. We were offered, from time to time, various articles of clothing at tempting prices. We also heard of boxes of drygoods intended for South America containing, amidst the drygoods, concealed watches and jewelry!

We were entertained very pleasantly by the Resident Magistrate and by the Rector. The Magistrate's wife was a pretty blonde, and he obviously was very proud of the blue eyes of his engaging small children.

The Rector (or Vicar?) was of the Anglo-Catholic type and always wore a long cassock, which, in spite of its white color, looked very hot and uncomfortable. One day when we were dining with him, he asked us quite seriously whether turtle was fish or flesh. He complained that fresh meat was very scarce in Bimini and that loggerhead turtles were caught too often on a Friday. One of us assured him that turtle is fish, hoping

that this might be true for ecclesiastical purposes since the turtle is not a warm-blooded animal. Our friend had taken this missionary assignment in Bimini because of lung trouble that made the climate of England a danger to his health, and he found his solitary life here very dull—his congregation was very small and his pitiful little choir of black boys was difficult to control.

Our interest was mainly in marine invertebrates. These we collected in four principal areas. Along the small coral reef near the mouth of the harbor we used a long-handled scoop net to collect crabs, mantis shrimps, mollusks, and the like, and even dived for choice specimens under the coral reef that we could look up at through the crystal clear water. We dug for burrowing animals on the sandy shoals of the south side of the harbor. But our chief delight was in towing a fine net slowly behind the boat in the harbor, or out in the blue water of the Gulf Stream.

There were exploratory walks about the islands as when we strolled to the Negro settlement, where we got the impression that some of the Negroes were not hospitable to white visitors.

Then too we were told that we should visit the "Fountain of Youth," for was not this "Bimini"! In 1513 the king of Spain sent Juan Ponce de León to discover and settle the fabulous Bimini where the Fountain of Youth was supposed to exist. It was believed to be endowed with great powers to cure disease and to restore youth to old men. When Ponce de León discovered Florida, the King sent him again as governor of both "the island of Florida and of Bimini." Apparently, the spring was not discovered; at all events Ponce de León died in Cuba in 1521 at about the age of sixty years. We were told that the spring was on the south island of Bimini, and to it we made a pilgrimage and brought back some of its miraculous waters scooped from a hole in the lime rock that might have held a barrelful of water, not salt but fresh. Its powers—are they not attested by the fact that we three now have lived nearly 250 years already!

One day we were talking with our boatman, and he told us of the "curly-tailed lizard," which, he said, sat on its haunches like a dog and ran on its hind legs with its tail curled over its back. A few days later he took us to South Bimini and, sure enough, there under a small bush sat a curly-tailed lizard just as he had described it. We found some others, several of which were caught after a lively chase.

The research work of the triumvirate was diverse. One studied a jellyfish and mantis shrimps. This peculiar jellyfish rested quietly on the bottom of a little lagoon among the mangroves of South Bimini, upside down, not swimming about and eating fish as do other large jellyfish. Its mouth is reduced to minute openings. Some fifty of these jellyfish of different sizes and ages were found forming a retired society. Here at Bimini it was found by experiments that they cleverly captured and digested small water fleas. These jellies grew from ½ inch to be 6 inches wide and with their colors and radiated parts looked not unlike flowers lying on the muddy bottom, the males different from the females in color. It was found that the female holds the young for a while after they come through the stomach (for in jellies the eggs pass first into the stomach). When they swim free they do not look at all like jellyfish but rather like simple protozoans, as they are minute cylinders actively propelled by innumerable paddles.

The mantis shrimps, or squillas, are rather flat leggy creatures that often live in burrows. They may have strange colors. Their claws are like those of the common mantis, an insect in which the end of the limb folds back much like the closing of the blade of a pocket knife against the handle. The young may be incredibly transparent and of bizarre forms as they swim about in the drifting life of the ocean. Many of these young received names as independent creatures before it was proven that they were but the young stages of mantis shrimps. Of the five kinds of mantis shrimp we found at Bimini two were described as new, though much like some others known in distant Australia. Their young differ so much from their parents that one can be identified with the other only by experimental breeding. One day

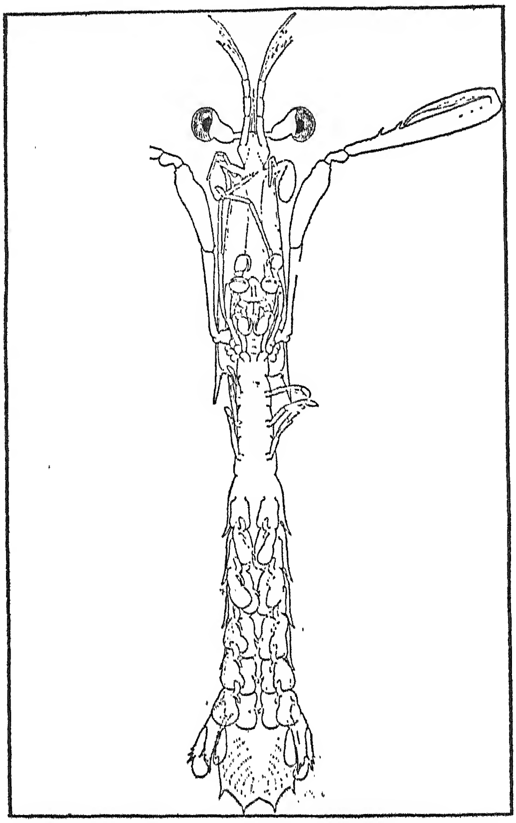


FIG. 3. LARVAL MANTIS SHRIMP
TAKEN IN A TOW NET, IT CHANGED INTO THE WIDER, SHORTER FORM OF FIGURE 4, SAME ENLARGEMENT.

we took two that were just alike, five-eighths of an inch long and apparently in the last larval stage. One of them was pickled in alcohol; the other (Fig. 3) was kept alive and nursed with loving care in sea water renewed at frequent intervals by water brought from harbor to laboratory in a bucket. One morning, to the great joy of the observer, this little animal was found to have shed its skin, and in so doing to have gained in width at the expense of one-fifth of its length. It was now in the adult form (Fig. 4) and could be identified as *Squilla quadridens* (having four teeth on its great claws), a form previously known from the coast of Florida. In addition to many beautiful but unidentified larval forms taken by the towing net on the surface of the water, we found on the bottom in shallow water many adult mantis shrimps. Two well-known species, *Gonodactylus chiragra* and *Pseudosquilla ciliata* were common on the sandflats in a foot or two of water, hiding among shells, stones, and seaweed, where their coloring, varying from mottled green and white to nearly pure green, was distinctly protective. When disturbed they moved from one shelter to another with great rapidity. At Nixies' Harbor we found a burrow in the sand where a male and a female were living happily together. They were about two inches long and beautifully colored opaque white with transverse bands, faun-colored in the male and pink in the female. This species, previously undescribed, was named after the islands, *Lysiosquilla biminiensis*. Another undescribed form was found burrowing in the coral sand of Bimini Harbor, nearly pure white; they were named from their color, *Squilla alba* (Fig. 5).

Another of the trio came to Bimini only to study the Bahama tornaria (Fig. 6), which the English naturalist Weldon had seen at Bimini. The significance of this creature is connected with the search for more knowledge of our possible ancestors. Zoologists tracing their ancestral records found difficulties in connecting the great groups together. Animals with backbones are very

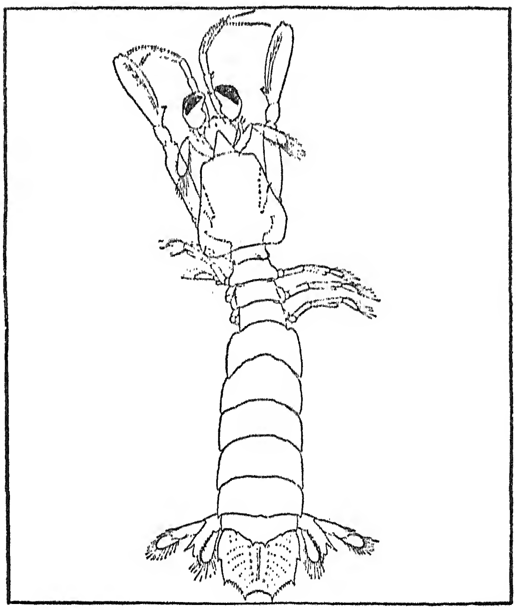


FIG. 4. ADOLESCENT MANTIS SHRIMP
Squilla quadridens, REARED FROM LARVA OF FIG. 3.

different from worms, starfish, or mollusks. How could one group derive from another? Vertebrate embryos show a characteristic and peculiar organ called the "notochord," a sort of chalk line indicating where the vertebral column will be laid down. They also show gill slits as are present in fishes. The claims of various worms were championed by certain naturalists. Among these pretenders to vertebrate relationship was the soft, slimy burrower in the sea sands, most unattractive and often protected by bromine-like odors; it was called *Balanoglossus* because the headend has the shape of an acorn (Fig. 7). There are several kinds of Balanoglossus, and some of them have young utterly unlike their parents, transparent creatures shaped like a top, long known as tornariae. Two kinds of tornariae were found at Bimini, one the big Bahama tornaria and the other as yet known only from Bimini.

One of the choicest of our finds with the towing net was a series of these beautiful animals of extraordinary size, the biggest as large as peas. From this discovery we inferred the existence in our vicinity of a very large adult, wormlike Balanoglossus. So we hunted for adults on the sand flats. There in shallow water we found holes and near them castings like those of earthworms. Our digging at first was fruitless. But finally, by moving very quietly toward a hole so as not to jar the ground and then making a sudden stab with a sharp spade, we did succeed in obtaining a few inches of a fleshy tube, as thick as one's thumb, that appeared to be the hind end of a giant Balanoglossus. That was all. Then we noticed that there were other holes with no castings. Perhaps these were the *intakes* of the burrows. Approaching these with stealthy tread, we finally succeeded in collecting several of the characteristic headends of a huge Balanoglossus. We never got a whole body. We dug them at Stokes Cay and at East Walls. We got these only at low tide, and there was evidence that the big worms put their heads up near the surface only in the afternoons and not in the mornings; we had to wait for afternoon low tides.

The third member of the trinity studied that problematical animal the lancelet, or

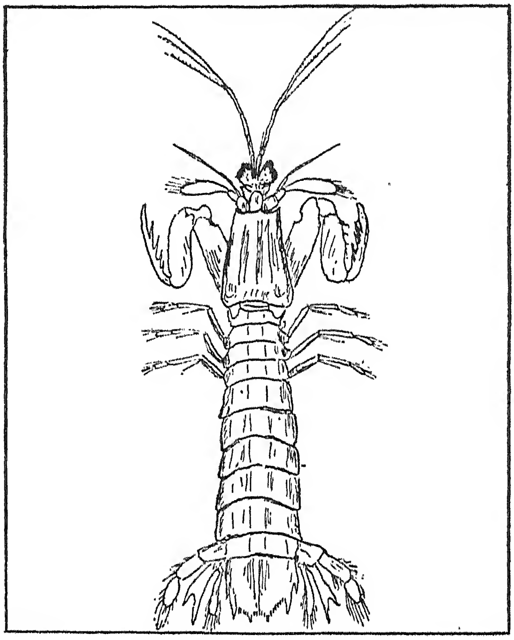


FIG. 5. *SQUILLA ALBA*
AN ADULT MANTIS SHRIMP DISCOVERED AT BIMINI.

Amphioxus, which is enough like a fish to be eaten in quantities by the Chinese. It has ample supply of branchial slits and was called *Branchiostoma* when thought to have gills in its mouth. A very muscular creature, it swims actively and burrows in the sand; but often it remains only partly buried, with mouth projecting to catch its minute food from the water. A simple, primitive creature, it suggests some of the features of a problematical vertebrate ancestor, having both notochord and gill slits. At Bimini we found them living not only in the white sand but swimming actively at night, when they were taken in the fine nets towed behind a row boat. In the laboratory they quickly buried themselves in the sand. Moreover, though entirely without eyes and with only dark specks in their spinal cords, they responded quickly to the direction in which the light struck them, going away from it. Placed in a dish with a window of the room facing east and another north, the lancelets swam from the open east window west and from the open north window south. When both windows were open the lancelets swam in a direction about southwest; that is, along the diagonal resultant of the two lights.

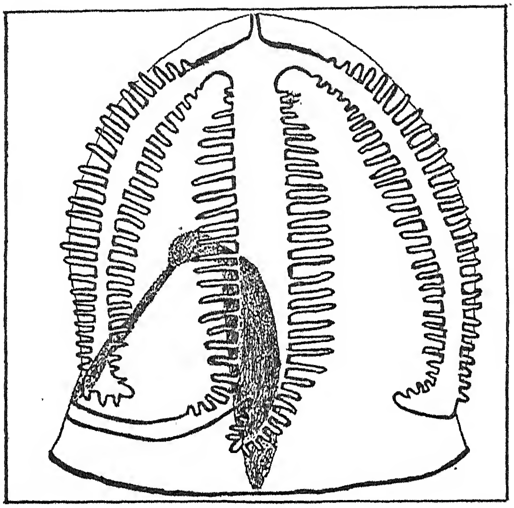


FIG. 6. A TORNARIA
THIS LARVA OF BALANOGLOSSUS CHANGED INTO THE WORM SEEN IN FIG. 7, OF THE SAME MAGNIFICATION.

All lancelets previously studied had, like vertebrates, the reproductive organs in pairs; one right, one left. But these lancelets from Bimini were not symmetrical; that is, they had the reproductive organ along only one side of the body. This we deemed of sufficient importance to make not only a new species but a new genus which we dubbed *Asymmetron* with the specific name *lucayanum*—an asymmetrical animal (Figs. 8 and 9) found in the region where the Lucayan Indians greeted Columbus and early explorers.

We arrived June 16 and hoped to stay three months—but did not.

One day there was great excitement in Alice Town; a strange sloop had anchored in the harbor. The skipper proved to be an Englishman who had been cruising in the Bahamas in search of pineapple shoots wherewith to start a plantation in Florida. Crossing the Stream, he had estimated his speed by timing chips thrown off the bow and floating to the stern of his schooner of known length. On his way home he put in at Bimini for repairs to his leaky ship, also to his eye-glasses, without which his vision was much impaired. One of us, who happened to have in his microscope case a very small screw-driver, undertook to mend the glasses, against the protest of the others who feared he would make matters worse. However, he succeeded, and the Englishman departed happy at having both glasses and ship repaired. With him as passenger went the one of us who had specialized on Balanoglossus. This was July the ninth.

The other two of us wished to remain until September. We were having a good time in Bimini. However, upon inquiry, we were told that if we stayed until September first, we should have to remain until the first of November. The hurricane season begins on August first and lasts through October. The Biminians have so great respect for hurricanes that at the beginning of the hurricane season every boat is hauled up on shore, and no one ventures out of sight of the islands. At length, we were told of a turtling schooner that would make a final trip to Nassau to sell its catch during the last week of July and would take passengers. Having packed our collections, apparatus and clothing, and

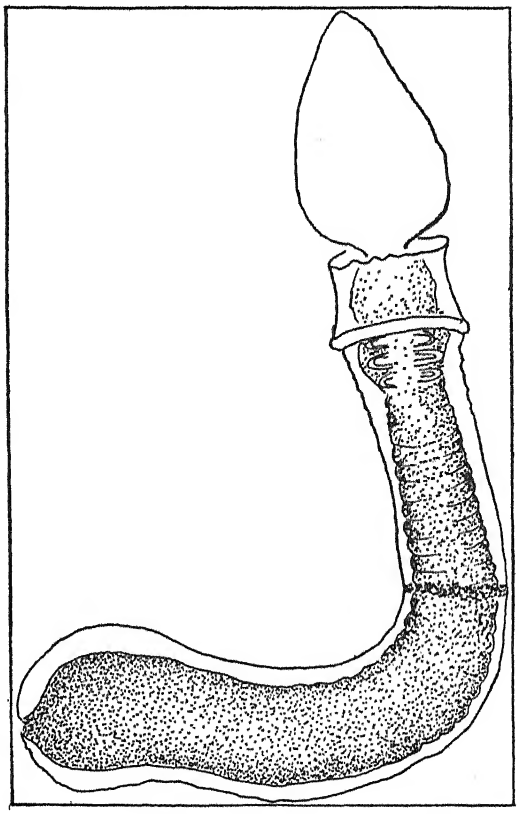


FIG. 7. MINUTE BALANOGLOSSUS
THIS WORM WAS REARED FROM A TORNARIA, FIG. 6.

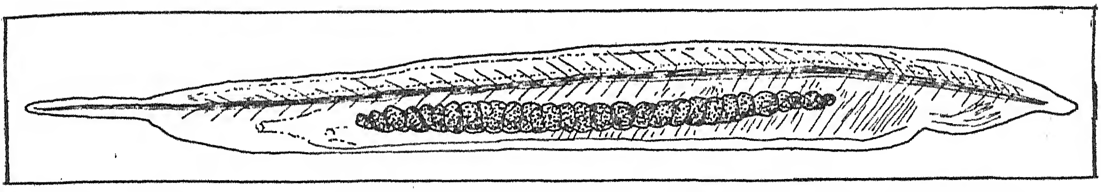


FIG. 8. THE LANCELET, *ASYMMETRON LUCAYANUM,* FOUND FIRST AT BIMINI
LOOKING AT THE ANIMAL'S RIGHT SIDE, THE DARK NOTOCHORD IS SEEN ABOVE AND THE SINGLE OVARY BELOW.

auctioned off our surplus provisions, we bade farewell to our friends in Bimini and embarked one fine morning for the voyage to Nassau. One of us did well as auctioneer, and the food we had assembled for three months was soon distributed to the eager natives—but only after paying duty on what we did not take away and also the proper fee for holding an auction in Alice Town!

We sailed from Bimini Harbor to the south until we were off Gun Cay Light, where we lay to, awaiting a boat from the lighthouse with one or two passengers for our ship. When they were aboard we got under way and sailed all day across the Bahama Banks. How the captain made this passage through an intricate mass of shoals we never knew. The noon meal was served on deck. It consisted of a bowlful of a sort of hash of rice and some kind of meat, perhaps conch, and was not bad. In the port shrouds were hung strips of conch foot from which we were invited to take a bite whenever so inclined. This meat had a sweetish taste like scallops. We supplemented this diet with our green coconuts, which furnished both food and drink. That we had these coconuts was good fortune since we were by no means well supplied with fruit in Bimini. Waiting on the shore for the boat to sail, we talked with the owner of a fine tree heavy with coconuts just between our laboratory and the shore, and he suggested that if we could pick the fruit we could have both fruit and tree. The day was hot, breath fell short, and muscles were not in practice for pole climbing, but eventually with buzzing head and thumping heart, one of us got up to the welcome leaf-stalks and rested while cutting off abundant fine nuts to drop down to the others to be loaded on board.

The evening meal was much the same. After that we were entertained by music. The captain's wife brought out a hymnbook and sang, accompanied by one of the crew with a jew's-harp or an accordion. She did not sing the words of the hymns, but pronounced the notes do, re, mi, fa, etc. Some of the others joined in. It was amusing.

The next day found us still on the Bahama Banks. The weather was fine, but the food ran low. The cook came to consult us as to what we would like to eat. There was not much choice, except more rice. Most of the passengers were on deck, talking, reading, or simply looking over the sea on watch for a landfall. The captain's wife wore dark goggles, explaining that they were all the

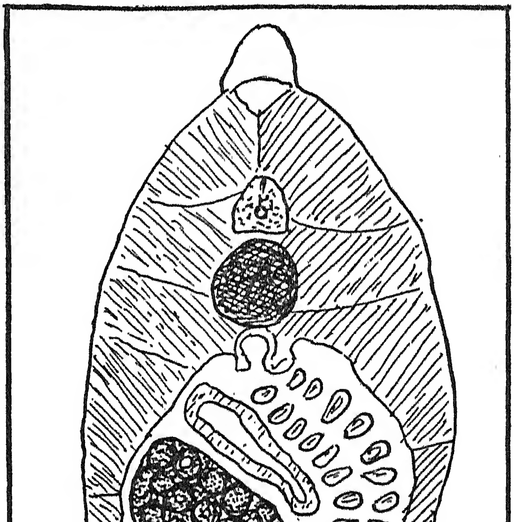


FIG. 9. A SLICE OF LANCELET
A CROSS SECTION SHOWS THE OVARY ON ONE SIDE ONLY, WITH THE NOTOCHORD ABOVE IN MUSCLE TISSUE.

fashion in Nassau. In the afternoon we sighted Andros Island to starboard and sailed out into deep water to a place where the captain believed us to be directly to leeward of Nassau. By a series of equal port and starboard tacks we reached there the next morning.

At Nassau we tied up at the sponge dock, where we parted from our shipmates and started out to explore the town. Although we were directed to the boarding house that usually took in shipwrecked mariners, we preferred the hotel.

Our first concern was passage home. We soon learned that there would be no steamer for the United States within a week or so. While waiting, we amused ourselves by sightseeing. We visited the sponge market where sponges were trimmed, sorted as to size and quality, and packed for shipment. Green turtles were there too, lying on their backs, earmarked for the English market.

We visited the sea gardens on the outer reef enclosing the harbor and there peered into the depth of a forest of waving seaweeds and corals mingled with rigid staghorn and brain corals and enlivened by the bright colors of fishes that darted in and out.

After seeing all there was to see in Nassau we made excursions into the country. A few miles out was the luminous Waterloo Pool. There at night we found a boat and were amazed by the phosphorescence stirred up by the oars. One of us stripped and plunged in; to the other in the boat he looked like an angel with four great luminous wings streaming backward from arms and legs as he swam. There fish rushed about like fiery comets. Thinking the luminosity due to bacteria, we collected a bottle of the water, but years later (1905), when the "Fire Lake" had become a two shilling evening entertainment, L. Plate, Professor in the Agricultural High School of Berlin, Germany, found in his fine nets shining dots, like the stars of heaven. Each proved to be a new sort of creature which he called *Pyrodinium bahamenense;* that is, the fiery whirler of the Bahamas. This is one of that remarkable company of peridinians that swarm the seas and often give it color by day and light by night: creatures that are plants with mouths they seem not to use, with medieval cuirass of cellulose plates, locomotor organs that make them revolve as well as advance, with active steering oar, and with thin expansions that give them bizarre forms and aid in keeping them easily afloat.

We found Bimini a most excellent place to study ocean drift from a solid island so close to the great Gulf Stream that its wonderful blue water can be seen from the shore. Opposite Bimini the Stream is only 45 miles wide, but it is about 500 fathoms deep and flows at a maximum of $4\frac{1}{2}$ miles per hour. In less than one-half hour one can row into the Stream where the fauna is both varied and abundant in calm weather. There is no doubt as to the suitability of Bimini for work on surface plankton. In the few weeks the three of us were there we gained, with very simple apparatus, the material for some contributions to zoological literature, to say nothing of happy memories.

MORE AT WASHINGTON

On page 336 of the foregoing idyl, "Three at Bimini," is a pen-and-ink sketch of the workshop of Doctors Andrews, Bigelow, and Morgan during a part of the summer of 1892. They did not complain about it, though it had all modern inconveniences. They could look forward to returning to their laboratories at Johns Hopkins to complete their work for publication.

We of the A.A.A.S. are just as tolerant of our prospective shelters as were the "Rover Boys" of their shack at Bimini. But where are we going from there? Are we to beg our way from dungeon to attic, from shed to warehouse, or are we to look forward to moving into our own building—a building reflecting the importance of modern science and scientists?—ED.

ETHNIC PATTERNS IN LATIN AMERICA*

By HARRY L. SHAPIRO

CHAIRMAN, DEPARTMENT OF ANTHROPOLOGY, THE AMERICAN MUSEUM OF NATURAL HISTORY

THE countries lying to the south of us share in common a number of characteristics that are frequently evoked to distinguish them from the United States. We have been taught that they inherit a well-defined tradition originating in the Iberian Peninsula and that they use Spanish and the related Portuguese as their official medium of communication. The literature which reflects their aspirations and embodies their culture is expressed in these same cognate languages. Their emissaries and their spokesmen whom we most frequently encounter are likely to be the descendants of the Spaniards and Portuguese who conquered the countries they now represent. For these reasons, among others, we tend to speak of them collectively as Latin Americans and to endow them with a homogeneity that masks their many profound differences.

Although it would be inaccurate to deny or even to minimize the indubitable community of tradition that unites our southern neighbors, no true understanding of them can afford to overlook the diverse ways in which they deviate from one another, often to a considerable degree. One of the ways in which they differ most is in the ethnic composition of their various populations. Contrary to the fairly common belief that most Latin-American countries are predominantly Spanish or Portuguese, there are, in fact, a number of them that are largely Indian and still others whose Negro population far outweighs the European or Indian contingents. These three racial groups, Indian, European, and Negro, together with the various mixtures among them, account for almost all the population of Latin America, but their several distributions throughout the area fall into a diversified pattern brought into existence by a series of cultural, climatic, and economic causes. The remaining elements of any consequence consist of Japanese, Chinese, Javanese, and East Indians, all of whom, however, are only of local significance and together form a very small fraction of the total population of Latin America.

Anthropologists are quite agreed that man did not originate in this hemisphere and that consequently all its population, past and present, was ultimately derived from Old World sources. The settlement of Latin America, and indeed of the entire New World, was accomplished through two historically distinct migratory movements. The first of these brought to the Americas the people we call Indian. When this movement began, how long it endured, and whether it was continuous or a thing of fits and starts are questions that cannot as yet be answered with any assurance. At present there exists a majority opinion that the firstcomers began trickling in about 25,000 years ago, at the end of the last ice age. Dissenters from this view have, on the one hand, pushed back this date as much as 50,000 years and, on the other, moved it up to a mere 2,000 years ago. Even the geographic entry is no longer so certain as it seemed a few years ago when the opinion was general that Alaska was the funnel through which these migrants passed on their way to the New World. Only recently, genetic studies of the native cotton of Peru have made it necessary to reconsider the possibility of trans-Pacific movements, a hypothesis advanced on other grounds as well.

If we cannot determine whether the migration was effected as a steady stream or as a series of irruptions, it does seem clear that peoples were still continuing to move across Bering Strait well into the Christian Era. If we accept the majority opinion as to the age of man in the New World, based on archaeological and geological evidence, then the span of time during which migration was going on reaches close to 25,000 years.

The determination of this point has considerable bearing on the problem of Indian

* From a paper delivered at a Conference on Inter-American Affairs, held at the University Museum, Philadelphia, Pa., November 17, 1944.

homogeneity. We used to be told that the aboriginal inhabitants were a uniform people distributed from Tierra del Fuego to the northern reaches of North America, and capped by a thin layer of Eskimos who themselves were related to the Indians, since both Eskimo and Indians were classed as Mongoloids. If, therefore, the history of Indian migration extends over 25 millennia or more, we must, to conform with this view of Indian homogeneity, assume that all during these thousands of years a single Asiatic stock was alone furnishing the migrants who were finding their way to the New World. Not only does this make inordinate demands on our credulity in the permanence of Asiatic conditions, but it is not borne out by the internal evidence provided by the Indians themselves. For, although the various types of living Indians do not suggest extremely diverse racial origins, they demand a sufficiently distinct ancestry to account for the differences between a Sioux and a Northwest Coast Indian, and between an Indian of our eastern woodlands and a denizen of the Amazon Basin. If we are to classify them all as Mongoloid, and I think on the whole justly, we must also keep in mind that there are many varieties of Mongoloids in Asia from which the Indians may derive their heterogeneity. I make this generalization on the origin of Indian variation without intending to deny the possibility that some of it may have developed in America.

When the Spaniards and Portuguese reached the New World they found it inhabited throughout its length and breadth by the Indians. The distribution and concentration of the aborigines, however, were by no means equal. For the most part the land was only sparsely occupied, except in two regions where the population had grown to relatively large proportions. In Mexico and Central America and in the Andean Highlands of South America civilization had taken root and was supporting a dense population. Estimates for the total population of the Western Hemisphere at the time of Columbus vary considerably, some authorities suggesting as many as 50 million inhabitants and others more conservatively giving about 8 million. Whatever the actual figure may have been, all are agreed that the Inca Empire in the Andes and an area comprising central and southern Mexico, Guatemala, and Honduras contained at least half, and probably a good deal more, of the total Indian population.

This, very briefly, was the situation when the second era of immigration began in 1492. Although we are here concerned only with the consequences of this migration for the New World, it was an event of world-wide importance and to be fully appreciated must be seen as such. Of all the mass movements that have taken place in the past, none approach in magnitude of numbers or in area involved the post-Columbian migrations from Europe. The stream of people rising from Europe spread throughout the earth, but its principal course set toward the New World and has continued in this direction throughout its duration of 450 years. The Mongol invasions of the thirteenth century, although comparable in the distances traversed, were confined to a relatively small number of conquerors. Similarly, the historically significant *Völkerwanderung* of the early Christian Era shrinks to the level of a local upheaval when compared to the vast eruption of peoples that occurred between 1492 and the present day.

Not only were the post-Columbian readjustments of population on a scale hitherto unknown, but also the character of the movement and its origins were much more complex and varied than those of previous migrations. It is impossible in a limited space to do justice to the history of this migration, but some idea of its magnitude and ramification emerges when we recall that the period began in the Renaissance and reached its apogee in the modern industrial age. During much of this time revolutions in political, religious, social, and economic affairs in Europe were creating stresses and providing motivations for wholesale migrations. Moreover, the complexity of the phenomenon was greatly increased by the untold number of African natives added to this flow of humanity against their will, and by the numerous Chinese, Hindus, Japanese, and other Asiatics also drawn into the currents of population adjustment that followed on the increasing integration of the world.

So varied a movement cannot easily be

broken up into mutually exclusive categories or phases, but it is possible to discern in it two contrasting, although frequently overlapping, forces. The first and older was the conquering and exploiting aspect of European expansion. It involved, at most, relatively few men, and it began in southwestern Europe. The second factor was largely a colonizing and settling activity, which began later and finally affected an enormous number of people from all parts of the world, but especially from Europe.

The first of these aspects created the great empires and extended the political and economic control of Europe. It engulfed practically all Africa, it managed to exert control over a large part of Asia, it spread over the islands of the Pacific, and for a time at least held complete sovereignty over all the Americas. At one time or another 85 percent of the earth's surface lay in the hands of European powers.

The colonizing impulse was more restricted in its expression, finding its major resolution in the New World and in the more sparsely inhabited regions of the Old World that were suitable for European settlement.

The first Europeans to leave for the New World came from Spain and Portugal. Many of them were adventurers, but some remained to settle in these new-found lands as conquerors and landlords. Having established their primary base in the West Indies, they soon invaded the mainland, establishing their hegemony from California and what is now the southern United States to the tip of South America. All this vast region, except for Brazil, which fell to the Portuguese, became Spanish. The political control thus established had enormous consequences because it effectively sealed off the territory over which it exercised its sway from settlement by any other European power.

It is customary to contrast the Spanish and Portuguese attitude toward their colonial empire with that of the English by saying that the English set up permanent colonies and settled the family units in their American plantations, while the Spaniards and Portuguese were not colonizers but conquerors who came to the New World only to gather a fortune to take back home. Whatever truth there is in this cliché, it is not the whole truth. If we compare the English and Spanish rates of settlement we find that they were not so different as we have been led to believe. Rosseeuw St. Hilaire has estimated that during the first 150 years of the Spanish Empire in America 3 million Spaniards all told migrated to Latin America. Even if we scale this figure down, as Kuczynski believes should be done, it still remains comparable with the number of all immigrants (English, Dutch, Swedes, French, and Germans) who settled in the United States during its first 150 years. Even as late as the American Revolution the total population of the United States was only about 3 million, the large proportion of whom represented a growth by natural increase.

This showing of Spain is all the more remarkable when we recall that during the period of colonial greatness her home population had become static and already revealed evidence of exhaustion. England, on the other hand, was at the beginning of an extraordinary expansion of population and could easily spare of her excess. It is unfortunate that for this early period there are no abundant and reliable figures with which to contrast the growth of the European population in Anglo and Latin America, but we may obtain an approximation of the state of affairs by combining Kroeber's and Willcox's estimates for the century between 1650 and 1750. These indicate that while the English-speaking peoples were increasing by 1,260,000 the Spanish and Portuguese had expanded their number by 2½ million.

Although the Spaniards and Portuguese had by 1750 explored much of their empires, with the exception of the jungle interior of South America, they had elected to establish their principal settlements wherever the aboriginal population was heavily concentrated. Brazil was a notable exception to this rule, since it contained no great Indian civilization with massive populations. As a result the Portuguese remained along the coast, hardly penetrating into the heart of their vast territory. But the rich and populous centers of colonial Spain grew up in Peru and Mexico where native civilization with its attendant populations had reached their maximum growth. In Argentina, Chile, Paraguay, and Uruguay, on the con-

trary, where the Indians were few, the Spanish colonies were of lesser importance not only in wealth but also in numbers.

The coincidence of Spanish settlement with aboriginal patterns of population concentration has been attributed to a number of causes. The wealth of the Inca and Mexican Empires undoubtedly attracted many who sought the loot that the native chieftains possessed. But after the treasure had been dispersed the availability of a large population must have been effective in holding the conquerors who brought with them a tradition of a peasantry attached to a vast estate. The Indians provided the labor necessary for such a mode of life and indeed made it possible. Dr. Ruth Benedict has pointed out that it was not the size of the population specifically but the system of *corvée* labor, by which the overlord could command free service, that explains the concentration of Spaniards in these areas of ancient civilization. To these Indians living in agricultural civilizations, politically well knit, the whites merely replaced one exploiting class, with which they were already familiar, for another. The faces were different but the demands were the same. Consequently, the Indians accustomed to exploitation by native overlords accepted these conditions. The Indians who were free of such a caste system rejected the demands of the whites and were exterminated or pressed back into marginal and inhospitable territories. The absence of a large subject native population does not, however, completely explain the neglect of the southern half of South America during colonial times, since relatively large Spanish populations also settled in Colombia and Venezuela where with few exceptions the native population was scanty also. These, however, were areas that by comparison with Argentina, Chile, and Uruguay were more favorable for Spanish agricultural practices. And although it is true that the Spaniards in the Caribbean soon sought the expedient of introducing African slaves to replace the decimated Indians, those on the mainland never imported Negroes to the same degree.

It must be kept in mind that the number of Spaniards during this colonial period probably never exceeded the Indian population in any country and in most areas was much below it. As a result a mixed group of mestizos, part Spanish and part Indian, came into being, and by the end of the colonial period most of the Latin-American countries, with the exception of Brazil and the Caribbean Islands, consisted of varying proportions of Spaniards, mestizos, and Indians. It can be safely affirmed that at the beginning of the nineteenth century Latin America, with the above exceptions, had emerged from three centuries of Spanish domination as an area primarily of Indian and part Indian blood. The numbers and proportions of pure and mixed Indians varied. The number of unmixed Indians was high in Peru, Bolivia, and Guatemala, and the mestizos were relatively numerous in Colombia, Chile, Venezuela, and parts of Mexico. In absolute numbers the Indian population was most densely represented precisely in those areas where they had been most heavily concentrated in pre-Columbian times. There are, of course, exceptions to this generalization. One in particular has been cited frequently. This is the West Indies, which were more or less completely denuded of aboriginal inhabitants by the early Spaniards. The cogency, however, of this exception depends on whether we accept early or recent figures on the population of the West Indies. A sixteenth-century writer estimated a native population running into millions (and much has been made of the fact that the West Indies, once a great center of Indian population, now has fewer than any other Spanish or Portuguese settlement), but more recent authorities have scaled this figure down. Kroeber, for example, allows no more than 200,000 for the entire region in pre-Columbian times.

This balance of Indian, mestizo, and white populations might have continued down to the present time had it not been for the migration of the nineteenth century. The preceding three centuries had witnessed a very considerable movement of population from Europe to the New World. The exact number of migrants is unknown, but from indirect evidence we may estimate that hardly more than 5 million left Europe for various parts of America. Thus in one third of the time ten or eleven times as many people departed from Europe for America

as did previously. It is for this reason that the migration of the nineteenth century is known as the Great Migration. Not only in size but also in composition the nineteenth century movement differed from its predecessors. The contributors to the earlier migration had come almost exclusively from the Atlantic nations. Starting from southwestern Europe, Spain and Portugal were the first to embark on a serious program of overseas colonization. They were followed by France and soon after by England. The Dutch made an ambitious beginning but faltered, and the Swedes also attempted a settlement, an experiment which they abandoned. The sequence of these national ventures into colonization conforms roughly to the order in which the respective nations turned toward colonial empire building. It was no accident that the Atlantic fringe of Europe first became involved in these expansions overseas. The Great Migration of the nineteenth century, however, drew into its orbit peoples and nations that had previously been little concerned with this trans-Atlantic movement. Some Germans from the Rhine country had settled in Pennsylvania early in the eighteenth century, but it was not until well in the nineteenth that the vast movement of Germans to the New World began. Like a wave beginning in the west it moved across Germany eastward, drawing into itself recruits from farms and cities. The Irish famines of the 1840's started another large contingent toward America. In general, however, the sources of immigration moved eastward and southward, while the earlier reservoirs along the Atlantic became less significant. By the end of the century every nation in Europe was adding to the stream of people flowing to America. The major portion landed in the United States. From 1820 to 1935 about 35 million entered the States. It is difficult to estimate the total number that elected to try their chances in Latin America, but it must have been less than 20 million and may have been greater than 15 million. Argentina alone received over 6 million after 1857. These streams of migration, moreover, differed in composition as well as in size. The northern branch brought representatives of all the countries of Europe to the shores of the United States and to a lesser extent to Canada. The southern one, on the contrary, was fed mainly from Spain, Portugal, and Italy, with minor tributaries from Germany and Poland. But of more significance to our enquiry, the migrations of the nineteenth century did not affect all Latin America as did the earlier phase. The greater part of the southern migration went mainly to Argentina, Uruguay, and Brazil, with only minor diversions elsewhere. Here was a stretch of unexploited land thinly populated by Indians and mestizos and unsuitable for the *encomienda* system of great estates. It was, however, eminently suitable for the new migration which consisted of Europeans accustomed to work their own farms. As a result of this flood of settlers the Indians and mestizos of Argentina have been swamped and are now reduced to relative insignificance in a population that is about 98 percent of European origin. The Spanish contingent no longer exclusively represents the European strain. The Italians form approximately half the total population and in some areas are the dominant strain numerically. Similarly, Uruguay, which had remained undeveloped all through colonial times (its population as late as 1800 was little more than 30,000), became a receptor of migration. Its small population, largely mestizo, was engulfed by Spanish and Italian immigrants who were able to convert the land to agricultural and pastoral uses as world markets expanded during the nineteenth century. Thus its population today is largely of recent European origin, with the remnants of its former mestizo inhabitants relegated to a small minority.

Brazil also was one of the major depots of Latin American immigration during the nineteenth century. About 4,600,000 new immigrants have settled there, although not all have become permanent additions to the population. Italians and Portuguese constitute the great majority of them, with Spaniards, Germans, some Poles, and other Slavic people making up most of the remainder. The southern portion of Brazil, neglected by the Portuguese of colonial times, has been the seat of most of the modern settlement. By virtue of its agricultural possibilities, its cool climate, and its freedom from previous exploitation it has proved attractive to the

more recent immigrants, who now make up most of the population. The population farther north has been relatively undisturbed by the later-comers, maintaining the ethnic composition established during the colonial era.

The impact of the nineteenth-century migration from Europe has been felt also in various other countries of Latin America but to a lesser degree. Some countries, such as Peru, Bolivia, Ecuador, Paraguay, Colombia, Venezuela, and Mexico, to mention only the larger ones, have felt only lightly the influence of the newer migration. Chile, on the other hand, largely a mestizo nation with a small Spanish landowning population, has absorbed a small but significant group of new settlers. German, Scotch, and English immigrants in relatively small numbers have been added without, however, altering very profoundly the basic composition of the nation.

The third principal element in the population of Latin America is the Negro, whose major distribution is centered in the Caribbean and along the tropical coast line surrounding this sea. The presence of the Negro in this area may be traced to certain historical events and to the climatic tolerance which he possesses for warm humid regions. He first appeared in the New World in the Caribbean and in Brazil, the traditional points of entry into Latin America. In both places he was brought in to replace or to supersede the native population, whose accustomed mode of life was incompatible with the demands of the large estates set up by the Spaniards and Portuguese. In the West Indies the Spaniards, faced with an Indian population that was dwindling to the vanishing point, felt compelled to introduce African slaves to furnish the labor necessary for their plantations.

With the land divided into large holdings and the labor problem solved by a rapidly growing Negro population, the incentive for additional European recruits to settle in the West Indies in competition with wealthy landowners and slave labor was naturally absent. The typical pattern that developed during colonial days was a small landowning aristocracy mainly of Spanish origin, with an ever-increasing slave population almost entirely pure Negro. Cuba, rather exceptionally, preserved a relatively large European population, and Puerto Rico, as a result of some nineteenth-century immigration from Spain, has maintained an even balance between Negro and white. Elsewhere in the Caribbean Islands, however, the Negroes or mulattoes are the dominant population. Haiti, for example, is all Negroid. Jamaica is largely so. The smaller islands for the most part may also be classified as Negro or Negroid.

In Brazil the situation was somewhat the same. The first Portuguese settlements were made at Salvador in Bahia, where tobacco and later sugar plantations proved very remunerative and stimulated the spread of vast estates. Both at Salvador and at Recife flourishing *fazendas* created an urgent need for labor that could work under tropical conditions. The declining Indian population, never very large, was soon rapidly replaced by importations of Negroes. Mixture among all three stocks, Portuguese, Indian, and Negro, proceeded apace, but while the Indian contributors were declining in number and the Portuguese were being only slightly replenished from Europe the Negro contingent was being actively increased by large and steady importations. As a result the Negro element in much of northeastern Brazil became an important section of the population, not only as purebloods but also as an element in the hybrid populations. Not recognizing these ethnic differences officially, the censuses of Brazil give us no light on the number of Negroes and mixed Negroes in the present population. Unofficial estimates, however, place it conservatively around 20 percent of the total population. There seems little doubt that it was higher before the newer European settlers of the Great Migration poured into southern Brazil. The relative density of the Negro population increases as one goes north into the more tropical regions of the coastal belt.

At various other points along the tropical belt of South and Central America we find additional centers of Negro population. In Colombia, for example, a relatively large Negro population was introduced to work sugar plantations and is now distributed along the coastal lowlands and in the valleys where sugar cane is still grown. Ecuador

also, along its northern coastal strip, has become increasingly Negroid.

The dispersion of Negro population continues to follow the humid coast of the Caribbean into Central America, while the coast line on the Pacific side has remained largely Indian, mestizo, and European. This does not mean that Negroes are unsuited to the highlands or Pacific slopes. Rather it suggests that they are better able than the whites and Indians to adapt themselves to the humid tropical belt on the Atlantic side. For this reason we find Honduras, Nicaragua, Costa Rica, and Panama with relatively large Negro populations along their Caribbean and with few or none on their Pacific shores. El Salvador and Guatemala facing the Pacific have practically no Negro settlements.

Similar circumstances account for the large Negro populations in Surinam and British Guiana. The paucity of a native population, the tropical conditions, and the adaptability of the Negro have combined to favor his rapid increase in these regions.

I have thus far referred only in passing to other elements in the population of Latin America. There are a few such, but they are only of very local importance at present. For example, both the British and the Dutch have imported labor from India and Java to supply their needs in British Guiana and Surinam. As a result over 42 percent of the population of British Guiana and about 24 percent of Surinam is listed as East Indian, with 19 percent of Surinam of Javanese origin. The Japanese in the present century have made a slight contribution to the migration to Latin America. Most of the settlers of this origin have gone to Brazil, where about 175,000 were reported in 1934 with a smaller group of 21,000 established in Peru.

It is, I believe, evident from this summary description of a highly complex history that Latin America is far from homogeneous in her population. Not only do individual countries differ profoundly in their ethnic composition, but within each of them the diverse elements are by no means evenly distributed. To say, for example, that Peru is 46 percent pure Indian is misleading, since in certain highland districts the Indian population is close to 100 percent. To estimate Brazil's Negro population as 20 percent of the total hardly does justice to the much higher percentage found in the northeastern section of the country.

Although I am fully conscious of the difficulties of drawing generalizations out of this complicated pattern of ethnic integration and recognize the danger of oversimplifying an intricate situation, I think certain tendencies are apparent in the population process of Latin America. If I list them without tedious qualifications, I do not mean to imply that they are universally valid. Nor do I intend to imply that the factors involved are simple or even positively determined. In some instances we may see historical courses at work; in others, ecological, cultural, and climatic controls determining events.

1. In general the aboriginal population, the Indians, has survived in strength in the highland regions where they formerly had centers of well-organized agricultural people. The very size of their populations, running into millions, prevented their extinction, since their numbers were great enough to withstand the decimation resulting from Spanish contact and still leave a large enough nucleus with which to rebuild their strength. Indians have survived also in the forested interiors of the Tropics. Their numbers there today are relatively small, but they always have been. Their protection lay in the isolation which the jungle provided. Wherever they have come into contact with Europeans on the margins of their territory, they were quickly wiped out.

2. The areas of Latin America where the white population, Spanish in particular, may be found today in greatest numbers and purity are those which were relatively neglected by colonial immigration. Either because of the lack of a numerous native labor supply or because the territory was unsuited for the agricultural demands of these early times much of the southern part of South America lay fallow with only a scanty population of mestizos and whites. The tide of the Great Migration of the nineteenth century inundated in particular these regions and converted them to areas of dominant white population. Typical of this transformation are Argentina, Uruguay, and southern Brazil.

3. The intermixture of Indian and white

that began soon after the Conquest has given rise to a mestizo class, intermediate often between the Indians and the aristocratic Spanish landowners. Mestizos may represent anything from a scarcely diluted Indian to a near-white. Both extremes form hazy boundaries with the races they approximate. As an element in the population they are especially significant, since only through them may homogeneity be achieved. In colonial times they formed a large proportion of the population where the Indians were few in numbers or where the whites were especially numerous. The countries falling into this classification and also escaping the nineteenth-century migration are those that at present are distinguished by their proportionately large mestizo population. We may, for example, point to Chile, Paraguay, El Salvador, and parts of Mexico.

4. The Negro contribution is chiefly concentrated around the Caribbean and in various outliers where the climate is humid and tropical. In much of this area they are overwhelmingly in the majority, not only as pure Negroes but also as Negro–white and Negro–Indian mixtures. Their distribution has to a large measure been guided by two factors: the absence of a large and flourishing native population and their adaptability as labor in a region climatically unfavorable for white people.

5. The variations in these elements in the countries of Latin America offer us a clue to understanding their cultural history and their future development. It is no accident that a remarkable renaissance in native arts and crafts flourishes in Mexico and Peru, that Brazil has evolved dance and musical forms that owe much to Africa, that Djuka art emerges in Surinam, and that Argentina is mainly European in spirit. The composition of their respective peoples with their cultural heritages explains much.

THECA

The microscope reveals a wealth of cells
In corium and epidermal shield
Devised to test, resist, or yield
Or build anew as accident compels.

The melanotic touch in coat of tan
Completing range from sickly white to black,
The size and form of hair till age shall lack,
Alone suggest the source and state of man.

No microfeature tells of highs or lows
Of beast or saint, the joyous or forlorn.
The tegument but shelters each within.
How weak in biologic wisdom those
Who tempt the wrath of millions yet unborn
And judge a man's real worth by hue of skin.

—JOHN G. SINCLAIR

ON THE SOCIAL AND MORAL IMPLICATIONS OF SCIENCE

By JOSEPH SCHNEIDER
DEPARTMENT OF SOCIOLOGY, UNIVERSITY OF MINNESOTA

THE advancement of science in all its phases, supported by the appearance of the philosophical interest among men of science themselves, has established the view that the universe is neither an order nor is it moral. The diffusion of that attitude in recent years probably accounts for much of the gloom which is current concerning the place of science in the scheme of things to come, and, no doubt, for not a little of the resentment which many scientific workers show toward those who would show us where science is taking us, and how. Many a scientist is still so self-assured in his own accomplishments as scientist that it comes as a sharp jolt to him to be told that he can no longer take the values which sanction his work as in the nature of things, and, therefore, for granted; but that they are for him, as for all of us, man-made judgments about the relationship of men to one another and to the world.

Until well into the nineteenth century men believed that the world was an order, and that the discovery of this order would make men happier, better, or both. The world was conceived as a Universal Harmony, a kind of cosmic Utopia, the essential principles of which it was assumed could be empirically discovered and given theoretical formulation through mathematical reasoning. By that time Nature was no longer regarded as caring whether the secrets governing this harmony were discovered, but there was the belief that if the principles of that harmony were delineated, the threads of the good life would somehow be exposed. Evil was ignorance of the Universal Harmony. Man could banish evil through a knowledge of the objective world, cool and indifferent as that world had, since the Renaissance, come to be considered in relation to man's aspirations.

The experiences of the first half of the twentieth century—now nearly completed—have shattered what may have remained of nineteenth century scientific optimism. There are some among us who would assert that scientific knowledge has not enriched the social and moral life one iota. Without going that far, it may be confidently asserted that the simplest needs of men still remain unsatisfied. Adequate food, clothing, and shelter for everyone is still an unrealized aspiration in a world where population is shrinking relative to the improvements in techniques for getting food. In our own community we have done well enough in these respects by past standards. Not even during the depression years did hunger reach famine proportions. But the mockery of the ideal existed nevertheless. Hunger did occur, needlessly. There was the promise of abundance side by side with a paralysis of will. With plenty to eat everywhere there was an inability to obtain it.

More recently the world has been plunged into a vast contest of organized destruction. The very magnitude of the destruction is of the scientist's own making. It was he who developed the principles from which were born in the minds of the inventor and engineer the guns, the shells, the tanks, the ships, the planes, and the robot bombs which threaten to make a shambles of the world for years to come, and even destroy the laboratory itself. It was he who developed from his studies of collective behavior the techniques of mass regimentation for death everywhere so in evidence. It has not been enough for the man of science to discover that Nature is indifferent to our ideals and does not encourage them. He has done more. He has sharpened and trimmed Nature's claws, filed her fangs, trained her, and then unleashed her against all of us, including himself. And the future is even more terrifying in prospect than the present.

The recurrent protests of the scientist, each time he is challenged by things as they are, that he is the creator of a bright and

shiny new world by approximation and degrees, and not all at once, have not been received of late with kindly countenance. The present prospect is that no matter what his way of working may be, science may itself be the end of the human enterprise. The belief in social progress, of which science has nearly always been viewed as the efficient cause, has come to a low state in our time. Few people, after beholding the work of the demigod in the laboratory and the practical effects of that work outside, any longer believe that mankind is moving forward in the direction of a desirable goal. Even the hope that this might be so is rapidly waning. People are beginning to doubt the world view of science, and, what is more, science itself. Men have achieved longer life and better health, it is true. The prospect of freedom from toil and greater leisure has been before us for a long time, but for the millions it is still an aspiration. Better living for them is still a long way off. The good life for them is still obscured by the serfdom to the machine, the factory system, and antiquated systems of land tenure. Indeed, security, peace of mind, loyalty, friendship, kindliness, and the general attitudes associated with the brotherhood of man appear to be becoming less as science moves forward. Witness, for instance, the rising hate cult which has sprung up on every side. An individual who professes Christian motives is, at best, and still after two thousand years, an impractical dreamer.

The transition from primary to secondary group organization has been the achievement of science. The general result has been the creation of mass society: huge aggregations of people in close physical juxtaposition, possessed of such like-mindedness born of the anxieties and uncertainties of their lack of status as to be forever the prey of suggestion and crowd and mob behavior. The monotony of living and working by the clock and of having one's activities paced by the machine, has unbalanced men's emotions, frayed their nerves, and made them fit victims of fads, fashions, crazes, and manias. Extended and instantaneous communication, coupled with the developed techniques of opinion management, instead of drawing mankind together into a common society, have only heightened conflicts, aggravated class and race hatreds, and produced the phenomenon of leadership. The return of a dark age is not outside the limits of probability.

Altogether, the indirect management of the human enterprise by men of science has not fulfilled the promises which are imbedded in the idea of progress.

Had the scientist stayed with his observations on falling bodies, the contents of the test tube, the structure of the mind, and generalized group behavior processes, the impact of science on daily living might not have been so great. The world of the celebrated man in the street has remained strangely untouched by the principles of science while at the same time accepting the practical benefits which follow from them. T. C. Mits knows how to use the automobile, the telephone, but he seldom understands the operating principles of either. But the general method of the scientist proved so successful in these respects that he, in the guise of anthropologist and sociologist, attempted to apply it to the study of social order and moral values also. The impact of scientific progress upon conduct would have been great in any event, but the attempt to deal with moral experience through the use of the same techniques which had proved so rewarding in other endeavors introduced a self-consciousness about behavior which only further helped to disintegrate what little orderliness might have remained.

The rewards of less ignorance about how people come by their moral values were not the discovery of an absolute moral law, a moral law of universal application. The discovery which was made was that there is not one moral law valid for all times and places but many, and all of them workable. It was discovered that people living in different places have different moral customs. It was also discovered that the same people in different times have different moral customs. Furthermore, it was inferred that one set of moral customs is as "right" as another. The fact which emerged is that what *is* is right. Parricide is as right as giving grandfather

a pension while he nurses his "misery" and increasing decrepitude. Each custom has some social utility; it is serviceable in the maintenance of the group. Therefore, the sanction of utility which attaches itself to certain customs makes them right in practice. Parricide may be an adjustment to an inhospitable environment. Thus these sanctions are unconscious attempts to maintain the integrity of the group. All in all, it must be said that so far as the results of scientific inquiry go, it is simply a fact that what some people call right others call wrong. The ancient Greeks without the benefit of the techniques of modern science saw as much and drew the same conclusion.

The relativism here implied is an objective relativism. The individual's behavior is no less narrowly or loosely constricted in the kind of relativistic system here described than in one where moral values are assumed to have a supernatural sanction, are absolute. The individual is a victim of the gross value or cultural system into which he finds himself born; that is, if he ever achieves sufficient self-consciousness and awareness to know it. Most persons never make that discovery. There is an unintentional conspiracy in every culture against any individual who makes that discovery, or, better, allows such a discovery to influence his behavior. When that stage of self-consciousness is achieved the individual may become critical of what is acted upon in the group as right. He may begin to wonder what is right, and if he does so long enough it may dawn upon him that he himself is as good a sanction for what is right and wrong as the group into which he was born. In fact, to the extent that he is a consciously creative and reflective creature, which the group never is, he may conclude that right and wrong are products of personality. When this occurs the objective relativism becomes a subjective relativism: the individual becomes a measure of moral values. There may thus grow up a situation in a culture where individualism and moral anarchy prevail to the extent that social control is all but nullified.

The broad implications which the statements just made entail are in a way shocking. What they imply is that man can live in any kind of moral order, providing that the relationships which are defined as moral do not outright curtail the life processes and the transmission of the social heritage. The usages and habits of men can, in the end, have no other significance than this; and so neither can the moral sanctions and ethical codes that are derived from them.

Higher and lower as terms of evaluation of different ways of life achieve their meaning from the common ethnocentrism which characterizes all collective behavior. The processes of collective behavior tend not only to provide for the survival of the individual but also of the group itself, which is, after all, also a repository of nearly everything that the individual can become. It is probably this dependence of the individual upon the group that leads him to exalt the ways of his own group over those of individuals born into other groups. When this explanation for cultural preferences is accepted, about all that remains to be said is that different peoples show a wide range of knowledge concerning the objective world and adaptations to it in the way of inventions and discoveries, and that no knowledge or set of adaptations can do more than provide for the perpetuation of the group biologically. Therefore, to attribute to the habits and usages which prevail in a group superiority on other grounds than this is a kind of error of judgment. To attribute to the habits of the group into which one happens to be born evaluations of higher and lower is simply to make what one knows and does a standard of measure. Better still, it is making the accident of birth into an exercise of will, of choice. The slogans of patriots everywhere are of this order of behavior.

The scientist can find no support for the widely disseminated view that moral sanctions are of supernatural origin. In fact, the pretense that ethical behavior is supernatural in origin may itself be only a further aspect of the general situation of group living in which the individual finds himself. Usage and wont make things respectable and then sacred. Conventionality breeds fear: fear of the new, fear of changes. This fear may in time be metamorphosed into some-

thing other-worldly and then come back to the individual through the group as a sacred sanction for whatever it is that is done. But whatever the explanation for the frequent belief that morals have a supernatural basis, the scientist must simply reply that he knows of no such thing. The world of morals, he concludes, is one with the growth of tree; a series of events of naturalistic origin. Both morals and trees have a natural history.

The ascendency or victory of one cultural or moral system over another comes down, for the scientist, to a study of the processes of social change. Specifically, it is a study in cultural diffusion. He cannot argue, however, the proposition that "might makes right." He does not *qua* scientist know what right means. Further, if he is at all a discerning individual, he will be quick to detect that the phrase "might makes right" is a confusion of meanings. The rightness of an act or mode of behavior is something different from the strength or force necessary to impose an act or mode of behavior upon another person. In fact, to say that "might makes right" is the same as saying that there is no "right" at all. The appeal to force can only show who is the stronger in combat. The moral issue involved in either case is left undecided. The referent of right is always an ideal, a belief in a higher moral order. Thus the Christian-pacifist may be eternally right and yet be beaten out at every turn by things as they are. Therefore, while the scientist cannot traffic in such words as "right," he distinctly does not give aid and comfort to the theory of combat morality and its protagonists. Things as they are are facts, not values.

A recognition of the confusion of meanings in the phrase "might makes right" does not prevent the scientist from observing that one conquest group may, and often does, simply because of a preponderance of force, impose its moral system on the conquered. How successful a conqueror will be in imposing his system of cultural preferences on the vanquished depends upon a number of factors. Not the least of these, in modern times, is that conscious ethnocentrism called patriotic nationalism. Another is the comparative differences in the contending cultures. If these are too great, as in the instance where an advanced people invade the primitive world, the shock of the impact may actually exterminate the aboriginal peoples. On the other hand, where likenesses are great, as among moderns, an attempt to impose alien culture preferences may create a conflict situation which in time may be resolved in a process of fusion known as syncretism. The old and new usages blend as they become adapted to the larger sphere of interaction.

The point of the foregoing, however, is that while might and right are words from two different universes of discourse or levels of thought, it is true as a matter of fact that the survival of a moral system may depend upon a preponderance of force. Passive resistance, or negative force, may achieve similar ends. The goodness or badness of a system is not at stake in these interactional processes, excepting as these terms may be used to mean that the habits and usages which achieve ascendency possess greater group utility than those which are suppressed, i.e., they assure perpetuation of the race and transmission of the social heritage.

The scientist's attempt to treat morals scientifically really ends up without there being any good and evil, right and wrong. The use of such words by him is a convention, a manner of speaking. The attempt to provide a mechanical accounting for moral behavior, or what is so called by the nonscientist, is to end up with statements implying description and analysis of sequences of events in time and space. The scientist can give an account of what is called evaluation; that is, he can lay out the causal nexus for that kind of behavior, but he himself can do no more than admit that the "facts" in the chain are indifferent, without attributes of praise or blame. So far as his investigations go, he cannot distinguish between the self-sacrifice of a mother cat for her kittens and that of a martyr-saint for his God. The behavior mechanisms involved are similar, they differ only in quantitative complexity. Indeed, he may even aver that all kinds of behavior involving choices are of a kind with plant tropisms.

The scientist as scientist stands neutral with respect to the clash of wills and ideals which goes on about his ears all the time. His world is, if not cold, at least gray. Were he to admit to himself for one minute that this were not so, the great edifice called science with a capital S would come crashing down. Objectivity is not a fetish, a creed, a dogma of science: it is a condition. Without it there can be no knowledge of how things work. Admit bias or prejudice into an observation—intentionally—and the results are rubbish. But the scientist is also a man, a bundle of culture preferences and value judgments. Thus he will, at his country's bidding, perfect propaganda techniques so that his fellow nationals may be more readily led into battle, and at the same time, and with that great show of scientific impartiality and cosmopolitanism which has always been in the scientific tradition, publish his results in a professional journal available to his colleagues in an enemy country, who then proceed to apply the same propaganda techniques to the end that the carnage may be more ghastly and more complete. And then at the conclusion of hostilities these brother scientists may meet at some international conclave, compare notes on the successes of their respective efforts to do the bidding of their respective sovereigns, and deplore the collective stupidity of the human race.

What the scientist cannot do as a research worker, namely, admit values and preferences into his calculations excepting as data, he admits as a man. The criterion of relevance is for him, as for all of us, socially induced. The problems he works on reflect the ethos of time and place. The fact that moral values disintegrate under the methodology of science does not mean that there are none. It only means that science is not competent to deal with issues of right and wrong. The anthropologist or sociologist, after subjecting all evaluation to the corrosive effects of scientific analysis and announcing that all values are relative, is, perhaps, no less ethnocentric than the man in the street. And it goes without saying that the physical and biological scientist is nearly always so, and does not know it. In any event, nearly all scientists in wartime are patriotic nationals, and in peacetime generally in the service of the mighty.

The complaint that while scientists do not traffic in values as scientists, they do take for granted the existing value system, which defines and gives meaning to their researches, is one that cannot be easily evaded. The unconscious ethnocentrism which today receives the calculated support of scientists everywhere, without hardly a single exception, is a case in point. Such lack of awareness is a poor reflection of the humanism and cosmopolitan rationalism which was the main encouragement of the scientific enterprise in the beginning. There was a time when men of mind expressed only contempt for the vainglorious show of politics and war. But today the man of science has become a hireling, a willing subject in the service of the nation-state; an indefatigable combatant in the righteous cause of a finite warrior god.

On the other hand, we should be fair. Memory is short and time is long. Men of science begin their lives as children, as we all do. They too are victims of their times, their parents, their teachers. It shows little awareness of the nature of the social process to accuse a single group for the shortcomings of the human enterprise. The devil theory of social causation, even if it were true, falls short of the condition of control. The truth is, it was not until man's growing empire over nature, and the expanding world of interaction which came with it, clashed with the parallel tendencies, historically, of the factory system, territorial localism, and democratic totalitarianism that the loud warning was given to be on the watch for moral incompetency among scientists. The man of science cannot be held accountable for the tradition known as Western European Civilization. Others beside himself, including priests, soldiers, and merchants, had a hand in making it.

There is one aspect of the scientist's relationship to the social order which cannot be glossed over. It has admittedly been the self-imposed task of scientists to discover and invent the conditions of the desirable life. That was their campaign propaganda in the beginning and it still is. "Give man-

kind power over nature," the argument runs, "and the use of that power will not be abused." But it has been abused in more ways than one likes to recall. The checks they themselves appealed to, they have themselves also destroyed. No scientist pretends any longer to know what sound reason and true religion are. These spontaneous controls which Francis Bacon exalted in the *Novum Organum* to silence the doubters in his own time have gone the way of all myths. It is not stating the case against science too strongly to say that men of science have done more than they know to place the moral life in jeopardy. The humble people everywhere are today farther away than at any time during the nineteenth century from that cosmopolitan humanism which it was explained to them would be their reward if they followed in the wake of scientific research. The simple faith of the millions has been rewarded with more efficient weapons for consecrating tribalism, and better instruments for nurturing a meanness of spirit.

A weakness in the scientific approach to problems of social order from the first has been the faith that man's innate good sense will prevent the misuse of power over nature. The winning of power over nature has tended just for that reason to become an end in itself, and generated among many men of science the comfortable conviction that they share no moral responsibility in deciding to what uses their discoveries and inventions are put. To affirm the contrary in the presence of these men is to be accused of asperity and branded as a science-hater and a demagogue. The outcome of this state of affairs has been that the spirit of adventure in discovery has overshadowed all else in science, and has even become a general attitude. People have forgotten the reasons for the acquisition of material riches. Acquisition has become an end, not a means to better living. Rest days and holidays have actually become fewer with the rise and progress of science. A kind of blindness in this respect is afflicting all of us. A state of affairs is developing where people are beginning to be increasingly burdened with the situation of not knowing how to make intelligent disposition of the abundance of the factory and the farm on the one hand and a growing awareness of an exhaustion of the basic resources of the earth in the face of a declining population on the other. And piled on top of this dumb show is the paradox that only a few any longer affirm a joy in working.

The conclusion to which we are brought is that the winning of power over nature has not effected the good life. The tools of science are powerless to decide the good life. The results yielded up by science are empirical and relational. If we should wish to call a given social and moral order more desirable than another, the tools of science can conceivably provide the means for its attainment. Or they might be used to decide, conditionally, an issue involving a conflict of means respecting lesser as well as greater goals. The scientist can point out a relational agreement or conflict in empirical knowledge and an existing social and moral order. But man *qua* scientist cannot decide which is the better social and moral order. The man of science cannot even decide against those who would cut him off from the opportunities of being a scientist. He is powerless against both his enemies and his friends; from those who would destroy him as well as those who would crush him in their affectionate embrace. Whichever alternative confronts him he can only face the situation with the knowledge that the universe is a chaos, and that whatever order man finds in it is the outcome of his efforts to gain a living.

BIOLOGISTS IN THE NATIONAL ROSTER[1]

By ROGER C. SMITH[2]

PROFESSOR AND HEAD OF DEPARTMENT OF ENTOMOLOGY, KANSAS STATE COLLEGE

A STUDY of the registrants in the general field of Agricultural and Biological Sciences in the National Roster of Scientific and Specialized Personnel reveals certain facts that may be of interest. As indicated on page 287 of the SM for October 1945, the total registration in this field as of December 31, 1944, was slightly under 6 percent of the total Roster registration of 440,000, or about 26,000 individuals.

A representative sample of registrations in this field yields the distribution by principal professional fields given in Table 1. These

TABLE 1

THE PERCENTAGE DISTRIBUTION OF ROSTER REGISTRANTS IN AGRICULTURAL AND BIOLOGICAL SCIENCES BY PRINCIPAL PROFESSIONAL FIELDS

Forestry	17.4
Agronomy and Soil Science	9.5
Bacteriology and Immunology	9.0
Entomology	6.9
Zoology	6.4
Botany	5.2
Horticulture	5.0
Dairy Science	4.9
Plant Pathology	4.7
General Biology	4.3
Physiology	4.1
Animal and Poultry Husbandry	4.0
Medical Pathology	3.7
Anatomy	3.3
Wild Life Management	2.2
Genetics	2.1
Plant Pathology	2.0
Parasitology	1.7
Nutrition	1.7
Range Management	1.0
Wood Technology	0.9
Total	100.0

figures are based on the registrants' own statements as to the fields in which they considered themselves primarily qualified.

The median age in all these fields is 38.5 years, the lowest being General Biology, 35.5 (which probably includes a large proportion of senior students) and the highest, Medical Pathology, 46.9. The median age of all Roster registrants is 37.9 years.

Registration of individuals under 29 in the fields of Entomology, Plant Pathology, Plant Physiology, and Anatomy is relatively small in comparison with other fields. It is a question whether this is a coincidence or is due to some lack of employment opportunities in prewar years.

The number of women in Agricultural and Biological Sciences is relatively small, 92 for every 1,000 men. However, in Human Nutrition there are nearly twice as many women as men. In the fields of Botany, Bacteriology, General Biology, and Zoology there are from one-fourth to one-fifth as many women as men. Forestry, Range Management, Wood Technology, and Agronomy and Soil Science are at the bottom of the list with a ratio of 2 to 3 women to 1,000 men.

A tabulation of the industrial associations of registrants in the Agricultural and Biological Sciences is not only interesting but somewhat unpredictable. The data on which Table 2 is based were drawn from representative registration records excluding all who

TABLE 2

THE PERCENTAGE DISTRIBUTION OF ROSTER REGISTRANTS IN AGRICULTURAL AND BIOLOGICAL SCIENCES BY KINDS OF EMPLOYERS

Colleges and Universities	45.4
U. S. Department of Agriculture	17.1
Conservation of Resources	4.5
Hospitals and Clinics	3.0
Farms and Agricultural Services	2.2
Drugs and Insecticides	2.1
Dairy Products	2.0
U. S. Department of the Interior	1.9
State and Local Public Health Service	1.5
Lumber and Timber	1.1
Nonprofit Research Agencies	1.1
Secondary Schools	1.0
U. S. War Department	0.8
Libraries and Museums	0.8
Industrial Chemicals	0.7
Trade and Professional Associations	0.7
All Other Fields	14.1
Total	100.0

[1] Published by permission of the Director of the National Roster of Scientific and Specialized Personnel, War Manpower Commission.

[2] Former Professional Specialist for the Agricultural and Biological Sciences, National Roster of Scientific and Specialized Personnel.

were known to be in the Armed Forces and all students.

Since the figures in Table 2 are based entirely on Roster registrations, absence of a significant number of individuals in fields commonly regarded as open to those with biological training may mean only that the Roster does not have as broad a coverage in these fields as in the engineering and physical sciences. There are also a few fields in which biological and agricultural specialists would appear to be out of place. In some cases examination of individual records has shown that biological backgrounds were being definitely used in these fields. For example, wood technologists are useful both in aircraft and shipbuilding industries. In other cases biologists were working at tasks requiring no biological training simply because of lack of employment opportunity in industries where they would normally be. The larger part, however, of those included in the heading "All Other Fields" are definitely in work in which their previous training is being used.

Roster registration permits individuals to record their order of competency in several fields. It is interesting to note the apparent versatility of registrants in some of the Agricultural and Biological Sciences. For example, out of 320 individuals claiming professional knowledge of Plant Anatomy, only 86 gave it as their first field of competency, while 94 regarded it as their second field, 70 their third, 47 their fourth, and 23 their fifth.

In 1943, the National Roster conducted a survey to determine the personnel situation in Agricultural Sciences. In addition to material from other sources the Roster secured returns from 48 out of 53 state and territorial experiment stations and from 38 of the 51 state and territorial extension services. Also 63 out of 99 colleges giving training in agriculture reported on their personnel situation, including faculty, research staffs, graduates, and students. In all the above types of institutions the additional requirements were estimated as being 15.3 percent and losses to the Armed Forces between September 1, 1941, and July 15, 1943, were 25.2 percent based on employees as of August 15, 1943. In this survey it was found that 73.9 percent of the 1942–1943 graduates with the bachelor's degree had entered the Armed Forces; of those receiving the master's degree in that period only 34.1 percent had left civilian occupations and 8.3 percent of the small number of Ph.D.'s. Twenty fields were surveyed.

PLACEMENT SERVICE OF THE NATIONAL ROSTER

The National Roster of Scientific and Specialized Personnel is taking an active part in finding positions for returning veterans who are professionally qualified. Arrangements are being made with the Armed Forces to furnish veterans with return postcards indicating the field of specialization and type of employment wanted; and, in addition, separation centers are being provided with lists of positions available and application blanks in order that employers may be promptly informed regarding the training, experience, and qualifications of scientists and engineers who are returning to civilian life.

Many employers who have operated large emergency war plants are not able to retain their full staffs and a considerable number of fully qualified individuals are being released.

While there is undoubtedly a shortage of professional personnel, in a number of fields, it is a very large undertaking to bring the right jobs and the right people together. The Roster is prepared to cooperate with all agencies, industries, educational institutions, professional organizations, and members of the technical and scientific professions in order that the fullest possible employment may be secured.

The address is: National Roster of Scientific and Specialized Personnel, 1006 U Street, N.W., Washington 25, D. C.

THE NATURALIST'S DILEMMA

By ALEXANDER F. SKUTCH

At every epoch there lies, beyond the domain of what man knows, the domain of the unknown, in which faith has its dwelling.—AMIEL.

SPACE is the most obvious component of the universe—unlimited, unfillable space. But what is space? Next comes matter, which in its totality occupies but an infinitesimal fraction of space—not so large a proportion, perhaps, as that of the fishes and other animals of the oceans to the whole body of water. But who can explain the essential nature of matter? Of spirit and intellect we have no certain, scientifically demonstrable knowledge except in ourselves; and yet, with how much dross and base metal do we find them alloyed in man.

We seek unendingly for that which, by virtue of its affinity to our inner selves, we can understand—for that which is spiritual. It has ever been so. Primitive man gropingly sought the spiritual element among the stars, in mountains, rivers, rocks, trees, and animals. He invented a whole army of deities, great and small, to dwell in the heavens, with tritons to people the seas, nymphs the rivers, dryads and fauns the woodlands, gnomes the rocks and recesses of the earth. His quest for spirituality was not unalloyed with grossness and bestiality, as was to be expected from his own nature.

Even today the finding of a new star, containing some thousands of trillions of tons of matter, would not excite so much popular attention as the announcement of proof of the existence of the smallest sentient being upon some other planet. In this the minds of the mass of men follow a true instinct, which doubtless they would not be able to account for. We value things in proportion to their rareness and the difficulty of acquiring them. As the discovery of gold excites greater attention than the finding of yet another deposit of iron, so must proof of the existence of other spiritual or intellectual beings command more interest than the demonstration of still more matter. Yet if we could see truly, perhaps we should find spirit coextensive with space, and matter but one of its varied manifestations.

But of all the age-long quests of mankind none has been more baffling, none has yielded a richer harvest of heartbreak and disappointment, than the search for infallible proof of the spiritual element in the universe. Spirit and intellect are closely associated in ourselves: we feel and think, hope and plan, suffer and try to explain suffering, with the same organ. On the whole, intelligent men are more responsive than dullards to spiritual influences, such as beauty, nobility of character, loftiness of sentiment, and unselfish love. The relationship is not always simple and straightforward; in one man intellect seems dominant; in another, the spiritual qualities are the more obvious; yet the two somehow go hand in hand. Still—and this is the greatest and most exasperating of paradoxes—the intellect appears to be incapable of demonstrating to its own satisfaction the existence of spirit, or even of intelligence outside itself. We draw analogies and point probabilities and ask ourselves how otherwise can it be, but no man—begging Spinoza's pardon—has been able to demonstrate, by approved methods of logic or science or mathematics, the existence of a Supreme Spirit. In the ultimate analysis, we know or fail to know Him through the mysterious workings of the spiritual element in ourselves; and no crushing weight of arguments, pro or con, can make us effectively deny the verdict of our spirit. Thought and investigation have swept away many false and repulsive notions of the deity; but I doubt whether they have ever crushed, or ever implanted, the feeling of a supreme spiritual Being in any human breast.

The quest for the spiritual element reveals in the seeker the same limitations, the same insufficiency for the discovery of truth, the same ultimate doubts, whether he directs his attention to creation as a whole or to the smallest of created things. And is not this the outcome we might expect? For the spiri-

tual element in each of us, in every living creature, appears to be derived from the same great source of spirituality; and the demonstration of the smallest part of spirit is attended by the same difficulties as surround the proof of the existence of the Supreme Spirit from which it flows. And being subject to the same difficulties, it has the same importance; for the parts bear witness to the existence of the whole.

The inner, spiritual, or mental life—as we may choose to call it—of the living creatures which share the earth with us is one of the most fascinating, as it is one of the most important, objects of human curiosity. This bird that sings so sweetly in my shade tree, this dog that follows me so faithfully around, this horse so obedient to the touch of my rein, this squirrel chattering among the boughs, what does it think, what does it feel, what does it hope? Is its apparent affection anything deeper than eagerness for another meal, its love more than a brute appetite, its parental devotion more than the unconscious manifestation of a blind instinct? Does it in truth, as some have held, cry without pain, eat without pleasure, sing without joy?

These and a hundred similar questions constantly assail the thoughtful observer of animals. We need their answer with pressing urgency. We need it, first, because many of us must daily deal with animals in a practical way. How can we treat them with intelligence, justice, and humanity unless we understand what they really are? We call ourselves the lords of the earth; as such we wield power over all the rest of its inhabitants. But how can we be wise, moderate, and benign sovereigns, rather than arbitrary, merciless tyrants, unless we know the true and inward nature of our subjects? This, then, is the more immediate and practical importance of understanding the inner life of our fellow creatures on this globe. But the subject bears a still vaster significance by virtue of its connection with man's age-long, unending quest for the spiritual principle in the universe at large. The recognition of any smallest part of spirituality points toward the source whence it flows. Indeed, if we follow the inductive method of science, we shall strive to understand the parts before we attempt to grasp the magnitude of the whole.

The naturalist who occupies himself with the habits of living creatures in their native haunts ought, if anyone, to be able to answer some of these urgent questions, to tell us something of the inner life of the four-footed or feathered beings he so patiently watches. No other outcome of his studies—no description of the appearance, or details of the food, or enemies, or mode of reproduction, or length of life of an animal—can be quite so important as this. If he can answer our questions, we ought to honor the naturalist along with the greatest of philosophers and venerate him as a prophet.

But alas! your naturalist is only an ordinary mortal, subject to all the humors, whims, moods, prejudices, and limitations of knowledge and of insight to which his species as a whole is liable. And his problem, as we have seen, is the same in kind as that which has baffled the philosophers and seers of all ages—is, in fact, one aspect of this great problem. Hence, a revealing analogy arises between the metaphysical philosopher and the student of animal psychology. It is a common observation that a man's philosophy is colored by his temperament, the state of his health, and all his spiritual yearnings. Or, to express the same thought in other language, the riddle of the universe has been inscribed in such characters that a man reveals his whole personality in his attempt to decipher it. And, by an exactly similar law, a naturalist's interpretation of the behavior of animals tells you what he hopes to find in Nature. If he expects to discover in living beings only more complex examples of the operation of the laws of physics and chemistry, he will be a mechanist. If he seeks only material with which to experiment in a psychological laboratory, without taking into consideration the possible agonies of his subjects, he will be a behaviorist. If he prefers to devote his attention to the mystic manifestations of incomprehensible forces, he must perforce become a vitalist.

We derive from our studies what we put into them. If love and sympathy enter into our efforts to understand Nature, we shall see love and kindness in her children. If we

start out obsessed with the notion that Nature is red in tooth and claw, we shall find no end of fangs and talons dripping gore. If we devote to our investigations cold intelligence alone, we shall discover nothing but cool facts, upon which we base certain more or less exact generalizations. Our philosophy, our views of Nature, are inevitably tinged by our personal penchants.

It is commonly held that animals—the term being used to include the whole animal kingdom except ourselves—are guided in what they do largely by "instinct." This is a most unfortunate generalization. We have hastily pasted a label over the entire class of phenomena we strive to understand, and then find that we cannot read the inscription on the label. For since none of the things we do ourselves are instinctive actions according to the general acceptation of this term, we are cut off from the simplest and readiest—perhaps the only—means of understanding what an instinct really is. We have our reflexes, as when we start at a sudden loud noise, or blink our eyes, or grasp whatever is within reach to save ourselves from falling. But all our more complex activities, involving a whole series of simple muscular actions, have been taught us, or we have learned by reason and experience how to go about them. We build a house or a chair as we have been taught to make them, or as we are guided by reason or experience to improve upon the current models. A bird, we say, fashions its nest by instinct. No pattern of comparable complexity is included in the baggage we bring into the world with us. If it were, human education would be immensely simplified. If we could instinctively write, read, cipher, give the important dates of history, and name the parts of a flower, we might start years earlier to explore the unknown.

A bird builds her nest! I have watched a hundred kinds of birds build more nests than I can recall; I have counted their comings and goings and identified the materials they use. But the construction of the nest is still a mystery to me. Does the young bird, beginning her first nest, start out with a mental picture of the completed structure? She might, of course, have studied the nest in which she was hatched and fledged, made inward note of all its so various materials, and learned exactly how each was employed; but I think this would call for a degree of infantine observation far more astounding than any manifestation of "instinct." Moreover, the birds which construct the largest and most elaborate nests were raised in the dark interior of similar nests, where they could see but a small part of the whole edifice, and that but dimly in the poor illumination. I forget what naturalist it was who attempted to evade the difficulty of the first nest by supposing that a bird breeding for the first time always mates with an older partner who had previous experience in nest-building; but he must have known nothing of the habits of hummingbirds and many others, of which the female always builds without even the presence of a male.

If a young bird starts her first nest with a picture of the finished structure, I think it more probable that the picture is innate than that it is acquired in infancy. Does she know why she builds; does she foresee the eggs, the newly hatched nestlings, the fledglings in their first plumage? Or does all the marvelous process of rearing a family of young birds unfold before her stage by stage as, foreseeing nothing, she proceeds from step to step guided surely by "instinct," in much the same fashion as the plot of a new novel develops before us as we turn the pages of the book, or as our own life opens before us with the passage of the years? There are naturalists who claim that since the actions of the bird during the breeding cycle are "instinctive," it foresees nothing and so fashions its nest without foreknowledge of the purpose it will serve. These naturalists have treated us unkindly; they have withheld from us the sources of their information. Without access to their secret founts of knowledge, I am unable on my own account to answer the questions raised in this paragraph.

Instincts, as we commonly conceive them, savor strongly of the Innate Ideas of Plato. May it not be that a bird, before escaping from the shell, was equipped with an idea of a nest, and a migration route, and what

is and is not good to eat, and did not lose its knowledge upon hatching, as men forget when they are born? Had Plato been a bird-watcher, he would undoubtedly have found in the instincts of birds additional support for his theory of Innate Ideas.

But perhaps birds, like men, forget what they knew in an earlier existence, only not so completely. Birds may make blunders in building their nests, sometimes, for example, trying to place them in positions where they will never find adequate support, as in the angle between the tip of a leaf and the neighboring stem which it loosely touches. It is instructive to watch the building operations of colonial birds, such as the oropéndolas of tropical America, which may hang a hundred of their long, skillfully woven pouches from the terminal twigs of a single great tree standing in isolation from its neighbors. The wide-spreading crown offers an almost unlimited choice of twigs, suitable for the attachment of the nests; the slender vines, strips of palm and banana leaf, and other fibrous materials needed for their construction are equally accessible to all the birds. Yet, with equal facilities, some of the oropéndolas blunder along and take twice as long to complete their nests as their more adept sisters. Are these slow, blundering workers young birds building their first nests? Unfortunately, we have no means of distinguishing the old birds from the young. But these and many other observations for which we lack space lead me to suspect that instinct is not the infallible guide it is commonly held to be, that it must often be supplemented and perfected by experience, observation, and practice—in short, by learning.

No example is more commonly cited to prove the "blindness" of instinct than that of the pair of small birds who go on stuffing a young cuckoo with food, while their own proper nestlings, thrown out of the nest by the more powerful foster child, lie cold and starving on the ground in plain sight. It seems to me that the situation could as well be interpreted to illustrate the insufficiency of reason. Who knows but that the foster parents, like the Lacedaemonians of old, wish to raise only the most vigorous babies? Or maybe they are only making the best of a bad bargain; for the great majority of birds are unable to lift their offspring, and an unfeathered birdling that has fallen from the nest is irretrievably doomed.

Still, the habitual, routine activities of animals are attributed to instinct, and since we are not in a position to know the degree of consciousness that attends instinctive actions, their study scarcely helps us to understand the inner life of these animals. But we may attempt to further our quest by considering those exceptional actions of animals which appear to be guided by intelligence, because we know from personal experience something of the state of mind in which intelligent or rational activities are carried on. We know, for example, that we do not create any object—except accidentally, by the rarest chance—without beginning with at least a vague mental picture of the thing we desire to make; while we are not sure whether the bird starts out with an image of the nest in whose construction she is guided by instinct. I would not have it inferred from this that I hold reason to be "superior" or "more marvelous" than instinct. Reason appears superior for certain purposes and inferior for others. Both are equally wonderful, and in the ultimate analysis equally difficult to explain. If instinct has its deficiencies and blind spots, one has only to cast his eyes about the world of today to be convinced that reason also is subject to exceedingly grave limitations and aberrations; so that as a guide to action it is very doubtful whether it is more to be trusted than instinct. For instinct is almost always clear and sound as to its purposes, although likely to become confused in applying unfamiliar means to its ends, or in following its ends amid strange circumstances. Reason's greatest danger lies in its propensity to become confused, morbid, or self-destructive in its purposes, for it is less intimately in touch with the full, strong currents of life. I suppose whether one would prefer to lead his life by instinct or by reason depends largely upon whether he is conservative or radical at heart. The single point that I wish to emphasize here is that, since so many of our own activities are

classed as rational, we are in a better position to understand such of the activities of animals as we can place in this category than we are those which we attribute to instinct. How little we understand instinct may be gathered from the fact that we apply the term indifferently to creatures so diverse as, let us say, birds and insects. If zoological affinity is a guide to the inner life, as it is to the structure, of animals, then—although both birds and insects are said to be led by instinct, and men by reason—in their psychic life birds must nevertheless resemble men far more closely than they resemble ants and bees.

So, unhappily, in our quest to discover something of the inner or mental life of animals, we must turn our backs upon all those actions classed as instinctive, which include by far the greater part of the things we see them do. The supposed nature of instinctive behavior is the chief support of those who would see in animals only machines made up of flesh and blood rather than metal and wood, mechanisms that sing without pleasure and emit a cry without pain.

Since our endeavor is to discover the whole inner life of animals, which includes feeling as well as thought, we are free to consider expressions of emotion as well as manifestations of intelligence. But here again we run into grave difficulties. Birds, in particular, we believe to be emotional creatures; in them, as Julian Huxley has pointed out, "emotion, untrammelled by much reason, has the upper hand." Their expressions of emotion are vivid and varied, calling into play voice, wings, and often the whole body. Thus, mated birds of the most diverse kinds, upon becoming united after a temporary period of separation, greet each other with voice and quivering of wings, at times with a brief musical phrase reserved for this purpose, and often in the nesting season with complex ceremonies involving the presentation of nest material, and antics which remind us strongly of a caper of joy and a loving embrace. It is difficult for one who witnesses these greetings to resist the impression that they are the spontaneous effusion of genuine affection. But as we continue to watch birds we learn that in each species the ceremony of greeting follows a more or less fixed pattern. We may compare it with a salutation and a handshake or, if we will, an embrace and a kiss, which are conventional forms of greeting among men. But there are hand-grips in which we feel all the warmth of years of friendship; there are hands as responsive to our own as the handle of a broom; and again, there are hand-clasps in which we detect a deceitful substitute for the punch that time and place forbid. A third person might find it difficult to interpret the emotions of two strangers whom he watches as they shake hands. And even kisses bestowed upon relatives vary, as every child knows, from expressions of warm affection to feelingless compliance with parental dictates. If it is so difficult to interpret the emotional content of conventional acts of greeting among other creatures like ourselves, how can we hope to do so with any degree of confidence when the protagonists are birds so very different from ourselves?

Thus, emotional displays after a fixed pattern had best follow other instinctive forms of behavior, like nest-building and food-hunting, into the limbo of things which will not help us in our present quest. Since the conclusions we strive to reach will be of the utmost importance, we must be exceedingly cautious and discriminating in the selection and interpretation of our facts, even at the risk of inclining toward skepticism in our recoil from credulity. Already we have little left. Still, there remains a little. Birds are at times seen to do things in a way which does not appear to follow an instinctive pattern, but rather to be an individual, intelligent solution of a particular problem. Or they express emotion in a fashion which appears spontaneous and individual rather than conventional. Let us turn to these actions which, from being uncommon and rarely witnessed, seem to be the peculiar behavior of the individual rather than the instinctive behavior of the species. Let us see if they throw light upon the nature of a bird's mind.

Several species of yellow-breasted flycatchers, common and widespread in tropical America, build large nests with a domed roof that keeps the interior dry during heavy

rainfalls, and with a round doorway in the side facing out from the supporting tree. Usually the birds construct the commodious edifice entirely by their own efforts, beginning with the foundation of coarse, dry weed stems and ending with the soft lining upon which the eggs, and later the nestlings, will rest. But years ago I watched a chipsacheery flycatcher (*Myiozetetes similis*)—the most widely distributed of these yellow-breasted architects—that built her nest in a most individual fashion. Her site was a small lemon tree, where a pair of scarlet-rumped black tanagers (*Ramphocelus passerinii*) were feeding newly hatched nestlings, in an open, cup-shaped nest such as the great majority of tanagers build. Before the chipsacheery had proceeded far with her work, some mishap befell the tanager nestlings, leaving their nest vacant. Thereupon the flycatcher claimed the empty cup and began removing the straws and weed stems from her newly begun nest and placing them upon the tanager's nest. Soon she had converted the open, cuplike structure into the domed nest typical of her own kind. In this she laid three eggs and raised her brood, only to have them carried off by a hawk when nearly fledged. By suddenly changing her plans when her neighbors' nest was left vacant, and using it as a foundation for her own, she saved herself the labor of hunting and fetching as much material as the tanagers' nest contained.

"A bird who knows how to think and plan!" I exclaimed to myself at the time. The sudden shift in procedure, to take advantage of a newly found opportunity, seemed to indicate forethought. It seemed incredible that a bird could convert a nest of one type into another of distinct style, without a mental picture of the completed structure. "Blind" instinct seemed inadequate to achieve such results.

In subsequent years I have never again seen a chipsacheery flycatcher effect such a conversion. But twice I have known a closely related flycatcher, the graycap (*Myiozetetes granadensis*), to select an abandoned open nest as the foundation of its bulky roofed structure. The chipsacheery that took advantage of the tanagers' deserted nest was not quite so individual a genius as I at first supposed, for other members of her genus are capable of remodeling nests in the same fashion. What am I to conclude from my fuller knowledge? That these yellow-breasted flycatchers are all intelligent birds, or at least produce a considerable number of intelligent individuals? Or that here is an instinct which manifests itself only when the bird finds an empty nest which fills certain requisites as to size and position, and at the period when it is beginning to build? The reader will be able to answer these questions as well as I. But this much at least we have learned: that an act which at first appears unique and individual may, upon longer acquaintance with a group of birds, prove to be more widespread than we at first suspected.

Let me cite one other example that warns us to exercise caution in concluding that behavior is individual, this time touching a display of emotion rather than of intelligence. A female blue cotinga (*Cotinga amabilis*), clad in a modest speckled dress of brown and buff instead of the intense blue and purple of the male, had built her slight nest among the topmost boughs of a great, spreading *yos* tree standing in a pasture at the edge of the forest, in the Costa Rican mountains. Late one afternoon the nest was discovered by a blue-throated toucanet (*Aulacorhynchus caeruleogularis*), a small green toucan with a great bill and an inordinate appetite for other birds' eggs and nestlings. My attention was drawn to the nest and its assailant by the loud, agonized shrieking of the mother cotinga. Peering up into the many-branched crown, I beheld the toucanet standing on one of the uppermost boughs in a defensive attitude, great bill raised in menace, while the cotinga darted angrily past its head, uttering cries of distress. Soon the toucanet flew off through the forest, and the cotinga vanished amid the cloud of verdure that was the crown of the *yos* tree. But a little later I saw her go to the fork of a thick, mossy branch at the very top of the tree, nearly a hundred feet above the ground. Here, standing upon a cushion of moss which enveloped the crotch, she picked up a billful of fine twiglets and let them fall to the ground, thereby revealing to me the position of her nest, which hitherto I had sought in vain. She repeated this act

so full of melancholy significance, lingered a few moments on the side of the nest in an attitude that suggested forlorn resignation, then turned about and darted away through the foliage. Later, both she and I found her well-feathered nestling which had been knocked or frightened from the nest and rested on the ground in the pasture below, and did our best to take care of it until it could fly. Mother-love brought this tree-top bird to the ground.

The lifting up and dropping of the materials of the despoiled nest was a heartbroken gesture which, it would seem, could be of no possible use in the struggle for existence, and hence could have no "survival value" which might cause it to become a general habit of the species. It appeared to be an individual expression of melancholy resignation. Nests of these tree-top birds are not found every day; and it is difficult to know whether the behavior I witnessed was peculiar to this particular cotinga or widespread in her species. So far as I am aware, the only other nesting cotinga of this genus ever watched by an ornithologist was a female of a closely related species (*C. nattereri*), which on two successive years attempted to raise a brood in a lofty tree on Barro Colorado Island, where she was watched by the sympathetic eyes of Dr. Frank M. Chapman. After the nestlings prematurely vanished, he saw the bereaved mother pull her nest to pieces, much as my blue cotinga had done. But because a mode of expressing emotion proves to be widespread, are we to conclude that less of feeling enters into it? Do not most of our own manifestations of emotion follow more or less definite patterns?

Last year, on the rocky shore of a wild, rushing mountain stream, I found my long-sought first nest of the black phoebe (*Sayornis nigricans*), a familiar figure along swift inland waterways of the milder parts of the American Continent, from Oregon to Bolivia. The thick-walled cup, composed of gray mud strengthened by bits of dead vegetable matter, was plastered to the nearly vertical side of a ridge projecting from the lower face of a huge, outjutting rock. Here, with a stony roof above and the rushing current below, it seemed inaccessible to all enemies and in danger of destruction only by a sudden rise in the current. The female built with the encouragement but not the help of her mate. When she had lined the hollow of the nest with soft grass and a few feathers and had laid her two white eggs, I decided to watch her as she kept them warm. Ordinarily I should have concealed myself for such a study; but the rocky shore of the wide torrent offered no convenient screening bush or any spot for the erection of my little brown wigwam. So, perforce, I sat unconcealed upon a mossy rock hard by the dashing current, beneath the shade of a wide-spreading, orchid-laden *sotecaballo* tree, about twelve yards from the nest.

When the female phoebe flew down the river and saw me watching so near her nest, she was afraid to return to it—not so full of fear that she fled precipitately away, but with just that degree of uneasiness which left her uncertain whether it would be quite safe to sit on the eggs in my presence. In this undecided state of mind she stood on the low rocks projecting above the water below her nest, and after a while flew up as if to enter it, but when still a few inches away lost courage and dropped back to the low rocks. Several times she started for the nest but always changed her mind before reaching it. While she delayed in this vacillating state, her mate arrived and hovered in front of the nest, beneath the huge overhanging rock. His presence gave her confidence, and without further loss of time she darted up and settled on the nest in front of him. Then he dropped down to stand quietly for ten minutes on the rock below, while she sat steadily upon the eggs above him. After he had gone away she continued on the nest three minutes longer, then flew off to catch insects.

When she returned to the nest after a recess of six minutes she still lacked courage to settle down upon it in my presence, despite the fact that she had already sat for thirteen minutes while I watched her from exactly the same position. As last time, she flew up very close to it, only to drop back to the low rocks again. Again and again she repeated this move, sometimes actually touching the rim of the earthen cup, but always in the end re-

turned to the low rocks. She flew across the river, then in a few minutes came back and continued her false starts toward the nest. But she could not quite gather courage to rest upon it, and in the end flew upstream to seek her mate.

Soon she was back again with the male, who went direct to the nest and perched upon the rim, facing inward. Now at last the female phoebe went to the nest without hesitation and settled on the eggs in front of him. He lingered beside her a minute, then dropped down to rest on the rocks below a minute or two longer. Then he flew away.

Through the remainder of my five-hour watch, the female phoebe would never enter her nest unless her mate were close by. Sometimes he would accompany her as she returned from her recess, and then she would promptly settle down to warm her eggs. At other times she would come alone and, standing on the rocks below the nest, call *chip* until he arrived to see her safely on it. Once, while he perched on the rocks on the opposite side of the river, catching insects above the water, she waited ten whole minutes for his arrival, meanwhile catching insects herself by darting out from the low rocks beneath the nest. Or, becoming impatient, she would go off in search of her mate, and finally bring him back with her.

The male phoebe would sometimes stand on the nest's rim, and then the female would go promptly to sit in it. Again, he would flutter momentarily in front of the nest, and this had the same result. Yet again, he would not go to the nest, but merely stand on the rock below. Then his mate would usually make a few false starts before she actually settled down. The male would always linger a while on the rock below her, for less than a minute to as much as ten.

This interesting conduct, unique in all my experience with birds, raised an important question. Was I witnessing the normal, unconstrained behavior of this pair—the way they would have acted if they could not have seen me; or was it that the female phoebe would not enter her nest in the absence of her consort because she lacked confidence in my presence and needed him to bolster up her courage?

To gather evidence upon this point, I returned in the afternoon and sat upon a more distant rock, now about fifty feet from the nest, but still with no intervening vegetation to screen me. Soon the bird returned alone, and seeing me there, hesitated a while and then flew off upriver, doubtless to seek her mate. But as he was not inclined to come just then she was obliged to come back without him; and again she stood on the rock beneath the nest, starting toward it as if to enter but lacking courage to do so. Another journey upstream, and a third solitary return to the nest; and now, since she could not prevail upon her usually so attentive mate to escort her, she had no recourse but to settle down on her neglected eggs in his absence.

During four hours of the following morning I watched the nest from the more distant rock. Now the female phoebe, becoming reconciled to my presence at the greater distance, settled upon her nest alone, with little or no hesitation. Of twelve returns to the nest after as many recesses, on nine she came unaccompanied; only three times, or at one-quarter of her returns, was her mate present. This may be taken as about the normal rate of occurrence of his pretty custom of seeing her on the nest—a custom practiced, with many variations, by a considerable number of monogamous birds of which the male does not incubate. He was present at every single return on the previous morning only because in his absence his consort would not settle upon her eggs. But later, when she had grown quite accustomed to my presence, she would enter the nest alone even while I sat only seven feet away. She gradually lost her distrust of me.

The behavior of this phoebe, which in the presence of a potential enemy would not go to her eggs unless her mate were close at hand, opens fascinating glimpses into the psyche of a bird—glimpses which, unfortunately, suggest more than they prove. It is for this reason that I have dwelt upon her conduct in such detail. I doubt whether her refusal to enter her nest, when afraid, unless her mate were close at hand, was instinctive in the sense that it followed a hereditary pattern. Only the study of several nests,

under similar circumstances, could settle this point. But certainly the conduct of this pair was not purposive in the strictly utilitarian sense; it had no "survival value." When an enemy too big or powerful to be driven away approaches the nest, birds do well to slink away in silence, on the chance that it will escape detection, not call their mates, for the presence of two attracts more notice than one alone would. But certainly the actions of this female phoebe were very humanlike. As all of us, she found courage, in the presence of one she loved and trusted, to do what she feared to do alone. And even, it seemed to me, toward the end she was taking advantage of the situation to win additional attention from her mate, coquettishly feigning a distrust she no longer felt, until at last she learned it would avail her no longer.

One aspect of the phoebe's behavior, her many starts toward the nest when her mate was not at hand, each falling short of it and ending in a retreat, is very widespread among birds under similar circumstances. Their attachment to the nest draws them onward, and their fear of a man or a strange object in its vicinity urges them in the opposite direction, and so they oscillate forward and away, until at length either parental devotion or prudence gains the upper hand, and they settle in the nest or leave it to its fate. How different is this vacillating conduct from the direct, straightforward action of the insect, which appears to be influenced by but a single motive at a time. This conflict of impulses in the avian mind closely resembles our own attacks of indecision. As everyone has experienced in himself, our consciousness is heightened by such stress of opposing emotions. The things we do and say in such circumstances are graven deeply upon the memory, while habitual actions frequently make so little impression that we cannot say an hour later whether we have done them. Who has not amused or vexed himself by continuing to search through his room for an object that he has mechanically slipped into his pocket? It is from this consideration that I believe we must look to the uncommon rather than the customary activities of animals, and to their conduct while beset by conflicting emotions, to throw light upon the dark and difficult subject of their psychic life.

A little, brownish, bare-cheeked Costa Rican Tityra (*Tityra semifasciata*)—a widespread member of the cotinga family—had built her nest in an abandoned woodpecker hole near the top of a tall dead trunk standing in a clearing in the forest. For her, nest-making was a simple process, consisting merely in the accumulation of a loose litter of small bits of dead leaf and dry twiglets in the bottom of the hole. A larger woodpecker's hole, only a few feet below that chosen by the Tityra, was the dormitory of four Frantzius's araçaris (*Pteroglossus frantzii*), a species of toucan much bigger than she, with huge red bills half as long as her whole body. The Tityra strongly disapproved of the presence of these inconvenient neighbors, which were probably not above making a meal of her eggs or nestlings; and each evening she, and sometimes also her mate, would pursue and dart at them as they flew to their high bedroom. But the great-billed birds took slight notice of the anger of creatures so mild-tempered as the Tityras. Finally, night closing down, the white male Tityra would fly off to sleep in the forest, leaving his mate alone in the clearing.

Left to herself, the female Tityra would rest upon the lofty top of the trunk that contained her nest and the araçaris' dormitory, or upon some neighboring eminence. Then, after a while, she would fly toward her doorway, but as she came close to it—and to the doorway of the araçaris whom she feared—she would lose courage and return to her high perch. Here she remained motionless, a very solitary and diminutive figure silhouetted darkly against the fading light above the western ridge, while all the bird world sank into hushed repose. What a struggle must have been taking place in her small breast as she rested, outwardly so quiescent, against the great, open, cloud-veiled sky! On the one hand, duty called her to pass the night in the nest, which had been completed and perhaps already contained the two mottled gray eggs. But just below it slept larger and more powerful birds that she feared, and she scarcely dared to slumber

so close to them—even if several feet of wood did separate the two cavities. Her thoughts must have turned with longing toward the sheltering tree where she was in the habit of roosting with her mate, in the forest far from these red-billed monsters.

At length, when even the late-retiring motmots and woodhewers had sought their sleeping places, and the bats had begun to flit through the dusk, the lonesome little bird made another move toward the round entrance of the hole, now scarcely visible in the darkening trunk. But in front of it her courage failed, she hovered on fluttering wings, then rose to rest once again atop the trunk. Five or six times more she started for her nest with the same negative result. At last, she winged off toward the forest to the south, whither she and her mate had gone together on previous evenings, and vanished amid the dusky foliage. She had not been brave enough to pass the night in her nest so close to the araçaris. But a few evenings later, with growing parental devotion, she found courage to enter the nest and sleep upon her eggs so near the dreaded toucans.

I believe it was Herbert Spencer who suggested that consciousness was kindled by the friction of conflicting impulses within creatures motivated by instincts. As she perched motionless against the darkening sky, the Tityra was without any doubt torn between opposing impulses. As I watched her from the pathway at the edge of the forest, sympathizing with her agitated state of mind, I found it hard to believe that she was not acutely conscious of her situation.

I MIGHT go on indefinitely to cite observations, made by others or myself, which seem to throw light upon the inner life of birds, to prove that they possess at least a modicum of reason, a high degree of memory, a dash of foresight, and such spiritual qualities as affection for their mates and young, devotion, courage, a feeling for beauty, a sense of play, anger, rage, and other attributes. But these examples will serve our purpose, since we are not here attempting to make an exhaustive exploration of a bird's mind but rather to understand the conditions under which such exploration is possible. I am well aware that clever students of animal behavior have devised experiments which make birds appear exceedingly foolish, their mental processes singularly blind. Yet even the older, less scientific literature is full of instances of the insufficiency of a bird's intelligence to meet all the contingencies of its life. But what of it? The books which deal with ourselves, our history and biography and fiction, contain far more numerous and far more ludicrous instances of the insufficiency of the human intellect.

Perhaps others who have watched intimately the lives of wild creatures have discovered, like myself, that every episode somewhat out of the ordinary which we are privileged to witness produces by turns two contrasting states of mind. There is the moment of sympathetic insight, when we enter—or fancy that we enter—into the feelings of the creature that we watch, and hardly entertain a doubt as to the motives which drive it to act, the emotions it experiences. Such intimate communion with the spirit of a creature so different from ourselves produces a warm glow that endures for a longer or briefer spell but sooner or later must cool, leaving only a memory. This state of exalted feeling is followed by what we may call the intellectual reaction, when reason sets to work, with all its upsetting questions and cautious doubts, to pluck apart and demolish the beautiful image which sympathy so readily created. Can we really know anything of the inner life of creatures so different from ourselves? Can we not explain all that we have seen on the basis of instincts, tropisms, behavioristic patterns, without calling consciousness into the account? Are birds, or other animals, actually capable of thinking, of loving dispassionately, of responding to beauty? These and a hundred more disturbing queries rise into our minds.

Hence comes the naturalist's great dilemma. But it is not a dilemma confined to naturalists alone. Every man not all spirit or all intellect—and doubtless there are few such—must experience it if he tries to pierce through the opaque mask of matter and reach the things that lie behind and beyond. The more nearly what we may distinguish briefly as the intellectual and the spiritual qualities

are balanced in him, the more bothersome this dilemma will be; if either of these qualities, spirit or intellect, has definitely the ascendency, it will trouble him the less in proportion to the degree of dominance of either. That man is indeed to be pitied who has not at least once or twice in a lifetime known moments of insight when great cosmic spiritual truths stood sharply and clearly before him; he is more than ordinarily fortunate if he has not at a later date focused the cold light of reason upon his revelation and come to doubt its validity. Such, on a smaller scale, is the common experience of the naturalist who would enter into the inner life of the creatures he studies, not merely record external details of behavior. We said at the outset that the quest for spirit was attended by the same difficulties, whether it be sought in the universe as a whole or in some humble creature of our fields or woodland. And, being subject to the same difficulties, it has the same importance, for the parts give evidence of the whole.

How, then, can we escape from between the horns of our dilemma? We might adopt an attitude of skepticism—which would do very well if we never faced the necessity for action. Aside from this negative middle course, whichever way we incline has its dangers. If we yield to our glimpses of sympathetic insight and give credence to them, we risk falling into scientific error. If we refuse to believe what cannot be demonstrated in a laboratory, we risk closing our eyes and ears to a whole section of reality, which, if recognized, would give depth and breadth of meaning, not only to our contacts with this or that living creature, but to life as a whole. If we elect this second course, we indeed cut ourselves off from the possibility of understanding aught of the psychic life of other creatures. Huxley long ago pointed out that the strict methods of science are inadequate to demonstrate the conscious processes of any human being except our individual selves. How, then, can we hope that they will reveal the inner life of members of other species?

So it narrows down to a question of whether we shall have faith in our flashes of insight or, denying them, cut ourselves completely off from all possibility of recognizing the spiritual element in the lives of other beings. For although spirit and thought appear to have their seat in the same organ, yet, strangely enough, only spirit can bear witness to spirit, and thought alone to thought. We may understand the inner life of another being only insofar as it bears affinity to our own inner life; only through sympathy can we enter into it. The naturalist, as we have seen, must inevitably be influenced by his own temperament and his own aspirations in the interpretation of the inner life of the creatures he studies, just as the philosopher is swayed by his own personality in erecting his system of the cosmos. For myself, each year I incline more strongly to put confidence in those rare flashes of sympathetic understanding that seem to penetrate the outer husk of a bird and reveal the life within; I believe that they are more likely to disclose the truth than those laborious analyses of behavior by which we attempt to discredit them. If I must incur the risk of error, I prefer to incur it with the gateways to the spirit open rather than shut. As in all questions transcending practical demonstration, each of us must make his own choice at his own peril. And every choice, if it avoids the hideous form of dogma, is entitled to respectful consideration.

UNCERTAINTY

The world is my idea. Well I know
That things exist because I think them so.
But see the impasse where such reasoning ends:
Whose is the mind on which my mind depends?

—Clarence R. Wylie, Jr.

OUR EVERYDAY RECKONINGS

By OYSTEIN ORE

PROFESSOR OF MATHEMATICS, YALE UNIVERSITY

AMONG the movie gags there are a few old stand-bys which are being renovated from time to time and which do not seem to lose their popular favor. One of them presents the inexperienced American who, when arriving in England, gets involved in the most awkward troubles in calculating prices and his change, while a kindly Englishman stands by explaining how in reality nothing could be simpler than a monetary system based on pounds, guineas, florins, crowns, half-crowns, shillings, pence, and even farthings. The audience leans back after it is all over with a satisfied feeling of how much simpler and better all this has been taken care of in the progressive U.S.A. with its ideal dollars and cents.

Of course, there have been and will be attempts to simplify the English system of coinage; societies have been organized for this purpose and bills have been submitted to Parliament. However, all attempts have run up against a wall of opposition to any changes in the traditional system, with some sound reasons behind certain objections but also with arguments at times bordering on the ridiculous. To give a sample, let us quote a few remarks made in a speech by Mr. Lowe, member for Kidderminster, in the debate on decimal coinage in July 1855, quoted with some indignation by de Morgan in his Budget of Paradoxes:

> From a florin they get $2\frac{2}{5}$ths of a penny, but who ever bought anything, who ever reckoned or wished to reckon in such a coin as that (Hear, hear).
>
> Such a coin could never come into general circulation because it represents nothing which corresponds to any of the wants of the people.
>
> What would be the present expression for fourpence? Why, .0166 (a laugh); for threepence? .0125; for a penny? .004166 . . . and so on ad finitum (a laugh) for a half-penny? .0020833 . . . ad infinitum (a laugh). What would be the present expression for a farthing? Why, .0010416 ad infinitum (a laugh). And this was the system which was to cause such a saving of figures, and these were the quantities into which the poor would have to reduce the current coin of the realm. (Cheers.) With every respect for decimal fractions, of which he boasted no profound knowledge, he doubted whether the poor were equal to mental arithmetic of this kind (hear, hear), and he hoped the adoption of the system would be deferred until there were some proof that they would be able to understand it; for, after all, this was the question of the poor, and the whole weight of the change would fall upon them. Let the rich by all means have permission to perplex themselves by any division of a pound they pleased; but do not let them, by any experiment like this, impose difficulties upon the poor and compel men to carry ready-reckoners in their pockets to give them all these fractional quantities.

The comments upon the reasonings of politicians are gratuitous. Yet, consciously or unconsciously, there are few of us who can feel ourselves free, at least occasionally, from substituting feelings for reasoning when it comes to finding arguments against changes in our old and adopted and accepted ways.

The reforms in our daily arithmetics, in weight and measure, in time and money, in counts and calculations seem to be closely associated with the great upheavals of history. Certainly, there is always a movement for reform and improvement even in ordinary times, but when life flows calmly human inertia is greater, small objections become magnified, and vested interests are more difficult to dislodge.

Wars and revolutions bring systems in contact with other systems. They must compete, be compared, and made compatible. Crises are apt to expose their inherent weaknesses. Besides, the revolutionary days in the history of mankind seem to make us all more susceptible to new ideas; a reasonable innovation is no longer an event of surpassing importance. They may even create a desire for change for change's sake, a feeling of relief in severing the ties with the past.

There is every reason to believe that the recent all-embracing war will bring us novel methods even in our daily commerce. Naturally, there will be no lack of proposals, some good, some bad, and some just plain crazy. There will be some which are reason-

able and advantageous, yet not too strange in comparison with our customary ways—in short, measures which stand a good chance of acceptance. Others may be systematically good, yet their advantages may be too small in comparison with the changes they entail. Finally, there will be an overwhelming number of proposals without any advantages whatsoever.

No one who belongs to the mathematical profession can avoid being asked from time to time to pass judgment upon a variety of arithmetical and metrological projects. It is perhaps a little surprising that one of the most common proposals from lay sources is the abolition of our old and comfortable decadic number system. Instead of counting by powers of 10, i.e., 1, 10, 100, 1,000, etc., some other basis number should be selected for this favored position. Some would give preference to 6 because the multiplication table would be so easy; others would count by 12 or dozens, since then it would be so simple to divide into 2, 3, or 4 parts; while still others would proceed by doubling 1, 2, 4, 8, etc. There are many claims for these proposals, and the mathematician is ready to admit that other number systems may occasionally be useful. However, there is no reason to believe that we shall ever be induced to give up our natural counters, the ten fingers. Our human relatives all over the globe count by tens to such an extent that other systems are quite exceptional. Some peoples count by scores; as a notable example let us recall the Mayans, and traces of such a vigesimal system are still to be found in the names of the numbers in many languages, as, for instance, in French and particularly in Danish. The twenty system is easily explained when one takes the toes to aid in counting. The dozen system may perhaps come in when a full fist is used to denote the number 6, so that two of them would be 12. Much more speculative are the theories concerning the origin of the Babylonian sexagesimal or sixty system, which is still with us in the division of our time as well as angles into minutes and seconds. The dyadic system with its repeated doubles or halves occurs in many instances in our weights and measures, or, to touch upon an illustration nearer the interest of some men, in the fractional quotations in the daily stock-market report. Such systems arise quite naturally in the search for the simplest division or multiplication procedures.

The transition from the Roman numerals to the Indian or Arabic numbers, as we still use them, is likely to remain the last real revolution in our arithmetic. The Roman numerals were most unsuited for extensive calculations, and an abacus, or reckoning board, was a practical necessity. Western Europe was brought in contact with the Indian "dust numbers" as used by the Arabs about A.D. 1100, largely through the influence of the Crusades. When compared with the Roman numerals they were so obviously superior in calculations that they won particular favor with the merchants and tradesmen who were instrumental in their spread all over Europe. As in the case of all reforms, there was considerable conservative opposition, and it took two centuries before the abacists, or Roman numeral adherents, had surrendered all main strongholds to the algorithmists, or followers of Mohammed ibn Musa Al-Khuwarizmi, the Arab mathematician whose works were the main sources on the new system. Not until the second half of the fifteenth century does one find Arabic numerals regularly on coins.

It is possible to predict that there will be two reform proposals which will be viewed with considerable favor by the world organization which will eventually be created to further international cooperation and coordination. The first of these is the perennial project of the universal use of the metric system, the second is the question of calendar reform.

The metric system has always been a scientist's system, in conception, in execution, and in use. The membership of the commission charged by the French National Assembly and by the French Academy with the theoretical and practical creation of the new system represented the cream of European science at the time of the French Revolution: Borda, Brisson, Condorcet, Coulomb, Delambre, Haüy, Lagrange, Laplace, Lavoisier, Méchain, Meusnier, Monge, Vandermonde. Lavoisier during his detention, shortly be-

fore his execution, asked in vain to be permitted to continue his work on the metric system under armed guard. The surge of science in the seventeenth and eighteenth century had demonstrated the desirability of having some common standard of measure. Instead of accepting some arbitrarily selected man-made unit, the scientific spirit craved a basis anchored in some invariable way in nature. There were at this time two main rivaling procedures for such a natural system, both actually used for this purpose, one based on the dimensions of the earth, the other derived from the length of a pendulum with 1 second's vibration. Nowadays there would probably be other suggestions, as, for instance, a standard based upon the wave length of some spectral line, or perhaps, in deference to the theory of relativity, a unit measure derived from the distance which a light ray travels in a second. Both the pendulum and the global measure were under serious consideration for the basic measure by the French Commission, but as everyone knows, the meter was finally fixed to be the ten-millionth part of a meridian from the Pole to the Equator.

This definition in itself is not the principal point in the metric system; the essential simplifications lie in the derivation of all other weights and measures from this single unit, the consequent decimal division and the systematic nomenclature. We may be a bit bewildered by the British coins, but this is a minor complication in comparison with the confusion experienced by anyone brought up under the metric system when confronted with our unsystematic divisions and extravagant array of units: inch, foot, rod, furlong, mile, hand, fathom, link, chain, pole, chord, perch, gill, pint, quart, gallon, hogshead, barrel, cask, peck, bushel (heaped or struck), troy and avoirdupois weights, grain, pennyweight, ounce, pound, quarter, hundredweight, ton, scruple, drachm, minim, stone, acre, and so on.

There was a strong desire for a scientific measuring system at the time of the French Revolution, but from a practical point of view the need now, in retrospect, appears to have been overwhelming. The standards of measure varied from province to province and from town to town in France as well as in most parts of Europe. For instance, according to one treatise there were in Switzerland 37 different feet, 68 different ells, 83 measures for grain, 70 fluid measures, and 63 weights. The French administration complained about the complications and obstacles created in the distribution of grain due to difference in standards and urged the adoption of a provisional meter before the final, more exact calculations could be completed. In addition, to the revolutionary spirit the old terms "Royal acre" or "King's foot" were clearly unworthy and obnoxious reminders of the past.

No reform, however desirable, is complete without its opposition. Even the judicious Montesquieu had complained previously: Is the pain of change always preferable to suffering the existing? He voices his sarcasm: There are certain ideas of uniformity which sometimes affect the great minds and invariably catch the smaller ones. It took nearly fifty years before the metric system was entirely victorious in France and a century before it became universal in continental Europe. In this century of expansion the metric system had to surrender one of its fundamental philosophical claims: to be a system anchored invariably in nature, not dependent on the arbitrariness of human choice. As time went on more accurate determinations of the lengths of the meridians could be made. The question arose here, as in principle for all natural units, whether the meter should be changed with each increase in the accuracy of measurement. It was decided that the answer should be no, the meter and kilogram should remain determined by the prototypes which one must hope are still deposited in the International Bureau of Weights and Measures at Sèvres near Paris. For practical measurements this agreement was, of course, without consequences.

Washington and particularly Jefferson were interested in the metric system and repeatedly urged the adoption of a systematized set of weights and measures for the United States. Jefferson had, however, a decided preference for a unit of length based on the pendulum, and this may in part ex-

plain why he did not follow up more vigorously his report of 1790 on a decimalized system. John Quincy Adams in his report to Congress in 1821 pays the following glowing tribute to the metric system: "This system approaches to the ideal perfection of uniformity applied to weights and measures, and whether destined to succeed or doomed to fail, will shed unfading glory upon the age in which it was conceived, and upon the nation by which its execution was attempted and has in part been achieved."

Despite such support of prominent men and influential groups the metric system, as we all know, is not in extensive popular use in this country, and it even appears doubtful whether any compulsory legislation stands any great chance of being passed within the next few years. Yet at present the metric system is legally approved, and, in fact, our standard units of measure and weight are based upon the metric prototypes, and there are indications that entering wedges are now being created for the metric system, particularly through the urgent demand for international coordination in many fields.

It appears to be easier to arrive at agreements on universal standards when international problems are involved. Local prejudices have less influence in such cases, and the need is often so obvious that a solution of some kind must be found. To cite an instance, no one who has gone through the nightmare of operating an automobile in a country with left-hand driving can avoid the feeling of danger which the situation implies. One emerges with a clear understanding that international navigation rules are imperative, on land, on sea, and in the air. To mention another example, the need for international medical standardization is pressing and fortunately rapidly progressing. It seems likely that all future international agreements in which weights and measures are involved will be made on the basis of the metric system. As early as in 1863 the metric standards were accepted by the International Postal Convention, and many other international organizations have since followed suit. But as far as the impulse on our daily measurements is concerned it may well be that the universal scientific acceptance of the C.G.S. (centimeter, gram, second) system in the long run will prove the strongest influence in familiarizing the public with the nomenclature as well as the other advantages of the metric system. Not only is science having a steadily increasing impact on education, but the numerous laboratories in research and educational institutions, in industry and manufacturing, form nuclei where the system is in constant use. The new U. S. *Pharmacopoeia* will contain only metric weights and measures, and it will bring the metric system to that great social institution, the drugstore.

It is to be expected that if this scientific influence should introduce us to the metric system it will lead us further and induce us to make changes in the readings in the few daily observations of nature which we still perform. For one thing, we shall probably then turn to the scientifically accepted centigrades on our thermometers. For another, it seems likely that the meteorologists, not to be confused with the metrologists, would convince us that we should read our barometers not in inches or centimeters of mercury but in millibars, as the international convention in weather observations now demands. The time may not be far off when we shall see them as facultative gradings on the commercial barometers.

The formidable scientific committee which was placed in charge of the organization of the metric system consisted largely of mathematicians and astronomers, and it would have appeared almost unnatural if they had not made an attempt at decimalizing the oldest of all our measuring systems, the Babylonian division of angles into degrees, minutes, and seconds. When one combines the fact that the meter is a one ten-millionth part of the meridian from the North Pole to the Equator with a division of the quadrant of the circle into 100° instead of 90° customarily, each latitudinal degree on the earth's surface corresponds to 100 kilometers. This would certainly be a simplifying element in navigation and in general estimates.

In the presentation speech to the French legislative bodies at the time of the completion of the metric standards, one finds the following quaint utterance on this subject:

"There is some pleasure for the head of a family in being able to say: 'The field which gives my children subsistence is such and such a proportion of the globe. To this extent I am co-owner of the world.' " Most modern mathematicians would be hesitant to express such an optimistic view on the interest of the general public in performing calculations. However, this decimal division of the globe immediately leads to a complication when applied to the Equator: the longitudinal degrees would no longer have their convenient relation to the change in time, 15° for each hour. But with the inexorable and irresistible logic of reformers and revolutions one proceeded to decimalize time. This reform was officially decreed, but it went too far even for the revolutionary spirit of the French. A few decimalized clocks were actually constructed, but they stand as memorials to a reform which died through public neglect.

Nevertheless, one can have no doubt that the decimalized and unified time and angle system would have been simpler and more harmonious than the ones now in use, but its advantages to the public as a whole are so small that we shall probably never see them reappear. But to indulge in some hypothetical thinking, should the astronomers come to the conclusion that a common decimal measure for time and angles would save enough labor to make profitable an investment in new tables, the navigators would be likely to follow suit and the time problem would again confront us. Laplace of the metric commission, "the Newton of France," took the consequences of his own proposal and used a complete decimal division of the angles in his master work on celestial mechanics. The system is still fairly common in France. Today's astronomers have contented themselves with a compromise by abandoning their "*partes minutae*" and "*partes minutae secundae*" for decimal parts of a degree in numerical calculations. Other systems of angle measuring may be useful, the mathematicians use radians and the United States artillery find their mils practical. The 24-hour clock will probably come into more general use through its adoption by the armed forces. In many European countries it has already been adopted officially; railroad and telegraph time are usually marked this way and clocks with 24-hour markings were fairly common before the war.

A common calendar for the world seems such a natural idea that any future league of nations is bound to bring up the problem for discussion. We may be a bit provincial in believing that our own calendar is almost universal and also that it is quite ideally constructed. In addition to our own Gregorian calendar there are the Chinese, the Shinto, the Mohammedan, and the Jewish calendars, all with numerous adherents; from India there are reported fourteen calendars of other types in actual use, and innumerable tribal calendars are spread over the world.

In all essentials our calendar is the same as the Julian calendar initiated by Julius Caesar in the year 45 B.C., with the Egyptian astronomer Sosigenes providing the necessary scientific foundation. One great innovation in this calendar was to make it exclusively dependent on the sun, leaving the moon entirely out of consideration; a second was the introduction of the leap year, which we still enjoy, to keep the year in step with the sun and the seasons. By this device the year was very nearly correct, only 11 minutes and 14 seconds too long. However, time will tell, and by the sixteenth century the year was ten days behind schedule and threatened eventually to get entirely out of its customary relation to the seasons.

After many hesitations, following expert advice from the astronomers, Pope Gregory XIII finally decided to move the calendar ahead ten days in 1582. To prevent similar developments in the future the leap year was dropped in all century years 1700, 1800, 1900, 2100, except when the century number was divisible by four as in 1600, 2000, etc. As one could expect, the move created the most violent opposition in certain quarters, notwithstanding a treatise of 800 pages of official explanations of the consequences of the change. When the reform finally came to England in 1752 there were even riots to bring back the lost eleven days, while some of the smartest guild members tried to obtain

eleven days back pay. The year is now a trifle too short and it has been proposed to make the years 4000, 8000, etc., leap years, contrary to the Gregorian rule. This would keep the year on an even keel for the next 20,000 years so that this side of our calendar should cause us no immediate concern.

There are, however, other aspects of our calendar which give us concern every time we handle our dates. For one thing it is inconvenient to have the weekday of any given date change from year to year. The Roman legacy left us with another disadvantage, the irregular lengths of our months. They are hard to remember, and the calculation of dates becomes complicated as we all know from sad experience. There are other anomalies in our calendar, which we perhaps do not notice so often; for instance, the first half of the year is three days shorter than the second.

The remedy is by no means obvious. The French Revolution, as we have seen, was possessed by the demon of decimalization and so proceeded to introduce a week of ten days. However, as de Morgan observes in one of his comments upon paradoxers, one can decimalize the Ten Commandments but not the Twelve Apostles. The number of days in the year is not divisible by ten, and no reason whatsoever is likely to make us abandon our seven-day week. When the 365 days of the year are divided by seven, there is an odd day left; consequently, if the week days shall repeat themselves on the same dates every year, there must be an extraordinary day which is not counted with the ordinary week days. This leaves us with 364 days to be arranged in the most satisfactory manner. Since this is exactly 13 times 28 days, one could divide the year into 13 months, each consisting of four weeks, or 28 days, which would repeat themselves in the same way throughout the year. Such a plan is certainly very simple in principle, and the 13-month year for a while enjoyed considerable favor. It was under discussion by the League of Nations and had strong backing, financially, for instance, by George Eastman of Kodak fame. There are, however, some drawbacks to the plan: The introduction of a whole new month would be difficult to reconcile with old contracts, and it would not be simple to divide the year into halves and quarters as we are accustomed to.

For these reasons the 13-month calendar has now largely been superseded by a new calendar proposal which its sponsors hopefully have named the World Calendar. The 364 days are now divided into four identical quarters, each of 13 weeks or 91 days. The quarter always begins on a Sunday. Each quarter has three months as usual, but to make the months as systematic as possible in their arrangement the first month in each quarter has 31 days and the two others 30 days. Here is the complete year:

THE WORLD CALENDAR

Every Year the Same

The year begins with Sunday, the first day of the week, and ends with an extra Saturday, the Year-End Day, which is the 365th day.

FIRST QUARTER

JANUARY							FEBRUARY							MARCH						
S	M	T	W	T	F	S	S	M	T	W	T	F	S	S	M	T	W	T	F	S
1	2	3	4	5	6	7				1	2	3	4						1	2
8	9	10	11	12	13	14	5	6	7	8	9	10	11	3	4	5	6	7	8	9
15	16	17	18	19	20	21	12	13	14	15	16	17	18	10	11	12	13	14	15	16
22	23	24	25	26	27	28	19	20	21	22	23	24	25	17	18	19	20	21	22	23
29	30	31					26	27	28	29	30			24	25	26	27	28	29	30

SECOND QUARTER

APRIL							MAY							JUNE						
S	M	T	W	T	F	S	S	M	T	W	T	F	S	S	M	T	W	T	F	S
1	2	3	4	5	6	7				1	2	3	4						1	2
8	9	10	11	12	13	14	5	6	7	8	9	10	11	3	4	5	6	7	8	9
15	16	17	18	19	20	21	12	13	14	15	16	17	18	10	11	12	13	14	15	16
22	23	24	25	26	27	28	19	20	21	22	23	24	25	17	18	19	20	21	22	23
29	30	31					26	27	28	29	30			24	25	26	27	28	29	30

Leap-Year Day, June 31 or W, a World Holiday, an extra Saturday, follows June 30 in leap year.

THIRD QUARTER

JULY							AUGUST							SEPTEMBER						
S	M	T	W	T	F	S	S	M	T	W	T	F	S	S	M	T	W	T	F	S
1	2	3	4	5	6	7				1	2	3	4						1	2
8	9	10	11	12	13	14	5	6	7	8	9	10	11	3	4	5	6	7	8	9
15	16	17	18	19	20	21	12	13	14	15	16	17	18	10	11	12	13	14	15	16
22	23	24	25	26	27	28	19	20	21	22	23	24	25	17	18	19	20	21	22	23
29	30	31					26	27	28	29	30			24	25	26	27	28	29	30

FOURTH QUARTER

OCTOBER							NOVEMBER							DECEMBER						
S	M	T	W	T	F	S	S	M	T	W	T	F	S	S	M	T	W	T	F	S
1	2	3	4	5	6	7				1	2	3	4						1	2
8	9	10	11	12	13	14	5	6	7	8	9	10	11	3	4	5	6	7	8	9
15	16	17	18	19	20	21	12	13	14	15	16	17	18	10	11	12	13	14	15	16
22	23	24	25	26	27	28	19	20	21	22	23	24	25	17	18	19	20	21	22	23
29	30	31					26	27	28	29	30			24	25	26	27	28	29	30

Year-End Day, December 31, or W, a World Holiday, an extra Saturday, follows December 30 every year.

It seems difficult to improve on this arrangement; indeed, it appears to be a very fortuitous coincidence that it should be possible to find such a scheme which embodies within it almost all the improvements one would like to make with such a minimal deviation from our usual schedule. The date

for Easter remains the only element in our calendar dependent on the moon, and if the various religious groups could agree upon a fixed date the new calendar would repeat itself perpetually from year to year. There is no necessity to elaborate on the advantages of this calendar: it would simplify tremendously the problem of making schedules for every school and for every business and industry in the country. The new calendar has been very ably sponsored by the World Calendar Association, and since the reform is in the unusual situation of having the approval in principle by a whole group of countries it may not be many years after the war before we see it in effect.

The extra day which the calendar demands would be placed before New Year's day, and it might properly be named and devoted to some universal moral purpose: Peace, Liberty, Unity, World Understanding. In leap years there would have to be a second extra day to keep the schedule in order. I like to think of it as the Day of Reckoning.

THE FABLE OF THE SCIENTIST AND THE POET

A Scientist who had no use for flimflam set out one day with his mistress, Definite Purpose, for the City of St. Louis, which is in Missouri. Having taken numerous bypaths seeking for a shortcut, they finally were on the point of concluding that they had lost their way when they met a Poet going in the opposite direction.

"Have you lost your way, too?" asked the Scientist, noticing a sort of hesitant gleam in the Poet's eyes.

"No," replied the Poet, "I have not lost my way because I wasn't going anywhere in particular. I am just wandering about looking at the sky. I have been watching the clouds until my neck is stiff. I think I shall write a poem about clouds. By the way, who is that woman?"

"Oh, her? She's a friend of mine, but I am seriously thinking of dropping her off at East St. Louis. She is getting to be a terrible nuisance and keeps telling me that if I had listened to her advice we wouldn't have gotten lost. She keeps asking me where the hell I think I'm going."

"Well," said the Poet, "it's none of my business, of course, but if I were you I'd get rid of her. Why don't you ditch her and stay here awhile with me and tell me about clouds. There are several questions I'd like to ask you about them. But that woman! She gives me the willies. If she can't keep you on the right road, what good is she? And besides, she does not seem to me to possess that air of beauty that one should desire in one's feminine companions."

"I know," said the Scientist. "Sometimes I agree with you, but shaking her off is not so easy as it seems. And so, since I must apparently choose between you, I'm afraid I must forego the dubious pleasures of meteorology and the company of an uncertain Poet in favor of the lady here, who, though she is something of a termagant and frequently causes a problem in regard to obtaining a hotel room, has on numerous occasions saved my professional face and once even saved my life."

"A frightfully long sentence," said the Poet, "but I see what you mean. It must be miserable, though, to be beholden to such a repulsive creature. It has been awfully nice knowing you. Must you go?"

"Yes," replied the Scientist, "I'm afraid we must. But do you happen to know which road we should take to get to St. Louis?"

"St. Louis is so far away," answered the Poet. "Why don't you go to Minneapolis, Minnesota, instead? It's a lot nearer and besides has a much more mellifluous name."

At this, Definite Purpose, who up to this time had taken no part in the conversation, swore a mighty unsanctified oath and grabbed the Scientist firmly by the arm, and together they went off down the road, almost running, leaving the Poet alone and a trifle bewildered.

Moral: There are worse things than being lost while going to St. Louis.

—Paul H. Oehser

THE SCIENCE CORE IN LIBERAL EDUCATION*

By A. J. CARLSON
UNIVERSITY OF CHICAGO

As THE natural sciences provide our only reliable understanding of man and of the universe, these sciences have gradually been incorporated as part of our liberal education. The sciences of physics, chemistry, biology, and medicine have given new implications to the old sayings: "know thyself"; "the proper study of mankind is man." But it is not true that the natural sciences dominate (some say destroy) liberal education in America today. If the nearly 200 colleges and universities of 16 midwestern states studied by me three years ago are at all typical of our nation, the natural sciences in our colleges make up only about 20 percent of the college faculties and college teaching budgets. I can only express my grief and regret that so many colleagues do not secure information on this question before they talk or write. We, the teachers, should speak by the card. And according to my best information and experience the natural sciences, or anything worthy of the name science, do not dominate the high school curriculum of our nation either.

Owing to the imperative needs of technology, this war has led to some fears, confusion, and debate both as to our alleged educational failures and dry rot of yesterday, and as to the danger of greater educational errors tomorrow. In much of this debate we are putting the emphasis in the wrong place; on the *plan* or the *system* instead of on the *teacher*. To be sure, there is technology without science, but there is no science without some technology. Why fear *skill* in any field? There is technology even in philosophy, in the humanities, and in the social studies. This war has fostered another delusion. We think we are going to have tomorrow not only "one world," but a "new world"! Hence, the tried and true in yesterday's liberal education will not apply to tomorrow. Yes, a new world, populated by the same old genus *Homo*. For history and anthropology reveal that man changes very, very slowly, considering the span of our race. It seems clear to me that we shall come out of this war with the same old brain, the same old struggles to survive, essentially the same old mores and values in human relations and in the pursuit of happiness. Here are a few recent samples of this futile and unfortunate debate:

In his recent book *The Rebirth of Liberal Education,* a study financed by the Rockefeller Foundation, Dr. Fred B. Millett makes the charge that science and the scientific method are the most dangerous foes of the humanities in our liberal education program. President Hutchins appears to voice the same myopic views when he charges that "specialism, which has wrought such wonders in scientific investigation, has promoted the extinction of liberal education. . . . The forces of specialism are constantly whittling down the program of liberal education and deprecating it." But President Hutchins knows, or should know, that specialism, in the sense of the trivial as opposed to fundamentals, is not confined to the natural sciences. There is plenty of specialism and the trivial both in the humanities and the social sciences. And there is little progress in the old pastime of the pot and the kettle. None of us can always be sure that what first appears trivial in the natural sciences will remain trivial. The failure of bacteria to survive in close proximity to certain moulds looked trivial at first, but few informed people would label the discovery of that initial fact *trivial* today.

Professor J. Duncan Spaeth of Princeton University is on the same beam when he says: "The eclipse of the humanities as a result of mobilization of education for total war is but the culmination of tendencies and drifts that have long been operative, and is a symptom of the deep *infection* [italics ours] of our modern, and especially our Western world,

* Abridged from Bulletin 1 of the Association for General and Liberal Education.

with a mechanistic and materialistic interpretation of the nature and source of *power*. It is the victory of the 'law for thing,' over the 'law for man'." But man is also "a thing," subject to the forces and laws of nature. I regret to note that Dr. Spaeth is in the poorly informed group who insists that the natural sciences *dominate* modern liberal education. This is simply not the fact. And there does not exist that clear and sharp antithesis between the natural sciences and the humanities outlined by Dr. Spaeth: "Science advances by experiment, the humanities build on experiences. Science . . . creates the knowledge that is power; the humanities . . . create the power that is character. . . . The humanities look backward, the physical sciences look forward." Nor can I agree with Dr. Spaeth that "the humanities are the only efficacious antidote to a narrow nationalism and cultural isolationism." The scientific method is universal. Biology and anthropology have established the essential homogeneity of the present human race. The sciences of medicine, health, and disease cannot (or should not) be confined by political boundaries. But I am in complete agreement with Dr. Spaeth when he says that both the sciences and the humanities are necessary in "a complete and generous education." However, when is even a "generous education" *complete?* Granted that some teachers in the natural sciences, as well as some teachers in technology, stay too persistently in their special ivory towers and are relatively ignorant of essentials in other fields of human needs. Granted also that a few very competent men in the natural sciences (especially in the stronger colleges) focus even their elementary courses in the direction of specialization rather than on the needs of liberal education for all citizens. The most we can make out of these facts is myopia of a few individual scientists. These facts do not warrent the above sweeping condemnation of science and the scientific method. I cannot agree that training to "carry your own knapsack" is entirely foreign to liberal education. I was trained in my youth to coordinate the hand, the eye, and the ear with the brain, to do some tasks with speed and accuracy (farming, carpentering) necessary for living. That training (plus the "classical course" in the American college of 50 years ago) does not appear to have retarded the quest or seriously damaged my mental processes. It may be questioned whether science and the scientific method can make the greatest contribution to liberal education without some technology, a minimum of individual laboratory and field work. But I fail to see that such laboratory and field experience is an enemy of history, sociology, or the fine arts. The goal of the natural sciences is the *understanding of man and the universe.* If the goal of the humanistic educational disciplines in liberal education is the *evaluation of values,* I for one fail to see any antagonism between them. Men and women of tomorrow need both. It would be more to the point to put the emphasis on cultural teachers rather than on cultural courses. Any real evaluation of "values" calls for some understanding of man. The understanding of man and the rest of nature should by itself create some "values."

WHAT is, or should be, the minimum core of science in liberal education? If we can find it, if we can agree on it, have we the men and the means to make it effective?

1. I think most educators will agree that this core must include: (a) mastery of the scientific method, the principles of experimentation and controls in experience; (b) the fundamentals of human biology in health and disease; (c) the fundamental forces and processes in the universe—all these things, not on the basis of the guesses of today or the philosophies of yesterday, but derived through application of scientific methods.

2. On the college level, as well as in the high school, the time required to attain these goals will vary with the teacher and with the student. But I think that this science core would not greatly crowd the offerings in the humanities, especially if the science teachers could inspire their students to work; that is, to use all their mental abilities to the utmost. It should be equally clear that these goals may be successfully approached by different plans and set-ups in different institutions. The plan or plans are of secondary impor-

tance, the teacher or teachers are of first importance. The general science courses for all college freshmen have gotten results in the University of Chicago. But the same plan, with different teachers, has failed in some colleges. It is clearly up to the genius of each college faculty. College administrators should not try to impose on the faculties any plan by fiat. The teacher must be convinced that the experiment is worth trying. Then, and only then, does it become an educational experiment.

3. In a recent 62-page report on the education problem of the world today the British Association for the Advancement of Science point out that science in general education should not be as strictly compartmentalized as are the natural sciences in the training of specialists and technologists. "The study of natural science [in general education] must be wider than that of the specialists' science schools; e.g. chemistry and physics should be taken together as part of 'physical science,' and general biology in place of separate treatment of zoology, botany, and physiology." This is essentially the type of courses in the Physical and the Biological Sciences required of all freshmen students in the University of Chicago during the last 15 years. I agree with our British colleagues that we do *not* have enough teachers trained to give such courses adequately. But I cannot agree with them when they say that because "few teachers in history, economy, and language have even a passing acquaintance with the natural sciences and the scientific method," and "few scientists have a good grasp of social problems" students in the humanities should not be compelled to take courses in the natural sciences, and students in the natural sciences should not be forced to take courses in the humanities and the arts. Educators who wait for utopia are obstructing progress. When we are sure of the goal, we can try for it, even with the aid of crutches.

4. So far I have spoken as if the liberal education of our fellow citizens was primarily or exclusively a concern of the colleges. This is, of course, not the case. This education at the high school level is even more important than at the college level, for the high schools reach more students. And as to the mental maturity or immaturity of our high school population, I am certain that a large part of the high school students can master the above science core had we the facilities, the vision, and the men and women who could really teach it. I speak not without experience on this point. For more years than I care to state I have endeavored to teach (let my students say how effectively) the dynamic biology of man to (a) college freshmen; (b) medical students; (c) graduate students. When you get your students' real attention and interest, their mental capacity is equal to the task at any and all of these levels of mental development. Of course, I admit that the dynamic biology of man has the advantage that, here and there, it contacts and clarifies the student's own experience. I have always felt a responsibility to college freshmen in this field. For without a clear understanding of man in health and in disease even college-trained men and women are but "sitting ducks" for the thousand and one varieties of medical and social quackery that feed on the ignorant.

5. The college teacher, like other people, can readily become a creature of habits and a worshipper of the past, including the sacrosanct liberal education curriculum on which he may have labored for a quarter to half a century. Science has indeed disturbed the traditional humanities, not so much by crowding the curriculum as by questioning ancient and easy answers and interpretations. Science bows to no man, not even to the "hundred great books" of the past. Yes, science is an iconoclast, a disturber of faiths and dreams, a challenger of nostalgia. But science is a product of man, and science changes significant parts of man's environment faster than does raw nature. Science creates new problems important in the lives of all the people. The science core in liberal education must provide an understanding of these changes and problems lest we perish.

AN ANTHROPOLOGICAL BIBLIOSYMPOSIUM*

MAN AND HIS WORLD

By W. M. KROGMAN

DEPARTMENT OF ANTHROPOLOGY, UNIVERSITY OF CHICAGO

Mainsprings of Civilization. Ellsworth Huntington. 660 pp. Illus. 1945. $4.75. John Wiley and Sons, Inc., New York.

TAKE equal parts of the *Pulse of Asia* (1907), *World Power and Evolution* (1919), *The Character of Races* (1924), *Civilization and Climate* (1924), *Season of Birth* (1938), add dashes of many other books and shorter articles, mix well with nearly fifty years of keen observation and speculation, and season with an intriguing ability to fit curves to theory and vice versa, and you have the present volume, a stimulating and at times heady concoction.

The book is in three major parts: the background of civilization; heredity; physical environment and human activity. I am going to discuss these parts in order and then shall attempt an evaluating synthesis.

Evolution has been guided by three principles: 1) a basic evolutionary force; 2) innate qualities of the organism as determined by heredity; 3) the physical environment (p. 35). The latter is extremely complex for it includes the factors of climate, distribution of plants, animals and men, demography, agriculture, diet, housing, industry, transportation, trade, inventions, machinery, science, education, art, religion, crime, war, politics, and philanthropy (p. 225). It follows, therefore, that Huntington is ultimately concerned with biosocial evolution. On the purely organic side "the tendency of evolution has been to develop some form of life for every environment which is not prohibitively cold, dry, or otherwise impossible" (p. 22). This has been facilitated by three things: the constancy of the temperature at the Earth's surface; a ready and balanced supply of water; and an endless source of free oxygen. Correlated with this are ten factors of life itself: size of planet, density of planet, distance of planet from the sun, shape of the planet's orbit, speed of the planet's rotation, length of the planet's day, degree of the planet's cooling, size of central sun, developmental stage of central sun, degree and periodicity of variation in sun's rays (pp. 19–20).

All these ultimately set the stage for vertebrate evolution. Then came a series of physical and climatic changes. In the Paleozoic, uplift gave rise to swiftly running waters which in turn demanded an axial stiffening—so a flexible backbone developed. In the Permian, aridity gave rise to lungs, warm blood, speed of locomotion, and higher metabolism. Later, descent from trees gave survival value to primate brain and hand (pp. 23, 26). Man was born and of this fact we are told that "since *H. sapiens* spread widely over the earth he has experienced no radical biological change" (p. 33).

In his treatment of organic evolution Huntington is quite orthodox. He follows the conventional sequence emphasized by paleontologists. But for him physiographic and climatic changes are the impelling forces in sequential progress: if there is a change on the earth's surface or in the earth's atmospheric envelope, there is a corresponding change in the earth's organic life.

But man is more than an instinctive animal, he is a cultural being as well. Accordingly, the basis of this entire volume is found "in the fact that *H. sapiens* is the most advanced product of organic evolution and modern civilization the most

* With this issue SM publishes its first "bibliosymposium." The editor coined the word and we think that it describes accurately the objective we had in mind; namely, to present extended reviews of a group of books on the same subject. The atomic bomb, resulting from unprecedented activity on the part of the physical scientists, brought military victory to our side. It now remains for the social scientists to match the versatility of their colleagues to ensure our winning the peace. As this bibliosymposium well testifies, the students of the science of man have not been idle. We have at hand the basic fundamentals of the relationships of intersocietal living; now we must strive to write them into the peace and make them work.

The project represents an experiment for SM. Presentation of another depends on a number of factors. First, there must be a group of books on the same general subject, published within a reasonably close time. Secondly, there is the matter of space, for SM always has a backlog of principal articles that must be published. Finally, there is the question of whether or not a majority of the readers want it—and on this subject we hope that we may have an expression of opinion from a large number of SM subscribers. A limited number of reprints will be available. Requests should be directed to the Book Review Department.—T. J. C.

advanced product of human progress" (p. 14). So we come to *culture*, which is defined as "every object, habit, idea, institution, and mode of thought or action which man produces or creates and then passes on to others, especially the next generation" (p. 7). For Huntington the evolution of culture is due to the exigencies of the environment, and, moreover, it develops in a certain sequence. "Man appears to be so constituted that, wherever the physical environment permits, he will in due time evolve such ideas as the raising of cereals, the taming of animals, etc.," and "a similar spontaneous development occurs along more abstract lines" (p. 2).

After culture comes *civilization*. "Advances in human culture in their turn modify both physical environment and heredity. The ultimate result is civilization" (p. 42), which "begins when people learn to practice agriculture, live in permanent communities, establish a definite form of government, and acquire the art of writing" (p. 7). But civilization is not inevitable. Many cultures do not make the grade, so to speak. Civilization entails a twofold selective process, which 1) "eliminates people who fail to reach a minimum level in certain innate qualities which are essential to preservation," and 2) "eliminates customs and ideals—mores—which tend to cause a kith to die out" (p. 172). This selective process, in turn, gives a balanced blend of physical environment, selective heredity, and a socially acceptable set of concepts concerning material possessions, customs, and ethical standards.

The development of civilization and the rate of its progress depend "upon the degree to which the geographic environment in all its phases is appropriate to the stage of culture attained" (p. 612). It depends also "upon health and upon the vigor with which people use their innate capacities and cultural advantages" (p. 612). Huntington defines vigor as "the kind of clear-eyed alertness and eagerness for work which one feels when in the pink of health," for "good health and high civilization obviously go together" (p. 237). Health and vigor are due to: 1) innate biological endowment; 2) a favorable physical environment with respect to climate, food, and disease; and 3) a culture high in medicine, nutrition, sanitation, hygiene, and transportation. There are other biosocial factors: "The tendency of the human species to mate at all seasons seems to be one of the cornerstones of civilization" (p. 32); and, "It is generally agreed that the family is the basic unit of society" (p. 32).

Two of the most potent factors in the emergence and progress of civilization are climate and temperature. Civilization arose first in latitudes 25° to 35°, and if within 25° of the Equator it arose on temperate plateaus (5000′–7000′) or cool seacoasts (p. 269). Examples of tropical civilizations are found in the highlands (the Aztecs in Mexico, the Yemen in Arabia, the Zimbabwe in Rhodesia); the cool west-coast areas (Inca in Peru); and the warm, rainy, forested lowlands (Singhalese in Ceylon, Cambodia in Indo-China, Java, and the Maya in Yucatan and Guatemala). But even then Huntington feels that "it is probable that migration was at the basis of all tropical civilization" (p. 398).

Huntington administers the coup de grace to Mackinder's *Geopolitik:* "Who rules Eastern Europe commands the Heartland, who rules the Heartland commands the World-Island, who rules the World-Island rules the World" (quoted on p. 199). Huntington observes that the peoples of the Heartland have had a great effect on history owing to their "innate character and closely knit democratic social system, which normally develops when the same kith resides for many generations on steppes or mountains where the pastoral mode of life prevails" (p. 203). Huntington concludes that the Heartland is no longer strategically important owing to human mobility.

We may now turn to Man, himself, to whom "the laws of biological evolution still apply . . . as forcibly as to amoebas or apes" (p. 608); or, again, "We may be certain that the same great principles which have governed organic evolution for a billion years or more will still prevail. Selection will assuredly eliminate certain types of peoples and also certain customs" (p. 609).

Man lives in groups and he moves about in groups. Therefore *migration* is an important factor in the emergence and diffusion of civilization. Huntington's major thesis here is the selective nature of migration—he believes in Sandburg's, "The cowards never started; the weak ones died on the way." He has this to say: "When a single migrating group is taken by itself, it seems probable that those who migrate farthest, or to places especially difficult to reach, or requiring difficult adjustments to new conditions, are likely to be the ones most highly endowed genetically as well as culturally" (p. 75); to which he adds, "People of diverse social levels and abilities migrate in different directions and to different degrees" (p. 93). Several examples of selective migration are given, especially of nomads, when the Asiatic Chantos of the Tarim Basin ("the essence of mediocrity") and the Khirghiz of the neighboring steppes are compared. "If temperamental tendencies are hereditary, as most assuredly they are, it seems almost certain that prolonged biological selection must have tended to

intensify innate qualities which give nomads unusual ability as soldiers, organizers, and rulers" (p. 179). He also compares the Hakka, in the mountains north of Canton and west of Foochow, with village-dwellers of North China, and concludes that here again is illustrated "selective migration which separates people of the same race into an uncommonly competent group far away from the original home, on the one hand, and a group, little above the stage of morons, on the other" (p. 185). Huntington is a firm believer in the role of superior peoples in civilizational development. He now develops the theme of these highly endowed peoples with "innate" abilities.

The answer to this problem is found in *kiths,* a useful concept developed by Huntington, a term devised (after rejecting "natio-racial" and "ethnic") for "groups of people smaller than races and often derived from a mixture of races" (p. 102). Kiths are defined as "groups of peoples with a similar culture and language and with the custom of intermarriage" (p. v): or, alternatively, as "a group of people relatively homogeneous in language and culture, and freely intermarrying with one another" (p. 102). The idea of the kith "recognizes that biological inheritance is a real and important factor" (p. 214). There are *microkiths* (as the royal families of Europe, the patricians of Rome, the aristocracy of ancient Greece) and *macrokiths* (as the Rumanians, the Prussians, the English) (p. 220). Evidently kiths are both sociopolitical and national. The latter are, throughout the book, the most frequently referred to: Jews ("no other kith has so persistently produced great leaders") (p. 154), Icelanders, Newfoundlanders, Puritans, Junkers, Quakers, Mongols, Sicilians, Picts, Angles, Normans, Parsis, Hakkas, Hittites, Spartans, Highlanders (in Scotland, Kentucky, Nepal, Kenya), Walloons, and the Portuguese Negroes of Brazil. This is a heterogeneous assemblage, running a religio-socio-politico-militaristic gamut.

Huntington believes the kith to be the potent force in cultural origin, diffusion, and survival. The Puritans are an excellent example of this. "If the selective forces are strong enough, and if the people who are selected are sufficiently isolated, the results of selection by migration may persist with amazing strength for centuries" (p. 126). A number of Puritan names, 588 of them, were selected "because of their dominance in southern New England after the Revolution"; they "represent the racial stock of 'kith' . . . which originated for the most part from 1630 to 1642" (p. 102). Of these names three were, together with four widely used English names, traced in *Who's Who,* the *Encyclopaedia Britannica,* in credit ratings, and in criminal records and public relief records. The ratios of the three "Old Colonial" names to the four used for comparison were as follows: *Encyclopaedia Britannica* 16.8, *Notable Americans* 10.5, *Who's Who,* 6.3, inventors 1.8, criminals 0.6, on relief (in 1933) 0.4 (p. 122). It was concluded that "these facts . . . all suggest that hereditary mental traits, as well as cultural conditions, enter into the differences among our various types of names" (p. 122). He goes on to state that "crime and chronic dependence on relief . . . are often significant of innate weakness as of misfortune" (p. 105). One is reminded of Hooton who regards criminal behavior as evidence of "biological inferiority."

While Huntington stresses *kiths* as a basic unit in civilization, he still refers to *race,* and usually does so in a special manner, almost always referring to an innate character. Thus on p. 43 he speaks of the "innate mentality of a pure Negro population in Africa;" on p. 44 of "innate racial character;" on p. 155 he says that "the process of selection depends upon mentality far more than upon the external physical traits on which races are based;" and on p. 37 he states that "we need exact knowledge of the innate qualities of races, and especially of nations and social classes." Finally Latimore (p. 170) is quoted regarding the North Chinese and the Mongols: differences between them are not so much physical, as in "stance, movement, expression, manner . . . not differences in physique itself, but of the life within the physical structure . . . these intangibles, which belong to outlook, culture, feeling, and the way of life, establish a cleavage." All these references are quite acceptable, but the interpretation begins to give one pause: "Human society is so constituted that competent people tend to rise to the top, thus creating social stratification" and "this stratification is associated with differences in innate capacities" (p. 147); "Want, misery, disease, and backwardness are favored by the existence of people with low innate capacities, or by the presence of too dense a population composed of people with ordinary capacities" (p. 224). This oft-repeated faith in "innate capacities" (biologically and culturally) may be used as a step towards a claim of national or racial superiority. Huntington himself doesn't make such a claim. In fact he says "although race has played a great part in history, the moving force has been the *idea* of racial superiority, rather than any actual differences other than those due to environmental or to dominant kiths" (p. 221). This is well and good, but the stated or implied supremacy of "innate qualities" is certainly an *idea* that may be taken up by racists!

Human social behavior is profoundly influenced by the *seasons,* which "have exercised a great effect upon the type of culture which has grown up in various parts of the world" (p. 311). Obviously agriculture, affected doubly by geography, per se, and by seasonal fluctuations, is very important in civilizational progress. Other phases of man's behavior are also influenced by the seasons: disease and death are most prominent in February; mental alertness is at a maximum in March; insanity, suicide, and sex crimes are at a peak in May and June; irritability, violence, and rioting take precedence in June; inclination to physical effort is at a maximum in October and November (pp. 358, 365, 368). If you want to live longer get conceived in June, born in March (p. 319). If you wish to achieve fame arrange to be born in February, or even January (p. 322). Of the latter two desirabilities Huntington suggests "that mankind still inherits an unknown mechanism which originally helped infants to endure summer heat, provided they were born in early spring" (pp. 273, 323).

There are undoubtedly certain physiological stimuli and responses in seasonal change so that individual reaction—and even group reaction as the summation of component individuals—may affect social behavior. But this is not, cannot be all. One must look deeper into the structure of the social organization. Huntington admits this, but he himself does not give social structure enough weight.

Climate in its entirety is a very potent factor. Huntington speaks of the "coldward and stormward" march of civilization (p. 405). Specifically, "in the last several thousand years the center of civilization has shifted from warmer to cooler areas, and from quieter to stormy areas where atmospheric ozone and electricity are at a maximum" (pp. 528, 547). "In the climatic pulsation of historic times a major feature appears to have been variations in the movements of polar and tropical air masses" (p. 375). In the presence of a storm there are changes in air pressure, air movement, water content, temperature, amount of ultraviolet, ionization, and atmospheric ozone and electricity (pp. 527–528). These changes record themselves upon human behavior, and Huntington cites as an example the period A.D. 1250–1450 when the amount of storminess increased, and extremes in weather occurred (pp. 505–506). As a result this "was a time of special alertness, initiative, and originality in most of Europe" (p. 599).

There is another phase that is deemed important; namely, *temperature.* The optimum is 70° F. in humid regions, 80° F. in dry regions (p. 268), with an all-around optimum of ca. 65° F., day and night (p. 273). Huntington makes one statement of special importance to the physical anthropologist: "The essential uniformity of the reaction of all races to temperature is altered only slightly by a few racial differences which may be interpreted as incipient biological means of protection against heat or cold" (p. 267). He goes on to cite the Eskimos and Tibetans who are stocky, with a layer of fat; the East Indians who are slender, with relatively large surface areas; the African Negroes who have developed many sweat glands and deep pigment. The adaptive morphology of the Eskimo and Tibetans and of the East Indians suggests Sheldon's endomesomorphy and ectomorphy, respectively.

Huntington feels that *diet,* closely related to climate and the weather, is very important: "the diet that has prevailed for generations is one of the major factors in determining the character of nations and kiths, and in settling the fate of wars and the rate of human progress" (p. 431). He extends this theme by stating that "the ideal diet . . . must be essentially that to which man became adjusted during his long and slow evolution" (p. 433). Such a diet, he contends, must be eaten in a fresh condition, must possess variety, and must grow in natural soils, little deteriorated by long or excessive cropping. The relation of diet to growth is a factor well known to students of child development. It follows, then, that if the physical growth of the population is facilitated by diet, the adult physique must be correspondingly influenced. We can agree that diet and civilization are correlated.

We come, finally, to the problem of *cycles* in human affairs. Such cycles are repetitive, rhythmic, and periodical. Hence they have predictability. Huntington is inclined to ascribe them basically to sunspots (following Tchijewski); these solar phenomena are reflected in increased electrical activity in the sun, which in turn affects atmospheric ozone and electricity. "There is scarcely room for doubt that through some electrical process sudden disturbances of the sun's atmosphere are associated with a well-defined influence upon man's health and psychological reactions" (p. 516). There are a number of cycles: 1) a 41-month (3.4 years) cycle which predominates in business as seen in the stock market (Dow-Jones) and in prices, production, and sales (Beveridge, Hoskins, Dewey, etc.) (pp. 463, 468): 2) a 7.5-year cycle or multiple thereof, seen in wheat prices, 68 years (Beveridge); sunspots and other electrical cycles, 22 years; weather in Europe, 22 years (Brunt); weather generally, 46 years (Abbot); rain and

flood, 90 years; cotton prices, 68 years (Dewey); Nile floods, 66 years (Brooks); and varves in Lake Saki, Crimea, 67 years (p. 461); 3) a nine-year (9.2 years) cycle, influenced mainly by atmospheric electricity and centering principally upon business phenomena (p. 476); 4) a 9.7-year cycle influenced mainly by atmospheric ozone, and centering upon animals and physiological phenomena, e.g., the 9.6-year cycle in chinch bugs and the ten-year cycle in caterpillars, salmon, and grouse (p. 488); 5) a 35-year cycle enunciated in 1891 by Bruckner: "the weather of Europe varies definitely in cycles with an average length of about 35 years" (p. 455); to which Huntington adds that since A.D. 1700 insect pests in Europe have had a general maximum at average intervals of approximately 36 years (p. 456).

These five cycles—3.4, 7.5, 9.2, 9.7, and 35 years—are not so clear-cut as one might always wish. They may well, at times, be either fractions or multiples of one another. There are climatic pulsations, cyclical in nature, but their essential independence of one another is not clearly demonstrated. There is no absolute proof that they may not represent an almost continuous series of exacerbations and remissions.

In the foregoing pages I've presented Huntington's major theses at considerable length. I've quoted him extensively, to the best of my ability. I've done so fairly, not ripping a quote from its context so that I've neither misrepresented Huntington nor (mis) quoted him to offer ideas of my own. I read this book literally line-for-line. It is good, well done, it is stimulating—and at times it is irritating. I wish, for example, that he had not said this: "Regardless of rain or shine, people's impulse to draw books from the library is subject to constant fluctuations in harmony with the coming and going of the tropical and polar air masses which bring storms" (p. 375). (I said to myself, "Hm, I'll wager *Forever Amber* was inspired by a tropical air mass"). I reacted to some of the treatment of the data regarding cycles by thinking of the story of the dear old lady who regarded the number 7 as very important in the Bible; when the Twelve Disciples were pointed out to her she triumphantly rejoined, "Yes, but 7 and 5 are 12, aren't they?" When Huntington refers to the reproductive cycle and basic diet as reminiscent of ancestral history, he subscribes once more to innate qualities. I have the feeling that he believes in biological and cultural superiorities; that's O. K., as long as we accept these as optima for *all* peoples; selection is operative, but we cannot permit it to be discriminatively so. Whether he intends to or not, Huntington gives aid to those who believe in "innate" racial biological and cultural superiorities.

The concept of *kith* is an important contribution to anthropological thought. In a sense it corresponds to Boas' "family-line type," and the German *Gautypus* and *Berufungstypus,* but Huntington develops it into a fairly consistent, workable concept. All told, this book should be read—carefully, painstakingly—by anthropologists of all "schools" of thought. Some will yelp vigorously at his causative correlation between natural phenomena and culture. But I think that one may react as vigorously to "All Culture from Climate" as to "All Culture from Culture." To me the answer is somewhere in between—I am not an appeaser, or a middle-of-the-roader. As a human biologist, I am skewed in Huntington's direction.

What I've said above may well represent an individual reaction colored more by my own fears and doubts than reflecting the true nature of the book. But there's one final criticism I would make; namely, that Huntington makes many claims or deductions concerning human heredity that are not, in fact, borne out by our present scanty knowledge. He uses raw statistical data from many and diverse sources and fits them to a series of curves that usually have the time element as basic. The curves may well be—and often are—similar, but the conclusion that the diverse data are related by virtue of common causative factors (usually climatic) does not necessarily follow.

But I do come to this conclusion: Huntington *has* something. We cannot accept him without challenge, or reject him without trial.

ANTHROPOLOGY AS AN INTEGRATING SCIENCE

By WILLIAM N. FENTON

BUREAU OF AMERICAN ETHNOLOGY, SMITHSONIAN INSTITUTION

The Science of Man in the World Crisis. Edited by Ralph Linton. 532 pp. 1945. $4.00. Columbia University Press, New York.

GETTING a group of authors to deliver papers written toward a common objective, but from varying points of view, has always been difficult. Ralph Linton, nevertheless, during the past year managed to find among the busy social scientists of the nation the makings of a symposium on the Science of Man; he assembled the papers, con-

tributed two of his own, and he got the Viking Fund to underwrite publication of the volume. This was no mean accomplishment considering that many productive scholars were cut off from writing by restrictions, others were in the armed forces, nearly all were diverted from regular work, and all were in some way disturbed. The book that the group has written is not a plan for the present crisis; instead it attempts to acquaint planners with discoveries and new techniques from the frontiers of research on man and his society. Let us hope that it may not take a generation to accomplish the purpose that this volume intended. In fact, some progress has already been made in that direction. When one hears the radio voice of the Chief Executive employing the terms "race, language, and culture" in their anthropological sense in his closing speech to the San Francisco Conference, one realizes suddenly that the layman is already somewhat familiar with a terminology that implies an objective approach to human relations.

In groping toward the concept of a unified world society, the intelligent layman for whom the book is intended may fairly ask, "What has the student of man got that others lack for understanding the present world crisis?" The answer might be, "A way of looking at men and events, a body of principles, and some techniques of study, which this volume attempts to demonstrate." Then the layman will want to know how well the book accomplishes its purpose.

The twenty-one essays composing this volume fall into several categories: general scope, the biology of man and related values, culture as science, culture and the scientific world view, applications, and special considerations. Naturally, there are lacunae, as there are bound to be in any symposium.

The general reader cannot fail to gain a fair conception of "The Scope and Aims of Anthropology" by reading Professor Linton's introductory chapter. Here anthropology makes a bid for the key position in "a new synthesis of science, especially of those sciences which deal with human beings and their problems." Whether anthropology succeeds as an integrating social science depends in part on the quality and outlook of its personnel. In recent years its disciples have differentiated into specialists who no longer profess anthropology but one of its various branches which Linton defines. Beyond a fundamental split between students of man and students of his works there are all too few good general anthropologists in the present generation. Few can command any more than one subscience within a general field and usually but one corner of a larger area. The modern anthropologist, if he is in the cultural field, is an archeologist, an ethnologist, a social anthropologist, or perhaps a linguist. Seldom is he two of these. Considering the wartime activity of linguists as teachers of languages in Army area and language programs, we cannot agree, however, that "linguistics is, at present, the most isolated and self-contained" subscience of cultural anthropology. Likewise, the cultural anthropologist has been called on to serve on a team of specialists who would control the civilization of a great area. This chapter answers the question "What is anthropology and what are its possibilities?" The serious student of other fields, seeking to knock down the barriers between existing disciplines, may find here hope and understanding of how anthropology may function in a truly integrated science of man. Judging by the contributors to the symposium, Linton has already succeeded in enlisting coworkers from other fields than anthropology.

Three papers treat the relations of biological man to his society. Dr. H. L. Shapiro (American Museum of Natural History) deals with a limited portion of human biology, clarifying "some of the problems that face human biology even though the answers are lacking"—the nature of racial differentiation, patterning and variation in man, race as reality, the taxonomy mania. How will the dysgenic influences of war affect human population? How will talent reassert itself in wartorn populations?

Of the three papers, "The Concept of Race," W. M. Krogman (University of Chicago), has especial significance for both student and layman. The former finds a redefinition of "breed, race, and stock"; the latter must learn: "that biologically there are no fundamental physical differences in all stocks and all races; that biogenetic potentials are shared equally by all stocks and by all races." . . . "Biological equality!"

That race differences are mainly social differences Otto Klineberg (Columbia) maintained in a little book some years ago. His chapter here on "Racial Psychology" summarizes recent evidence that for the "more heterogeneous populations . . . 'race' and psychology appear, [now] . . . to be unrelated." If this is true, the hope for true democracy lies in broadening the educational and economic opportunities for all persons in our society.

Culture is the key concept in the science of man. Yet this is a much misunderstood term. Employing the method of the Socratic dialogue to develop "The Concept of Culture," Kluckhohn and Kelly (Harvard) introduce four anthropologists, a philosopher, a psychologist, a biologist, a lawyer, and a business man. The discussants segregate culture, a way of life, from society, a group

of people; and they distinguish between explanatory and descriptive levels of culture. The dialogue makes for a long essay. The effort to define the concept and make it intelligible to the lay reader doesn't quite come off. The utility of this essay, moreover, is impaired by its having been shortened by one third from a mimeographed draft which the authors circulated among their colleagues. Nevertheless, the layman can discover how anthropologists employ the concept culture and what they mean by it. He probably would like it in shorter compass. This will require a new paper. It is an important lesson for our civilization to learn that the Japs and Germans are not what they are because of their genes.

From an understanding that cultures walk about on pairs of legs it is a short step to realizing that personality types are shaped by the history of individuals living in societies. "The discovery that personality norms differed from different societies" has shocked both ethnology and personality psychology into new approaches to mutual problems. Abram Kardiner (Columbia), an eminent representative of the latter discipline by virtue of his large study *The Individual and His Society* (1939), develops "The Concept of Basic Personality Structure as an Operational Tool in the Social Sciences." Already the union of personalist and ethnologist has produced fertile progeny who operate at levels beyond the reach of old line adherents to either discipline. It is difficult to see yet how the new techniques that have been developed in this field may be applied to current problems because the findings already fly in the face of vested interests, organized on the dominance-submission principle.

Although cultures over the world exhibit great diversity, as ethnological studies have abundantly shown, they also have certain basic similarities. In discussing "The Common Denominator of Culture," Professor George Peter Murdock (Yale; currently in Naval Government) crams into small compass some findings from his own tremendous reading that gave substance to the Cross-Cultural Survey at Yale. "The true universals of culture," he finds, . . . "are similarities in classification, not in content." Thus the common denominator becomes the structure of Wissler's "universal culture pattern." The roots of the single plan he seeks in "the fundamental biological and psychological nature of man and in the universal conditions of human existence." Since cultures have to be learned by individual members of societies, considerable space is devoted to the nature of the learning process, a topic which Dollard (his colleague) takes up in a later chapter.

No living culture is ever static. Culture change claims two chapters. Herskovits (Northwestern) discusses "The Processes of Cultural Change," following an essentially historical approach to cultural dynamics as opposed to "those who hold the nonhistorical point of view 'framing' their investigations in terms of cultural stability." This is an eminently clear and useful statement. It is important to recognize that "the cultural focus of our modern society lies in the field of technology);" "that the inventor of intangibles is rather termed a revolutionist. . . ." Hallowell (also Northwestern) treats "Sociopsychological Aspects of Acculturation," which is an attempt "to understand the conditions and processes involved in borrowing [when peoples meet] and the effects upon the mode of life of the peoples concerned." Although many of its conclusions apply to so-called simpler societies in contact with western civilization, it is equally true that we moderns who find ourselves in closer proximity to neighbors in distant lands can no more go back to "the old life" than the Cree Indians of James Bay.

So much for the anthropological background leading up to Linton's "Present World Conditions in Cultural Perspective." Here culture as science reaches out to embrace the world. Pointing to similarities between the organization of cultures and the organization of individuals, Linton illustrates certain disharmonies and bents; techniques of production known to the society and shifts of interest determine how far such developments will go. Thus social evolution may be measured by certain basic discoveries: tools, use of fire, agriculture, new sources of power, and the scientific method. Thus given the tremendous productive power of modern industry and of scientific agriculture, the student of man finds encouragement in the increasing number of individuals who recognize the need of conscious planning. Lest we feel too smug, Linton reminds us that among the other cultures besides the Western nations it may be some "backward" people who may "adapt the machine to man rather than man to the machine," producing the stable and enduring civilization that we sophisticated moderns have failed, so far, to build.

The contributions of some of the nonanthropologists to the world view are among the more satisfying chapters. Howard A. Meyerhoff (Smith) succeeds admirably in assaying for the nonspecialist "The Present State of World Resources;" and "Population Problems" by Karl Sax (Arnold Arboretum, Harvard) is brimful of interesting leads.

Although important as special problems, it is difficult to reconcile "The Changing American Indian" (Steward) and "Some Considerations of Indianist Policy" (Gamio) with chapters on global problems, when the symposium devotes no

space to the problem of the Arabs and little to the Jews, Negroes, and Japanese-Americans.

What the science of man can contribute to mitigating evils in the perennial colonial problem is more central to the present world crisis. No anthropologist is more nearly aware of this than Raymond Kennedy (Yale). His essay "The Colonial Crisis and the Future" bespeaks clear conception of the problem, vigorous powers of analysis, and a fearless advocacy of international democratic action. Between the free countries, principally in North America and Europe, and the world's dependent peoples, living mainly in Africa, Asia, and Oceania, Kennedy notes a clear pattern of segregation. His universal traits of colonialism are: "the color line; political and economic subordination of the native population; poor development of social services, especially education, for natives; and rigid social barriers between the ruling class and the subject people."

"Applied Anthropology in Colonial Administration," by Felix M. Keesing (Stanford), points out what the anthropologist has and can contribute to easing the transition of native peoples to the changes which contact with modern civilization inevitably bring. Whereas governments have in the past maintained research organizations devoted to building up background information on native peoples, Keesing would have them add a practical research arm designed to aid colonial officials with studies of "culture contact." He wisely abstains from advocating that the researchers become administrators.

In another area, since their reference has been Western Civilization, the reader will find collateral support for many points raised by the anthropologists in "The Problem of Minority Groups," Louis Wirth (Chicago); and in "Techniques of Community Study and Analysis as Applied to Modern Civilization," Carl C. Taylor (Department of Agriculture). The former lends much understanding to events in Europe as well as in this country, and the latter is an introduction to a method of social analysis that has grown *pari passu* as an integral part of scientific agriculture in America.

Of peculiar interest just now is one other paper, "Communications Research and International Cooperation," Lazarsfeld and Knupfer (Columbia and OWI). Communications research is a relatively new field, and the experience that is being had during the war offers much to future educational and informational activities. What are the limits of freedom and of authority in international discourse?

The political scientist, Grayson Kirk (Columbia) warns his colleagues, in winding up the symposium, that "in a world filled with many small states, a few of medium size, and a very few of surpassing power, the development of satisfactory international collaboration . . ." will be most difficult. Whether we can maintain the conditions of collaboration that are indicated remains to be seen. These students of man have had their say. Nonetheless, as Kirk concludes, "The millennium, however, is not at hand."

ANTHROPOLOGY AND THE PROBLEMS OF HUMAN ADJUSTMENT

By MELVILLE J. HERSKOVITS
DEPARTMENT OF ANTHROPOLOGY, NORTHWESTERN UNIVERSITY

The Governing of Men. Alexander H. Leighton. 404 pp. Illus. 1945. $3.75. Princeton University Press, Princeton, N. J.

THE circumstance of the war has done much to underscore in the minds of scientists the social significance of the problems on which they are engaged. Applied science is nothing new so far as the exact and natural disciplines are concerned, since the relationship between science and technology is one of long standing—almost as long as the history of these disciplines themselves. Among the social sciences, however, which are younger, while the immediacy of scholarly study to the everyday affairs of man's existence has long been obvious, the techniques of transfer akin to the work of the engineer in applying the findings of the research physicist or chemist to industry have been almost entirely lacking. Intervention in human affairs is so direct, and can hold such far-reaching consequences, that not only has there been reluctance to assume the heavy responsibility entailed, but many students of society have doubted whether the degree of objectivity essential to the successful intervention of social science in the direction of human society has been achieved.

In the case of anthropology, an interest restricted to studies of the history and processes of human culture and the range of variation in human social institutions began, some two decades ago, under pressures exerted by European Colonial Offices, to make a place for research pointed toward drawing up practical measures to be applied to the governing of the subject peoples of the several empires ruled from the homeland. In the New World, concern with the Indian had a

similar effect—though the ends sought were not the same—as the problem of the Indian in various countries began to press for solutions based on scientific analysis rather than purely polictical measures. Thus in the United States, aside from a tendency to apply some of the techniques of ethnology to investigations of American communities and to certain problems of industry, the Indian Bureau became the principal agent in promoting what came to be termed applied anthropology.

During the past war, when an entire way of life was under attack, anthropologists like other scientists did not pause to argue about the terms on which such services as they might render in putting their specialized knowledge at the command of those who were in need of it to wage battle, were to be employed. For anthropologists, perhaps more than those of any other scientific discipline, had cause to understand the nature of the war that was being fought—a war of ideologies which, if Fascism had triumphed, would have meant the end of their scientific activities except as these might be useful to perpetuate the rule of a self-styled master race. They did what they could, and if what they did contained a considerable measure of improvisation, this was because the questions they were called on to answer were even newer to them than were those put to men in the other sciences.

How old techniques were adapted to new situations is well illustrated in the book under review, the report which Commander Leighton, "a psychiatrist who had some previous experience in studying Navaho and Eskimo communities," renders of the work he and his anthropological colleagues of the Research Unit he headed did in advising as to the administration of Japanese evacuees. For here was one of these new and untried problems, in which the leisurely procedures employed in the researches of peacetime had to be put aside under the urgency of meeting day-by-day demands of human beings under stress and where recommendations had to be couched, not in the long periods of the scientific article, but in short, understandable memoranda to administrators who could make use only of suggestions that could be translated into action.

The setting of Commander Leighton's work was Unit I, Poston, Arizona Relocation Center where, during the month of May, 1942, about eight thousand Japanese evacuees from the West Coast found themselves interned for what, as far as they knew, would be the duration of the war. They represented all the socioeconomic strata of any American minority group, from the bachelor casual laborer in the California fruit-growing industry to well-to-do merchants and lawyers. They represented all degrees of acculturation to the American scene, from the immigrant who had never learned any English to the college graduate who was to be distinguished from his American fellow-students only by his physical characteristics. Finally, they represented the three principal categories of Japanese in America. These were the Isseis, born in Japan and intending eventually to return there, none of whom were American citizens; the Niseis, or "American-born, American-citizen, and American-raised children of the Isseis;" and the Kibeis, or the Niseis whose "formative years had been spent in Japan and who . . . returned to America bringing with them types of behavior that made adjustment difficult in this country" (pp. 67–80).

The experiences of these Japanese immediately prior to their coming to this camp had been trying. Though many of them knew nothing other than American life, and most of them were later proved to be loyal to this country, they had been treated as treacherous aliens. Their funds had been frozen so that it was impossible for most of them to carry on their accustomed trades or, indeed, to earn any livelihood; the FBI had periodically and without warning taken them into custody; some had on short notice been shifted from residences of long standing; the newspapers had inveighed against them; they suspected, not without reason, ulterior motives on the part of competitors who might want them out of the way; they had been threatened with mob violence; they had nowhere to go. That these experiences materially contributed to heighten the tensions and increase the suspicions manifest at Poston and other Centers, suspicions which in Camp I eventuated in the strike that is the climax of the author's narrative, is apparent from the quotations he cites concerning these phases of preevacuation conditions.

One gathers from this book that Pôston was the only one of the ten Relocation camps that permitted research on the part of anthropologists and psychologists, and that utilized their advice. This was because this camp was located on the Papago Indian Reservation, and thus came under the control of the Indian Service as well as of the War Relocation Authority. But it was the Indian Service which, since the assumption by Mr. John Collier of the Commissionership, had turned to the social sciences for assistance in solving the problems of Indian administration. While the experience in using applied anthropology in the Indian Bureau brought advantages to the Poston setting, divided authority introduced a note that did not make adjustment any the easier in a camp

thrown together in the desert, without any of the amenities of American life for the internees who had been accustomed to them, or any chances of attaining the standard of living under which their lives had been ordered before the war.

The situation was almost as difficult for the administrators as it was for the evacuees. Faced with an entirely new problem, they had to feel their way toward solutions under the handicap of dealing with a group of people whose traditional ways were in many respects foreign to the administrators' experience. Out of this arose the reaction on their part that permitted Leighton to classify them into two broad categories, those who were "people-minded" and those who were "stereotype-minded." To the first group, the evacuees were "people first and Japanese secondarily;" to the second, they were "Japanese first and people secondarily" (pp. 81 ff.). This second group, which included minor officials, technicians, and long-term government employees, were found to have an educational background in the main inferior to those who were "people-minded." They were therefore susceptible to patterns of thought that subordinated individual differences to concepts of groups taken as wholes—a state of mind whose far-reaching consequences are amply documented in this book.

There is no space here to detail the frictions that led up to the strike, nor the manner of its settlement, nor how it resulted in a somewhat better adjustment than before, for only a full-length narrative can do justice to this sequence of events. Nor is it to be assumed that the settlement of the strike was due to the utilization of the knowledge of the research staff by those in whose hands lay the decisions that had to be made. A certain amount of such advice, particularly after the strike, seems to have been acceptable. But one need only read of the devious route which Leighton was forced to travel in order to obtain a hearing to realize that a major problem in the application of social science on any level is to induce those who make policy to listen to the recommendations of such experts as they may have available to them.

The importance of this book is not in its record of the incidents at Poston, nor as a report of what Leighton and his associates did there, but rather in the analysis they have made of the situation and the practical suggestions they present to administrators who must deal with groups whose cultures differ from their own. Of importance, too, is the fact that the broad view is never lost sight of. An instance of this is where, in the body of the descriptive text, one encounters passages such as the following: "The precedent established in handling the Japanese has direct bearing on the future security and behavior of these other (minority groups). The treatment of one minority is a rip-cord that leads into the affairs of others, particularly during war when there are unusual opportunities for exploiting the labor of minority peoples, and when the irritations and frustrations of the times easily lead to emotional sprees of aggressive expression at the expense of scapegoats" (p. 53). Of a similar import is the repeated pointing of findings from the Poston experience to the governing of occupied countries (pp. 263, 280, 285, 291, 306, 335, 342); the realization that the Poston strike resembled in its principal characteristics Japanese peasant uprisings (p. 195); or the reminder that "face" is important in American as well as in Japanese tradition (p. 174).

The principal objective of the book is given at the outset where, in the introduction, it is stated how, beneath the problems of adjustment presented by the strike, were matters of deeper import, ". . . the question of what in all this is recurrently human." And this theme is returned to in the second part of the book where, in a series of principles and recommendations to administrators based on these principles, the "recurrently human" elements in the events described are drawn on to give guidance in terms of "the real body of constants and laws in human living," the author feels that "his hands, groping blindly beneath the surface, touched here and there."

In these concluding chapters, the author moves from fundamental postulates such as the basic unity of mankind and the great variation in the customs of differing groups of men—with recommendations as to a general approach to problems involving a plurality of cultures that most of us might profitably remember if we are to adapt ourselves to the demands of a world society—to more specific considerations. These are broken down into three categories, which derive from the psychocultural approach employed, as they relate to individuals under stress, to systems of belief under stress, and to social organizations (a term unfortunately employed in place of the better designation "culture") under stress. Finally, there is an historical and methodological appendix written by the author and one of his associates, Dr. Edward Spicer, which tells of the organization of the research unit, how it worked and how its materials were organized, and indicates the results achieved by it, both in terms of publications issued and advice accepted by the administration it was set up to serve.

This book, of course, does not resolve the fundamental and debated issues concerning practical

or applied anthropology—its scope, its subject-matter, its aims, or the basic moral problems that arise when one contemplates the direction of human conduct that is its ultimate goal. It does not do this because such questions are not within the limit of its intent; nor, indeed, are they even implied in its treatment. Its author has taken a specific situation that was fruitful for analysis, presented that situation in terms of a conceptual framework well thought through, treated his data with insight and consideration for the human values no less than the administrative requirements. Herein lies its strength as a scientific document, its persuasiveness as pointing a way toward an applied anthropology that can perhaps utilize with effectiveness and with restraint the findings of research that are not so immediately germane to everyday affairs.

It has been said that applied anthropology, if it is to establish itself as a recognizable branch of the broader discipline, must succeed in applying anthropological knowledge with objectivity so as to yield insight into our own behavior, and to lead to the intelligent solution of our own problems, before going further afield in attempting to solve the problems of other societies. It would seem that in the orientation of his book no less than in the handling of his materials, Commander Leighton has made a good beginning toward establishing this as a recognized aim, and through it toward setting up an applied anthropology that begins to make sense.

ANTHROPOLOGY APPLIED TO AMERICAN PROBLEMS

By MORRIS E. OPLER

OFFICE OF WAR INFORMATION, WASHINGTON, D. C.

The Social Systems of American Ethnic Groups. W. Lloyd Warner and Leo Srole. 318 pp. Illus. Vol. III. 1945. $4.00. Yale University Press, New Haven, Conn.

THIS is the third of a series of six volumes which together will sum up the efforts of twenty-five writers, analysts, and field workers who made an intensive investigation of an old New England community which they call Yankee City. This particular book, according to the preface, is offered as a "detailed study of the social life of a number of ethnic groups, including the Irish, French Canadians, Jews, Armenians, and Poles." The authors explain that they are applying the same techniques and viewpoints which social anthropologists use in a study of societies of simpler peoples.

In an attempt "to communicate how it feels to be a member of an ethnic group" the book begins with sketches representing episodes in the lives of seven "fictive" Irishmen. These are designed to convey the interests, social status, manner of speech, and degree of assimilation of the Irish "ethnic" of various age and occupational groups. Then follow chapters on the spatial distribution of members of the several ethnic groups, their economic activities, the social status each group has attained, and the influence of the form of the family, the church, parochial or language schools, and various organizations on the life of each group. The last chapter is a theoretical discussion of the rate of ethnic and racial assimilation in America.

The story of large-scale immigration to this country and of the adventures of the immigrants and their children is an important one, and, though many books have been written on the subject, it is a tale well worth repeating from various vantage points and in relation to certain localities. Most studies of immigrants and their children deal with one group only. This study has the merit of discussing the eight such groups of Yankee City, of indicating the time and tempo of their arrival, of describing the backgrounds from which they came, and forces which set them in motion, the reception which they received, their relations to each other, and the characteristics of the adjustments they have made.

In respect to any one group nothing very startling or unexpected is revealed. The Jews, for instance, are pictured as first living in the poor and crowded section of town. The immigrant men established a small synagogue and continue to wear beards. They refuse to engage in economic activity which would force them to work on Saturday. They insist that their children attend Hebrew school. The women have little to say about religious matters or community affairs. Those of the second generation, however, engage in more diversified economic activities, are decidedly less orthodox religiously, and scatter more spatially. They meet a certain amount of intolerance and opposition in their attempts to buy property in desirable residential sections and to locate their new synagogue in more pleasant surroundings, but their economic and social rise is steady. By itself this a familiar tale. But it takes on meaning and perspective when considered against the background of the strivings and achievements of other groups as well.

There are, however, a number of questions which are automatically raised by the manner of treatment of the data which is attempted. One has to do with the very definition of an "ethnic." On page 28 we are told: "The term ethnic refers to any individual who considers himself, or is considered to be, a member of a group with a foreign culture and who participates in the activities of the group. Ethnics may be either of foreign or native birth." This will seem to many a loose way for the social scientist to determine the groups which are to be analyzed. No doubt the average man in the street believes that Jews constitute a special race. That does not entitle the physical anthropologist to list the Jews among the races of man. It is assumed that the scientist has some criterion for classification besides that of gossip or popular ignorance. In like manner, it is probably true that there are those who flatly claim that any Irishman is "a member of a group with a foreign culture." But there is no good reason why such prejudice should be accepted as a scientific guide. The failure of the authors to define the term *ethnic* as an individual who *actually is* a member of a group with a foreign culture rather than an individual who considers himself, or is considered to be, a member of a group with a foreign culture leads to a fundamental confusion. The authors are obviously aiming at an analysis of matters of culture or social behavior. They are trying to indicate for us to what degree an Irish subculture or a Polish subculture persists in the American environment. In science, particularly in social anthropology, a clear distinction is made between culture (learned behavior) and ancestry (heredity). But one of the vulgar sins of our time is that persons are constantly being classified culturally, regardless of the facts, according to the identity of their parents and grandparents. This is the misconception which reached a logical extreme in Hitlerism. It is discouraging to see the authors come dangerously close to what is essentially the same mistake.

Thus, the first "ethnic" we meet, the fictive Irishman, Dr. Daniel Corbett, has no discernibly "foreign" characteristics. He has grown up in Yankee City. He is a successful dentist, but over a few highballs confides to a friend that he is "in a jam" and "right behind the eight-ball." The jam seems to be his indecision about whether to spend the week end in a Boston hotel with a young widow of easy morals or to go home to his wife, Molly. His dissatisfaction with Molly arises from the fact that, because she is a devout Catholic, she refuses to practice birth control. The widow, apparently, has no such scruples. As a result of the size of his family and the attendant expenses, Corbett is not able to climb the social ladder fast enough or to live in the most fashionable district of town. The story has a happy, or at least a decent ending, however, for this time, after unburdening himself to his friend in good American slang and profanity, he decided against visting the widow.

Corbett is a descendant of an Irish immigrant. Except for that genealogical detail it is difficult to see anything peculiarly Irish about him. Certainly the fact that he and his wife are Catholics cannot be accounted a "foreignism." Some of the earliest settlers in this country were Catholics. And there are Protestant fundamentalists who are just as strongly opposed to birth control as is Molly. The incongruity here stems from choosing members of an ethnic (culturally defined) group on a genealogical (biological) basis. This is done not only in Dr. Corbett's case, but for all the denizens of Yankee City. They all become members of an ethnic group or not, according to ancestry. As a result the authors end up with approximately half of all the residents of Yankee City members "of a group with a foreign culture," which is, on the face of it, an absurdity.

The authors claim that in writing this volume they have utilized the same techniques and viewpoints which they apply to the study of societies of simpler peoples. That may be, but it is also certain that they have used techniques and types of evidence which many social anthropologists would hesitate to employ. I do not think the social anthropologist ordinarily accepts as valid evidence concerning a people the pronouncements about them which come from none-too-friendly neighboring tribes. Yet the authors lean heavily upon characterizations which members of one "ethnic" group use to describe members of another.

For instance, source materials such as the following excerpts are used to prove that the immigrant generation is usually over-solicitous about money and savings:

An F_2 Irish attorney, who, like all the local Irish, does not class the Irish among the ''foreigners,'' made this observation: ''These foreigners are funny people. They will work in the factory and make good money, but they will never spend any of it. . . . They save their money and when hard times come they will go the welfare board to get aid and at the same time are liable to have money in the bank'' (p. 81).

A Welfare officer reported of the Italians: ''The Italians have raised a great stink because they haven't received any Red Cross flour. So the cases of all the Italians were investigated, and it was found that almost all of them had $300 or more stored away and yet they were hollering for free flour'' (pp. 81–82).

A more tragic incident occurred in a case of which

a native nurse tells: "The Poles don't like to spend money. There is one family here that own their home. The woman has cancer and the doctors have told her for a long time that she ought to have X-ray treatments. But they wouldn't spend the money and now it is too late" (p. 82).

How much a third-generation American lawyer who starts a conversation by observing, "These foreigners are funny people," is likely to really know about the home life and economics of the Polish immigrant is a moot question. Moreover, it would be interesting to know whether welfare records bear out the charges of the welfare officer or whether one or two cases were multiplied in the interests of obvious prejudice. That the immigrant woman who spurned advice concerning X-ray treatments did so because of the money involved is one explanation. There have been so many similar cases where fear of a doctor or of medical treatment was involved that it is difficult to take the nurse's reasoning as final. All in all, much of this evidence sounds like the "anti-relief" mutterings which arose among a section of those who were still solvent during the depression of the 1930's.

In the last chapter the authors take up the question of assimilation and finally provide what they name a "Table of Ethnic and Racial Assimilation." Hitherto most writers on the subject have defined assimilation in terms of the behavior patterns of the immigrant or minority group and the degree to which its members have approximated the standard usages and acceptances of the greater society. In this book the word is read to mean the degree of resistance which the larger group offers to the presence and economic and social movement of the smaller. Amalgamation and the rate at which a group disappears, rather than cultural adaptability, seem to be the criteria. Thus the Negroes and the Orientals are said to assimilate "very slow" and to be subject to "great to very great subordination." This shift in definition, without adequate discussion or clarification, will seem to the thoughtful to be most unfortunate, especially at the present time. It plays into the hands of elements which are asserting that Negroes and Orientals are unassimilable and that we need more drastic segregation and further immigration restriction. I cannot help thinking that if one-half of the energy that went into the preparation of novel statistical and tabular arrangements for this book had been saved for thinking through the elementary concepts with which the authors deal, the results would have been of more enduring value.

It is decidedly an overstatement to call this book a study of the social systems of American ethnic groups or to say that it employs the methods of social anthropology. It is a study of the social and economic mobility of the immigrants who came to these shores and to Yankee City after 1840 and of their descendants. In this more delimited field a very careful and able job, largely statistical in approach, has been done. To those who are interested in questions of social mobility, the volume is heartily recommended.

NEW TECHNIQUES FOR ANTHROPOLOGICAL RESEARCH

By A. IRVING HALLOWELL

NORTHWESTERN UNIVERSITY, DEPARTMENT OF ANTHROPOLOGY

The Psychological Frontiers of Society. Abram Kardiner et al. 475 pp. Illus. 1945. $5.00. Columbia University Press, New York.

THIS volume is a distinguished contribution to that relatively new level of synthesis in the social and psychological sciences which is emerging from an increased awareness of the potent contributions that traditionally distinct disciplines can make to the scientific study of man through coordinated knowledge and the use of special techniques.

It was not so long ago that cultural anthropologists were hardly aware of the psychological significance of their data, since they were almost exclusively preoccupied either with describing and analyzing the varied cultures of nonliterate peoples, or with examining problems of a culture-historical nature. Any problems of a psychological order were considered to be in the domain of other groups of specialists. As Linton remarks in his Foreword to the present volume, "The old-style investigator was much more interested in lists of relationship terms and descriptions of religious rituals than in how children were brought up or what were the attitudes between spouses" (p. x). On the other hand, the raw data of cultural anthropology, which rapidly accumulated in the first decades of this century, were by no means ignored by all specialists in human psychology. Some of them, particularly psychoanalysts, made occasional use of anthropological material in terms of their own psychological conceptions, not always to the satisfaction of the anthropologist.

In recent years the mutual recognition of an important frontier region between psychology and anthropology has been focused chiefly in the exploratory studies that have been undertaken in order to discover the reciprocal relations between the genesis, typological range and functioning of human personalities and the differential cultural systems described by the anthropologist. For, as Linton points out, "Except in the most superficial terms, the workings of society and culture cannot be understood without constant reference to the needs and capacities of the individual. Conversely, particular individuals cannot be understood without constant reference to the cultural and social environments in which they have developed and within which they have to operate. The really pressing problem is one of ways and means; how the findings of the various sciences involved can be integrated and applied" (p. v).

Dr. Kardiner's book is a demonstration of "ways and means." In the Preface he says, "It describes and perfects a technique and claims some degree of precision in tracking down the reciprocal relations between culture and personality." This technique of analysis which Kardiner has been developing over a period of years received its initial application to the problem of personality and culture in *The Individual and His Society* (1939), the outcome of analysis and discussion with anthropologists interested in the same problems. At Columbia University Linton and Kardiner have collaborated in a joint seminar for a number of years. For any detailed understanding of the author's contribution to the study of personality and culture this earlier volume must be consulted, and for a comprehension of the more technical aspects of his theoretical position, *The Traumatic Neuroses of War* (1941) as well.

The present volume, then, is a sequel to *The Individual and His Society.* It embodies (a) further refinements of the author's operational concepts, specifies in greater detail the constituents of what he calls "basic personality" (Chap. II) and discusses the origin of value systems and social goals; (b) the concrete results of his method as applied to entirely new data (there is a summary and interpretation of the field work done in Alor, Netherlands East Indies, by Cora Du Bois and in *Plainville, U. S. A.*, by James West); (c) the checking of interpretations through the use of two independent sources of information, biographies and the Rorschach technique; and (d) a condensed account (Chap. XIV) of a projected volume which will apply the same technique of analysis "to the main currents in Western civilization."

Since this is not a technical review, it will be unnecessary to go into the refinements of Kardiner's technique; but in order to appreciate the kind of task he has set himself, his basic approach must at least be understood. For the operational tools he has developed and the kind of analysis he assays have been derived from certain questions which he insists a psychology fruitful for the study of personality and culture must be able to answer. This is why he frankly states in his Preface that, "This work stands or falls by the correctness of the psychiatric technique employed. The question resolves itself to this: Is our conception of the path of integrational processes in man correctly delineated, and is our notational system accurate enough and delicate enough to catch all modalities of adaptation?" (p. xix). Kardiner's conceptual approach to human behavior is dynamic and holistic rather than atomistic. He views psychology as the science of human adaptation (p. 5), "Its determinants, its modalities, and motivations, and of the mental and emotional phenomena that accompany the viscissitudes of adaptation. Psychology attempts to describe the minutiae of adaptation and to explain sequences of varying orders which are not apparent to common sense." But psychology is not a homogeneous science and of the many techniques available some are better "suited to special assignments though none can claim universal applicability." He maintains (p. 21) that a psychology suited to the needs of social science must be equipped with operational tools that provide access to the emotions and motivations of individuals and that transcend a single facet of adaptation, for example, perception. Above all, "It must be a genetic psychology in order to follow integrational systems from their inception, and to be able to identify their viscissitudes and modalities under the influence of various drives and the effects of controls imposed on the drives and impulses. It must be able to account for the adaptative role of fantasy and must be able to detect the emotional constituents of rational thinking . . . to track down the sources of the affectivity potential of the individual, the capacity for idealization, the patterns of self-esteem and aggression, the psychosomatic patterns, the superego formation, and the like. It must be able to analyze such complicated end products as values, ideals, religion, for these are the currency of the social life of man." "The only psychology that can approach these problems with any hope of success," he says, "is psychoanalysis." But although Kardiner is a practicing analyst, he rejects the use of "instinct" in the orthodox Freudian sense as an operational concept of heuristic value. In fact, he pointed out in *The*

Traumatic Neuroses of War (p. 137) that, like the classical psychologies and behaviorism, psychoanalysis in its use of "instinct" is an atomistic psychology. In that book he adopted what he called the "action syndrome" as his operational unit. In his present book he designates this large molar unit as "action system" (p. 21), and remarks the advantage it "enjoys is that it can be identified by cognitive, coordinative, and executive constituents; whereas the 'instinct' had to be identified by qualitative criteria only" (p. xvii). No matter how psychologists may view Kardiner's evaluations of other psychologies, he has made his own theoretical position and aims explicit; and his revision of psychoanalytic concepts he has tested, not only in his books dealing with personality and culture, but in the volume on the war neuroses.

In *The Individual and His Society* Kardiner introduced the concept of "basic personality structure" in order to identify certain psychological constellations shared by individuals of one culture as compared with another. This concept proved a useful one in making intersocietal comparisons, whereas "character" (differences between individuals) sufficed in an intrasocietal context. Basic personality, in short, "only indicates a certain range and certain modalities within which great differences can arise," a matrix, as it were, in which variant character traits develop. Useful as the concept of basic personality structure is, Kardiner is quite right in saying (p. xix) that the merits of his work by no means depend upon it. He adopted it, he says, "to obviate the lack of clarity in the terms group, national or social character, because a group can no more have a common character than it can have a common soul or pair of lungs. . . . What was new and important about this concept was not its name, but the technique of its derivation and the introduction of a genetic point of view into sociology. The concept of basic personalilty structure thus became a powerful operational implement, for through it we acquired a precise means of delineating the interrelationship of various social practices through their compatibility or incompatibility with certain constant identifiable human needs and drives" (p. 24). The reader will need to study Kardiner's exposition carefully in order to see exactly how he arrives at the basic personality structure of a given culture. It must suffice here to point out that basic personality constellations comprise a hierarchy of integrational systems that cannot be fully explained by any mechanistic theory of learning, but which originate in the actual experiences of individuals as a result of a socialization process that starts in earliest infancy. It is the author's cardinal contribution to have approached this problem in precise etiological terms from the genetic standpoint. He has also shown how these integrational systems are reinforced by later experience and has attempted to demonstrate predictable connections between different types of basic personality and the differences in attitude-value systems in various cultures. In other words, basic personality structure becomes the unconscious basis of certain projective systems (like religion or folklore). Such systems may be contrasted with reality systems; that is, devices for adapting oneself to the world through rational or realistic means. The culture of different societies is polarized in one direction or another.

Whereas in his previous book Kardiner was compelled to make all his psychological inferences on the basis of descriptive ethnographic data collected without psychological problems in mind, and therefore had no way of checking the conclusions reached, the opposite is true of the present volume. The nature of this substantiating data can best be appreciated by reading *The People of Alor* by Cora Du Bois, where a fuller account of the manner of life of these people is given, a number of biographies analyzed in detail by Kardiner, and a summary report made of the interpretation of the Rorschach data by Dr. Emil Oberholzer. The Rorschach interpretation was done independently, and its substantiation of Kardiner's interpretation of the basic personality structure of the Alorese is truly impressive.

One final problem, incidentally discussed in connection with the analysis of the Tanala in the earlier volume and the Comanche in the present one, is the relation of basic personality structure to cultural change. It is this question that Kardiner has initially surveyed in his final chapter and promises to analyze in a forthcoming volume.

The scientific importance of the work of Kardiner and his collaborators lies in the effort they have made to devise ways and means of identifying the actual etiological factors that are responsible for moulding the human personality under varying conditions, the constant checking of hypotheses and the avoidance of a dogmatic attitude with respect to final conclusions. When the method is tested further and the results of additional studies are before us, we may find that one of the cornerstones of a scientific social psychology has been laid.

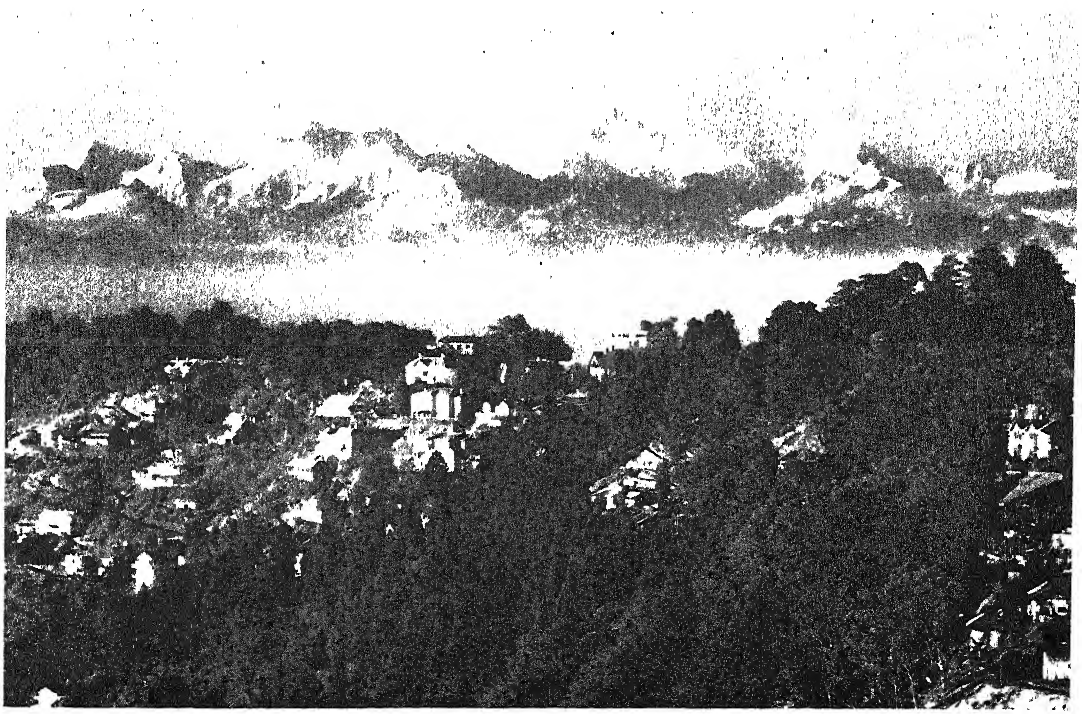
IMPRESSIONS OF INDIA

By RALPH W. PHILLIPS
SENIOR ANIMAL HUSBANDMAN, U. S. BUREAU OF ANIMAL INDUSTRY, BELTSVILLE, MD.

WHEN India is mentioned, Americans may think of vastly different things: One may think of rich palaces, stately tombs, the oriental pomp and splendor for which India has long been legend. Another may think of the poverty of the masses, and the lack of knowledge and appreciation of facilities and practices that are essential to sanitation and health. Still another may think of religious fanaticism, of creedal differences between Mohammedan and Hindu, and of confused politics so intimately interwoven with religion that it seems impossible to separate the two. Others may think of a caste system based on far different concepts and sociological foundations than our own ideals of life, liberty, and the pursuit of happiness, or of sacred cows that man dare not violate, even to end their misery when they are mortally injured or ill.

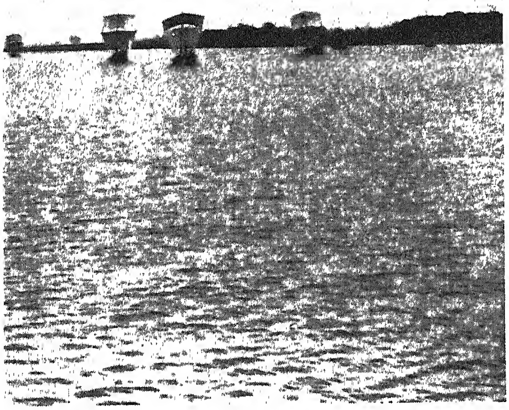
India is all these things and many more. This varied country might well be described as a land of contrasts.

The towering Himalayas bound India on the north, and this range bends southward into Burma on the east, where it is popularly known as "The Hump" by American airmen. In the northwest lie the dry hills of Baluchistan. Below these mountains lies a vast plain. Through this plain flow the three mighty rivers of India, the Indus in the west, the Ganges in the east-central portion, and the Brahmaputra in the east. The two latter have formed a vast delta, and below where they join they flow to the Bay of Bengal through an intricate and shifting network of waterways. All three rivers are fed from the snowfields of Tibet. The eastern portion of the plain through which these rivers flow is low and wet but it becomes



Das Studio

MOUNT KANCHANJUNGA, SEEN FROM DARJEELING
KANCHANJUNGA, THE HIGHEST PEAK IN THE PICTURE, TOWERS 28,156 FEET. IT IS SECOND ONLY TO EVEREST.



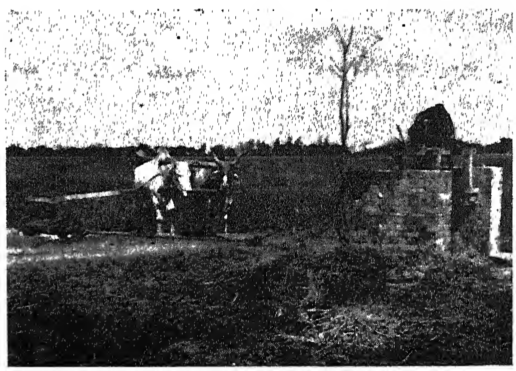
LOW, WET COUNTRY IN BENGAL
THESE BOATS ARE ON ONE OF THE MANY CHANNELS THROUGH WHICH THE COMBINED GANGES AND BRAHMAPUTRA RIVERS FLOW TO THE BAY OF BENGAL.

drier towards the west. In those parts of the western plains where the rivers do not flow (Rajputana and Baluchistan) the land is a desert. All the regions just described may be called roughly northern India. The region just south of this plain is composed of a series of low mountain ranges. Southern India has a mountain range which extends along most of the eastern side of the peninsula. The narrow space between these mountains and the western coast contains steppes which descend abruptly to the sea. To the east of the mountains there is a broken plateau and beyond that lie the plains of Madras.

The climate of India is quite variable. Southern India is hot throughout the year. Madras, for example, has a mean temperature of about 81 degrees F., ranging from a mean of 75 in January to 87 in May. In the Indus valley in northwestern India, temperatures range from 30 degrees F. in January to 125 degrees in May. The southwest monsoon is perhaps the most significant factor in India's climate. This midsummer wind brings a punctual but sometimes inadequate rainfall to most of the country, and economic life depends to a large extent on this rainfall. The annual rainfall varies from less than 5 inches in the Sind desert to more than 500 inches in the Khasi Hills of Assam.

In this land of geographical contrasts there are 1,581,410 square miles of earth. These do not constitute a political unit. The provinces of British India occupy 865,446 square miles while 715,964 square miles are included in the Indian States and Agencies. There were 388,997,955 people tabulated in the census of 1941. Of these, 295,808,722 were in British India, while the remaining 93,189,233 were in Indian States and Agencies. They differ widely in their racial origins, religious beliefs, customs and modes of living. Approximately 87 percent of these people live in rural areas.

India is too large and too varied a country



BULLOCKS AT WORK IN NORTHERN INDIA
Left, PULLING A PLOW ON THE PLAINS; *Right,* FROM A SHALLOW WELL THE ANIMALS LIFT IRRIGATION WATER.

A TYPICAL VILLAGE SCENE
THE VILLAGE POND OR TANK CONSTITUTES ONE OF THE MAJOR SANITATION AND HEALTH PROBLEMS IN INDIA.

to know intimately after only a few months. But like all other distant parts of the globe, India is being brought nearer to America by steady improvements in communications and transportation, and it is increasingly important that we Americans know more of these once-distant lands that are now becoming our near neighbors in world affairs. So the viewpoint of one who studied the livestock and the animal husbandry institutions of India and pondered over the problems that must be surmounted in order that the livestock of India might serve that country more effectively may add something to our understanding of India and her people.

My visit in India occurred during 1943 and 1944, while en route to and from China on a special mission for the Division of Cultural Cooperation of the Department of State. I have already referred to the oriental pomp that is part of India's heritage. It was with considerable pomp that I entered India on the backs of four turbaned stretcher bearers. Illness had overtaken me while en

CATTLE AND WATER BUFFALOES
AS THESE ANIMALS MOVE TO PASTURE, THEY ARE USUALLY HERDED BY THE YOUNG OR OLD OF THE VILLAGE.

POMP AND CIRCUMSTANCE
LARGE BRASS IDENTIFICATION BADGES ARE WORN BY GOVERNMENT ATTENDANTS OR MESSENGERS CALLED PEONS AND BY SIMILAR SERVANTS OF BANKS, ETC.

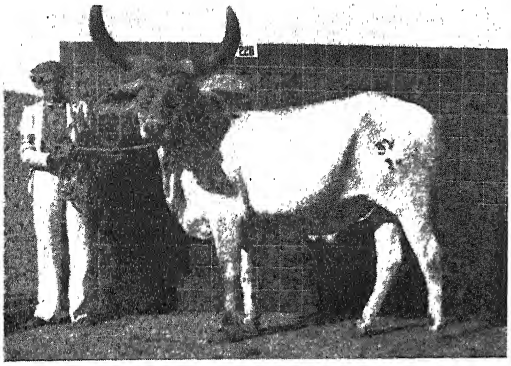
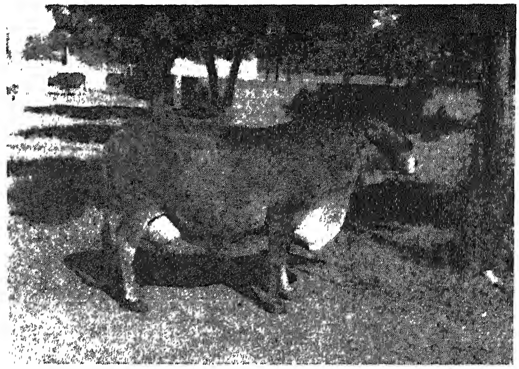
route from San Francisco to Bombay, so my first impressions of India were from ambulance and hospital windows; glimpses of brilliant-colored flowers, of costumes so varied they shock one accustomed to the relative uniformity of American fashions, and of motor, animal, and man-powered traffic moving along the "left" side of the streets. Recovering sufficiently to leave the hospital in Bombay, I traveled to Calcutta on one of the very few air-conditioned trains in India, and from there went to Darjeeling to enjoy the mountain air and regain strength before crossing "The Hump" to China. My real opportunity to see India came after completing a mission in China. At the invitation of the Government of India I was privileged to travel through much of the country, under the auspices of the Imperial Council of Agricultural Research, inspecting animal husbandry institutions, and visiting livestock-producing areas.

India is primarily an agricultural country, and a large portion of the population lives close to the soil, either on farms or in villages from which they go forth to till the surrounding fields. Farms are generally small and tools are primitive except in relatively small areas where improved equipment has been introduced. Under these farming conditions animals are essential for power. There is relatively little feed for types of livestock other than those used for power since most of the grain and other food produced is required for human consumption. Approximately 75 percent of the cultivated area is in cereals, the two principal crops being rice and wheat. Sugar, cotton, jute, and tea are also important crops. In the drier regions and in some areas where crop farming has not been developed, pastoral pursuits are more common, and cattle, sheep, and goats are used to harvest grass and other forage, turning them into products that are edible or otherwise usable by man. Horses and donkeys are also used to some extent in the drier areas, and camels are important in northwestern India. Because gasoline is expensive, many bullocks and other animals are used for draft and packing in cities and on the highways, roads, and trails.

The recent famine in Bengal was one of the major catastrophes of the war, and a study of the factors contributing to it shed much light on the nature of Indian life and economy. The following statement made by one witness before the Famine Inquiry Commission gives the background concisely:

> Until Japan declared war, India had no serious food problem beyond the fundamental truth that two-thirds of its population normally existed at a level little above the starvation line, and by western standards, well below it. That in itself is an important fact, the effect of which is to be fully grasped before the true significance of the situation which ultimately developed can be understood. The effect was that a slight disturbance of the economic practices of the country, and a small diminution of the overall available supply, had consequences altogether out of proportion to their intrinsic gravity. So delicate was the balance between actual starvation and bare subsistence, that the slightest tilting of the scale in value and supply of food was enough to put it out of the reach of many and to bring large classes within the range of famine.

Such a shifting of the balance in value and supply of food took place. The winter rice crop in 1942 was deficient, and the stock carried over from 1942 to 1943 was below normal. The provincial government failed to control the price and distribution, and many found this basic food beyond their means.

A KANKREJ BULLOCK AND A SAHIWAL COW
Left, THIS TYPE IS USED PRIMARILY FOR DRAFT; *Right,* THIS IS ONE OF THE FEW MILKING BREEDS IN INDIA.

Loss of normal imports further aggravated the situation, for imports from Burma were cut off and restrictions were placed on the movement of grain from one province to another. The tragic results are summed up in the following quotation from the report of the Famine Inquiry Commission:

> What happened was that producers sold their rice as they thought fit at the best price they could obtain, or held it in the hope of still higher prices. Traders bought, held and sold with the object of obtaining maximum profits, and consumers who could afford it bought as much as they could and not as much as they needed. The results were on the one hand unprecedented profiteering and the enrichment of those on the right side of the fence; on the other, the rise of prices to fantastic heights and the death of perhaps one and a half million people. It has been reckoned that the amount of unusual profits made on the buying and selling of rice during 1943 was 150 crores [112 million pounds sterling].

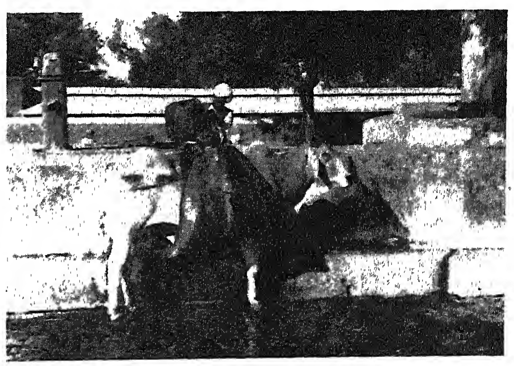
In the meantime, the country was suffering from a shortage of transportation. Further complications arose owing to lack of coordination among government agencies and the public. The starvation and the epidemics which followed bring into bold relief the narrow margin between bare subsistence and actual starvation that exists in much of India.

In Table 1 is a comparison (Morris, 1945)

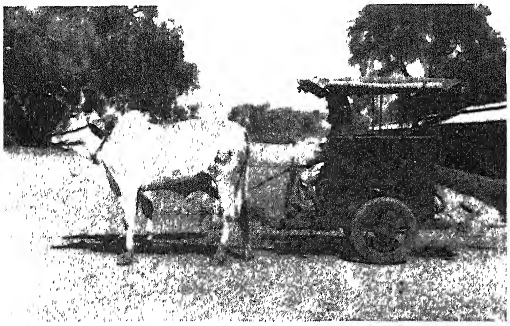
TABLE 1

THE NRC OPTIMUM DIET VS. THAT OF THE INDIAN RICE EATER

Diet	Calories	Protein (grams) Total	Protein (grams) Animal	Calcium (grams)	Iron (mg.)	Vitamin (I.U.) A	Vitamin (I.U.) B_1	Vitamin (I.U.) C
Optimum ...	3000	70	33	0.8	12	5000	594	1500
Indian	1750	38	1	0.16	9	500	160	300

A KANGAYAM BULL AND A HISSAR BULLOCK
Left, A DRAFT BREED OF SOUTHERN INDIA DEVELOPED BY THE PATTAGAR OF PALAYAKOTTAI; *Right.* WATER BAGS.



A BULLOCK TONGA
THESE LONG-LEGGED BULLOCKS ARE ABLE TO TROT.

of important constituents of the diet of the typical poor Indian rice-eater with the optimum diet recommended by the National Research Council. It further emphasizes the low level of nutrition upon which many Indians exist.

In the face of this low dietary standard, Indian population increased by more than fifty millions from 1931 to 1941. Thus, at this rate of increase, there is the problem of feeding and otherwise supporting over 5,000,000 additional persons each year.

The Cattle Problem in India

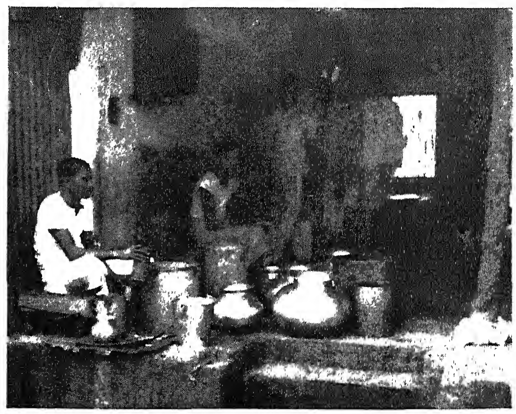
Cattle are by far the most important livestock in India. The agricultural structure of the country is based largely on the use of cattle for draft purposes. Bullocks also furnish much of the power of transportation. Many of the native types are used entirely or primarily for draft, but they provide some milk for human use. European dairy breeds have been imported, particularly bulls, for crossing with native cows to increase the milk supply. Beef is consumed by some portions of the native population and by the foreign groups. Hides and bones, salvaged from dead animals and from slaughterhouses, are important by-products. Manure is used for fuel. In addition to all these functions, cattle are intimately associated with Hindu mythology and religion.

Since the cattle occupy such important places in Indian agriculture and in the religion of the major portion of the people, an understanding of the place of cattle in national life sheds much light on the problems of agricultural, economic, and social improvement.

In the census of 1940, the number of cattle of all ages tabulated was 162,729,555. This number includes figures for a few areas obtained in years other than 1940. The actual total is probably somewhat larger since some of the Indian States did not participate in the census.

For many centuries the majority of the people of India have been vegetarians. Therefore, they are in greater need of milk and its by-products than people of most other countries.

Milk and milk products provide the chief source of animal protein in the Hindu diet, but little concrete information is available on the milk production of Indian cows, outside a relatively few animals in government-owned or sponsored herds. However, a survey has been made which gives some information on the village cattle (Imperial Council of Agricultural Research, 1939). It was made in seven typical breeding tracts and was conducted during the winter months. The average daily milk yield observed for cows was 3.74 pounds, and the estimated yield per lactation was 943 pounds. Water buffaloes were also included in that survey, and the figures for daily and lactation yields were 7.82 and 2,160 pounds, respectively. The average milk production per capita in the areas studied was 23.36 ounces, while the consumption of milk per person was 10.16 ounces. This included the ghee, curd, and other milk products expressed as milk



AN INDIAN DAIRY SHOP

equivalents. The remaining 13 ounces were sold.

The average daily consumption (ounces) of fluid milk by persons of various ages and the two sexes is of interest since it apparently reflects the predominant position of the male, particularly in the Hindu family:

Age Group	Male	Female
Adults	6.63	3.34
Children	5.82	3.77
Infants	3.77	2.53

It was also observed that the proportion of females receiving milk was uniformly less than that of males.

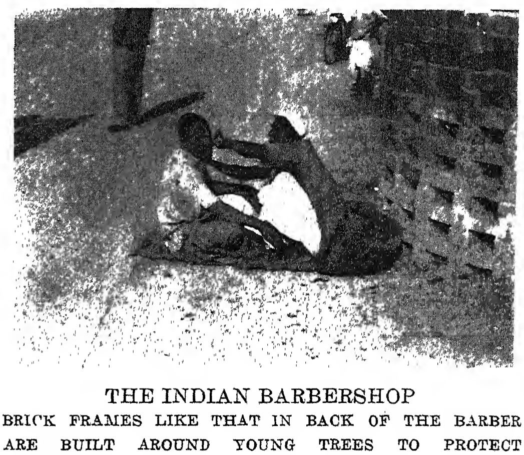
It should be emphasized that the data on milk production and consumption summarized above were obtained in tracts where cattle breeding is an important pursuit. In other areas, where breeding is of less importance and many working bullocks are purchased, the available milk supply would undoubtedly be lower. One authority has estimated that the supply of fluid milk for the whole population is less than one and one-half ounces per day.

Definite data on milk consumption in the cities are not available. Some milk is carried into cities from country districts but most of the milk in the larger cities is produced within the city limits. For example, it is estimated that approximately 50,000 cattle and buffalo cows are imported each year and are quartered in the city of Bombay for milking.

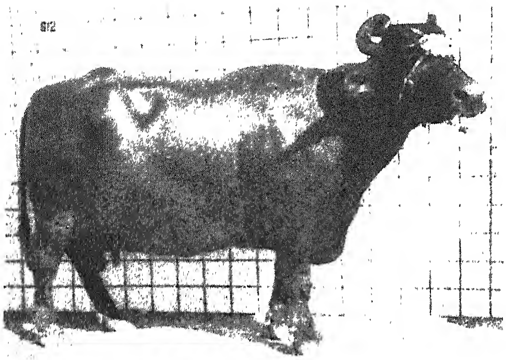
India is a land of castes. Each caste is a sort of trade union and its members have been doing the same type of work in much the same way for many generations. Members of a number of pastoral castes, such as the *Ghosi* and *Gwale*, have long combined the breeding of cattle with the raising of crops. These pastoral castes once had plenty of free pasture land upon which to graze their cattle. Bakhsh (1944) points out that the security of life and property afforded under the British government has given impetus to tilling of land, so that every available piece of ground is utilized for the growing of crops. Under these conditions, many of the members of these pastoral castes have migrated into large towns and cities.

MANURE FOR FUEL
CATTLE DUNG IS COLLECTED, CAKED, DRIED, STORED.

These transplanted pastoral people constitute some of the most backward communities in India. They are too poor to afford good housing for themselves or their animals, so usually live in the poorest sections of the towns and cities. They often lack sufficient capital to purchase cattle, so must borrow



THE INDIAN BARBERSHOP
BRICK FRAMES LIKE THAT IN BACK OF THE BARBER ARE BUILT AROUND YOUNG TREES TO PROTECT THEM FROM THE CATTLE THAT ROAM THE STREETS.

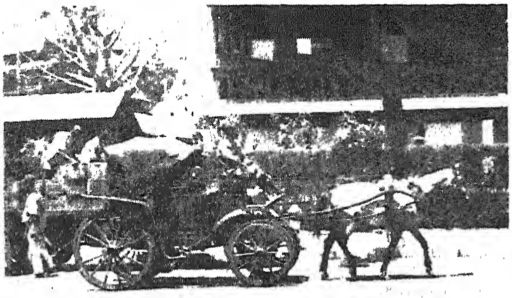


A MURRAH WATER BUFFALO COW
"MURRAH" MEANS CURLED, SO THIS BREED IS NAMED AFTER THE CURLED HORNS. IT IS THE MOST IMPORTANT OF THE BREEDS OF WATER BUFFALOES.

from the *bania*, or money lender, at an exorbitant rate of interest. Under these conditions the owners of the cattle are constantly in debt and have no opportunity to better their own conditions or those of their cattle.

Under the conditions generally existing, milk delivery methods are quite primitive, and standards of sanitation commonly practiced in the United States are unknown. A limited amount of milk is delivered in capped bottles in the larger cities, but much of it is delivered by other means. In some areas, according to Wright (1937), it is usual for milk to be sold in bazaar shops; in others some of the milk is delivered to the purchaser's home and some is sold in retail shops, while in others the cow is driven from door to door and milked in the presence of the customers. Milking is done almost universally with wet hands. Transport to market is by head load, bicycle, or cart. The vessels used frequently have no lids and the mouth of the vessel is therefore generally stuffed with grass or straw. Adulteration of milk is common. Figures published by Wright show that approximately one-third of all samples tested in nine provinces were adulterated and that the water added varied from small amounts up to approximately 50 percent. Contamination of milk, the hot

BHUTIA PONIES WAITING FOR BUSINESS
HILL TRIBESMEN TRY TO SELL RIDES TO VACATIONISTS IN DARJEELING AND OTHER MOUNTAIN RESORTS.



INDIAN TAXIS

Left, A SCENE IN BOMBAY SHOWING A VEHICLE CALLED A GHARRY; *Right,* A CAB IN NEW DELHI CALLED A TONGA.

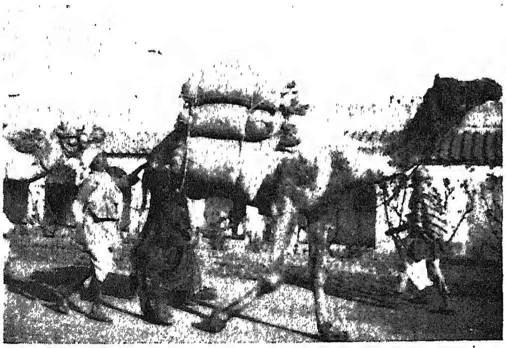
climate, and lack of refrigeration make it difficult to keep milk for very long. Fortunately, the practice of boiling milk before use is almost universal in India.

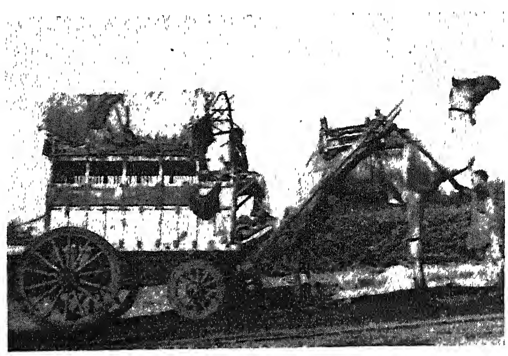
Much milk that is not used as whole milk or for production of ghee is made up into various types of milk sweets and products of soured milk. The sweets are made by condensing the milk to a suitable consistency and adding sugar and other materials. Many of them are very tasty. Some milk is soured to make special products. Much sour milk is available from the churning of soured whole milk for butter. Butter is then clarified by heating to produce ghee.

There is much room for improvement in milk-producing methods to provide better sanitation, but it should be recognized that the expense of many of the techniques used in the United States would so increase the price of milk as to put it out of range of much of the population.

Owing to the emphasis on draft power in India it is only natural that most of the types and breeds of cattle developed are specialized for work in the fields or on the highways. These animals give very little milk; however, certain breeds have been developed in which milk production has been emphasized. For the most part, these have been developed by tribes living in rural areas where grazing of livestock was a major occupation. At least six major types of cattle are generally recognized in India and within some of these types there are several breeds. These are described elsewhere (Phillips, 1944) along with summaries of available data on their milk production under improved conditions on government-owned and supervised farms.

Many rural sections have more cattle than the available range and pasture can support. In these areas death losses from starvation or from disease coupled with lowered resistance are frequent. Religious prejudice among the Hindus makes it very difficult to





A DROMEDARY AND A CAMEL CART IN NEW DELHI

A RIDING CAMEL IN THE PUNJAB

reduce the numbers of cattle to the point where available forage will support them adequately.

One of the serious problems in Indian agriculture is the maintenance of soil fertility. Most of the manure from cattle is not returned to the land but is used for fuel. It is carefully collected from city streets, highways, and fields and made up into flat cakes either on the ground or stuck to the walls of the buildings. After it is thoroughly dried, it is stored and burned as needed.

Most of the efforts to improve Indian cattle in which any scientific methods have been followed have been conducted at government-owned farms or privately-owned farms subsidized and supervised by government workers. At the present time there is only one outstanding example in India of cattle improvement work being done by a private party without government support. This is a rather unexpected situation in a country where cattle are held in higher esteem than in any other country in the world.

The military dairies are among the important organizations contributing to the regular supply of milk and to cattle improvement. Army officials, in order to be assured of a constant and clean milk supply, have found it necessary to establish their own dairies. These are managed in up-to-date fashion and have modern equipment for processing milk.

Many attempts have been made to introduce foreign breeds of dairy cattle in order to increase the milk supply. At first it was felt that these breeds could be introduced in pure form, but it was soon discovered that European breeds did not have sufficient adaptability to the hot climate of India. Some workers in India now oppose the use of imported breeds and prefer to base improvement on selection among the native dairy breeds. Others feel that the most rapid increase in milk supply can be obtained by crossing imported bulls, such as the Holstein, with the native cows, and possibly establishing new types from these crossbreds. It seems certain from available information that little, if any, more than fifty percent of European blood can be introduced and still have sufficient resistance to the environment. This is borne out by the following figures on production in cattle having various amounts of European blood, most of it from the Holstein:

Fraction of Imported Blood	Number of Records	Pounds of Milk
$\frac{1}{8}$	21	4,839
$\frac{1}{4}$	175	5,982
$\frac{1}{2}$	589	6,977
$\frac{5}{8}$	204	6,985
$\frac{3}{4}$	396	6,664
$\frac{7}{8}$	86	6,180

Animals having a high proportion of European blood grow satisfactorily up to the time of their first lactation. Then many of them show that they cannot stand the strain of the environment by becoming emaciated. During hot weather many of them have difficulty in maintaining a normal body temperature.

Many of the old, crippled, and diseased cattle of India place a continued drain upon the economy of the country because they are kept by owners beyond a useful age or are placed in the old cattle homes called *gowshalas* and *pinjrapoles*. These homes are maintained as charities or by levies on commercial transactions. Approximately 1,500 of these homes are said to exist in India. In only a

few of them is there any real effort to salvage useful animals and make them contribute something to human welfare.

The occidental finds it difficult to understand the ill treatment of cattle, which is quite common in India, in view of the reverence with which cattle are considered. It is considered immoral by the Hindu to kill an animal to put it out of its misery, but many animals die in agony as a result of disease, injury, or starvation. The Moslem policeman who would dare to put a cow out of her misery after she had been disemboweled by a street car would find himself in serious difficulty with the Hindu portion of the population. Other evidence of disregard for the comfort of animals is often found among oriental peoples. Mankar (1943) states that animals are mercilessly beaten when unable to yield milk owing to heat or estrus. He also states that about 20,000 young calves are starved to death in Bombay stables annually. These are calves brought with their dams into the city or which are born on trains en route or after cows arrive in the city. Mankar states that most of these calves die within a week or two after entry into Bombay owing to starvation and neglect, as there are no sheds for calves in the stables and it is not economical to maintain them in the city. But little is to be gained by reciting examples. The difference in oriental and occidental views of what constitutes humane treatment of animals is but a reflection of different religious, social, and economic backgrounds.

SMALL GOATS OF BENGAL

The Water Buffalo

Water buffaloes occupy an important place in Indian agriculture, but their position differs from that of cattle. Most Indian cattle are used primarily for draft power, and milk production is secondary. With water buffaloes the situation is reversed, milk production being the more important. Many

A JUMNA PARI BUCK
THESE LARGE GOATS HAVE LONG, FOLDED EARS. IN NORTHERN INDIA THEY YIELD MILK, MEAT, AND HIDES.

buffaloes are used for work, but they are considered too slow when compared with longer-legged, fast-moving bullocks of the draft breeds of cattle. Also, they are less able to withstand heat, and during hot weather require access to a pond or must have water poured over them at intervals to prevent too great a rise in body temperature.

The number of water buffaloes tabulated in the census of 1940 was 45,441,689. This did not include figures for some Indian States which did not participate in the census. These, along with the cattle, make the bovine population of India somewhat over two hundred millions.

Some comparative figures have already been given on milk production of cattle and water buffaloes in the villages, which indicate the superior producing capacity of the water buffalo. Under conditions on government-owned or supervised farms, water buffalo cows produce about 4,000 pounds of milk in a lactation of nine months, on the average. The fat content is higher than that of cattle, averaging about 7.5 percent. The fat is usually much whiter than that of milk from cattle, apparently owing to a low carotene content. The milk production of the buffalo cow is equaled or approached by only a few of the native breeds of cattle.

The water buffalo is one of India's useful animals. However, opposition to it is voiced by some Hindu leaders, who would prefer to have it replaced by cattle.

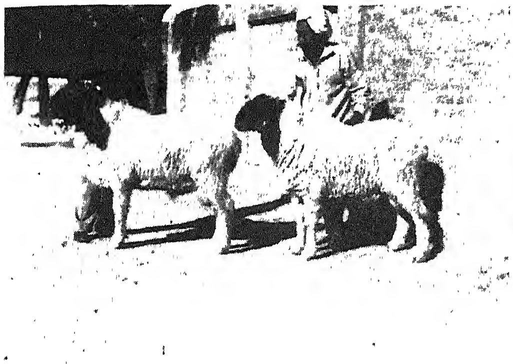
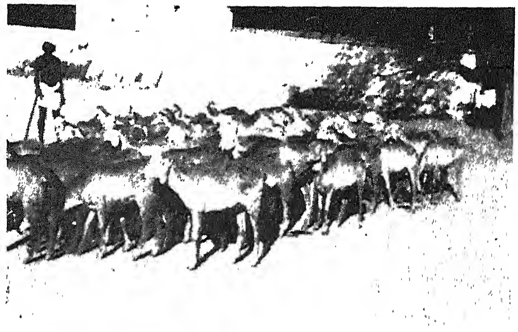
Other Livestock

Horses are of small importance in India. Compared with over two hundred million bovines, only 2,243,430 horses were tabulated in the 1940 census. The draft horse, which is so common on the farms of Europe and the United States, is not used to any extent in India. Ponies are produced chiefly in the Himalayan foothill regions, and the best saddle horses are bred in the drier regions of northwestern India. Most of the latter have some Arab blood.

Donkeys are also of relatively little importance. The number in 1940 was 2,028,467, or somewhat less than the number of horses. Donkeys are of two main types, small and large. The small ones average about 32 inches in height, while the large ones are about 44 inches high at the withers. They are used chiefly as pack animals. Mules are of very minor importance, only 79,512 having been tabulated in 1940. The donkey and the mule are held in very low regard among many Indians, some of whom would consider themselves degraded if seen riding behind one of these useful creatures.

Camels are used only in the drier portions of northwestern India. The number tabulated in 1940 was 1,109,565. They are of the single-humped type, or dromedary, and are used primarily as pack animals and for riding. Someone has described the camel as "an animal with fourteen universal joints," and a jolting ride was sufficient to convince me that the description is apt. Camels are also used to a limited extent for draft, being hitched to wagons with shafts that lead at a sharp angle up to the hump.

The combined numbers of horses, donkeys, mules, and camels total only 5,460,974. The relative paucity of these animals serves to emphasize the importance of bovines in Indian agriculture and national economy.

TWO KINDS OF INDIAN SHEEP
Left, LOHI RAMS OF A BREED THAT IS FOUND IN NORTHWESTERN INDIA; *Right,* HAIRY SHEEP OF SOUTHERN INDIA.

Goats are found in all parts of India. They are used for meat and skins, and some types produce milk for human consumption. Long-haired types in some northwestern areas are also sheared to obtain the fiber. There is much variation in size and type. Much could be done to improve both their productivity and methods of managing them so they would make a greater contribution to the national welfare. The number tabulated in 1940 was 59,369,261.

Sheep are also of considerable importance in the drier portions of India where much grazing land is adapted to sheep production. There were 49,060,959 sheep recorded in the 1940 census. These include several types. Fat-tailed sheep with very coarse wool are found in the northwestern regions adjacent to Afghanistan. Thin-tailed sheep that produce wool of somewhat better quality are found in other portions of northern and northwestern India, and in some sections of the Deccan. Another type is found in southern India which is covered only with brown hair. This type is used only for meat, skins, and manure. Wool from the other types is used in carpet manufacture and for various types of coarse cloth. Some types of Indian sheep produce enough milk to justify milking for human consumption. Ewes of one type are reported to yield as high as eight pounds daily at the peak of lactation. It is rich in fat, and some of it is mixed with the milk of cattle and buffalo and used in the production of ghee.

Swine are of little importance. Most Hindus are vegetarians, and Moslems will not touch pork, which therefore is used only by the foreign people and by low-caste groups. The number recorded in 1940 was 1,955,396. The presence of large numbers of foreign troops during the war increased the demand for pork and also the interest in improving breeding and production methods.

In the villages where hogs are raised, they are used primarily as scavengers and live to a considerable extent on human feces.

Chickens are of some importance in Indian agriculture. The number recorded in 1940 was 80,272,333. This seems like a large number, but when it is recalled that there are more than twice that many cattle in India, it is obvious that there are few chickens on the average farm. The orthodox Hindu abstains from eating poultry as well

HOG HOUSE IN A VILLAGE
THIS TUNNEL-LIKE AFFAIR IS THE ENTRANCE TO A SMALL UNDERGROUND ROOM WHERE HOGS CAN ESCAPE THE INTENSE SUMMER HEAT OF THE PLAINS.

AN ELEPHANT CARRIES FODDER
ONLY A FEW ELEPHANTS ARE USED IN AGRICULTURE.

as any other flesh. This also applies to eggs, particularly fertile ones, since the eating of a fertile egg would be equivalent to destroying a life. One authority has estimated the average egg consumption per person in India to be eight eggs per year.

Looking Ahead

Discussion and planning for that bright new world of the future rests on the hope that there will be enough food, clothing, medical care, and other essentials for all. Those are worthy objectives and are ideals which most Americans heartily support. However, many Americans measure the accomplishment of those objectives in terms of their own abundant food supply and the many other things which contribute to their health and comfort.

Planners for the future well-being of the world must be realistic if there is to be any hope of success. India and adjacent oriental countries are so densely populated in relation to the amount of tillable land, and their agricultural methods are so primitive in many respects, that there is no hope of increasing their per capita production of food to our levels, within any time we reasonably can foresee. At some future time sufficient reduction in populations or increase in productivity of the land or both may have occurred to make possible an approach to American standards of living. For the present, progress must be measured in terms of the existing conditions in the countries concerned rather than in terms of our own standards.

Americans who have not had experience in the Orient have so little conception of the gap between the ways of life here and there that it is difficult for them to comprehend the prevailing low standards of existence in India and adjacent oriental countries. We have felt pinched under wartime rationing and shortages in many of our favored articles of diet, but even with those restrictions we have been extremely well fed compared with the normal diet of large sections of the world's population.

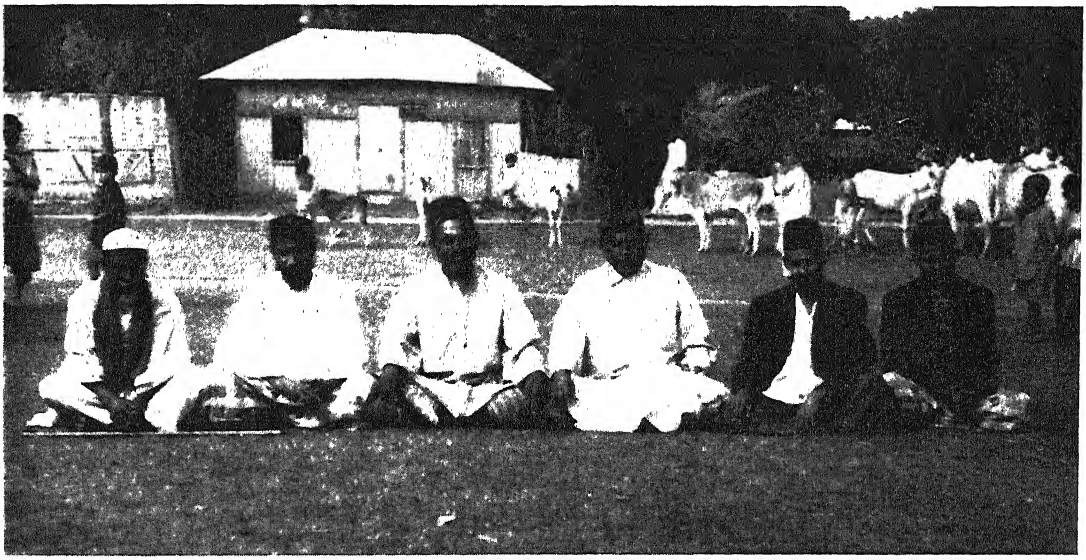
If we are to help our neighbors to help themselves, we must first understand the conditions under which they live and work, and their traditions and customs. Many of our fellow citizens of the world have less food and less of many material things than we have. But they have just as much pride in their countries, their traditions, and them-

VIEW IN THE BOMBAY STOCKYARDS
THE LARGE RICK OF STRAW IN THE BACKGROUND IS FOR FEED, NOT BEDDING. ANIMALS ARE HELD IN GROUPS BY PATIENT COOLIES. HINDUS ABHOR THE SLAUGHTERING OF ANIMALS, ESPECIALLY CATTLE.

selves. The last thing they want from us or anyone else is charity. Therefore, I use the phrase: "help our neighbors to help themselves."

The United States can produce some food above the needs of its people. But that surplus would not go far in raising the dietary intake of substandard peoples to an optimum level. If real progress is to be made in that direction, it must be largely by measures taken within the countries where the additional food is needed. We have made much progress in developing improved food production and processing techniques. Those techniques, or modifications of them to fit Indian conditions, could be applied to advantage in India, and also in some other countries. The immediate advantages are to our neighbors, of course, but in the long run there are advantages for us, including improved outlets for some of our own products and less economic disturbances to put strains on international relations.

There are several stumbling blocks in the path of introducing improved agricultural techniques in India. The level of education in rural India is extremely low, and improvement in this respect will be slow. Many religious dogmas and customs stand in the path of progress. The caste system tends to keep people in the trades and customs of their ancestors and discourages progressive attitudes among the people. Differences among Hindus, Moslems, and other religious groups often prevent cooperative studies and approaches to solutions of problems. Religion is so firmly intrenched that its influence pervades all parts of Indian life, often hampering or even openly opposing progress. India is not a political unit. Aside from the several provinces of British India, there are several hundred native states and agencies. Agriculture is considered a "provincial subject" and major responsibility for development is therefore in the hands of the provinces. Recent developments have greatly strengthened the position of agricultural work in the central government. But much remains to be done before the agricultural work of the country can be effectively coordinated. Many problems are national or at least regional in scope, and the only effective approach to them is by the use of all available personnel and equipment in a concerted attack. This is especially true in a country where the number of technically trained personnel is quite limited. Another problem is presented by the lack of practical



VILLAGE LEADERS AT A CATTLE SHOW
THESE LEADERS PROUDLY POSED DURING A CATTLE SHOW STAGED FOR THE AUTHOR IN THEIR BENGAL VILLAGE.

experience by many technically trained men. Too often, the man with an excellent scientific background is unable to bridge the gap between the laboratory and the land, since he has never worked with animals or on the land. And he is prevented from gaining such experience because working with his hands would be beneath the dignity of an educated man.

Such an array of stumbling blocks, plus others not listed, may make the path of progress seem rather hopeless. The difficulties are great, and any attempt to minimize them would only place another stumbling block in the path. Those difficulties are so great that they can be overcome only by bold, decisive action, based on the best technical information available, and upon the use of the best scientific leadership that is to be had. So the immensity of the problems of more food and better balanced diets for India, and for other countries where substandard diets prevail, is a challenge to the countries themselves and to the countries who must assume leadership in the building of tomorrow's world.

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BIRD'S-EYE VIEW

A babe released from sleep in utero
Where, all its wants supplied, it grew so fair,
Nor struggled 'til it felt the need of air,
Awakes and strives the world of sense to know.

Matured with sensory satiety
Or hammered hard with labor's ceaseless blows
Most men returning drift to seek repose
In stance secure, renouncing some variety.

A restless few maintain the childish heart
Of high adventure to the bounds of thought
And there take wings to ride exploring clouds.
Alone they see the past in its full part
And point the future as true prophets ought
In bliss beyond the need of cheering crowds.

—JOHN G. SINCLAIR

SCIENCE ON THE MARCH

INSECTICIDAL AEROSOLS*

Mr. Chairman: I greatly appreciate the honor the City of Philadelphia has bestowed upon me personally, and indirectly upon the Bureau of Entomology and Plant Quarantine. This Bureau has made possible the program of research that led to the development of insecticidal aerosols. Thank you very much.

It may be of interest to outline briefly the events leading to the application of insecticides in the form of aerosols and the research leading to the development of the so-called aerosol bomb which was so popular with our armed forces.

Before the war Capt. Sullivan and I started a study to find better methods of applying the fly-spray type of insecticide. Sprays were often low in effectiveness because of poor quality and because inefficient sprayers were used to apply them.

We decided to try aerosols. Aerosol is simply the scientific term applied to a suspension of finely divided materials in air. Some common examples are smoke, fogs, clouds, or suspended dust. The finer these particles are, the longer they will stay suspended, but we soon found that a certain size range was best for insecticides.

There were several ways already described in the literature for the production of aerosols from materials similar to insecticides. One method consisted of partially burning a material and using the insecticide-laden smoke. An example is tobacco smoke. Another method employed heat to break the insecticide into fine particles. The army screening smoke generator is an example. This method is now being developed for the production of aerosols on a large scale. Mechanical methods were also available, some of which used a stream of air as in the common atomizer. All of these methods were studied but they lacked the convenience and other features that would make aerosols a practical improvement over sprays.

Photo by Philip B. Wallace

THE JOHN SCOTT MEDAL OF THE CITY OF PHILADELPHIA

* An address by Dr. L. D. Goodhue, Senior Chemist, Bureau of Entomology and Plant Quarantine, Beltsville, Md., accepting the John Scott Award for his part in the development of insecticidal aerosols. His co-worker, Capt. W. N. Sullivan, on leave as Associate Entomologist of the same Bureau, received the same award. In accepting it, Capt. Sullivan spoke extemporaneously about the uses of the aerosol bomb in the Army.—Ed.

Next the liquefied-gas method was conceived and tried. This new method was successful from the beginning. It consists essentially of dissolving the insecticide in a liquefied gas under pressure and allowing this solution to escape through a small orifice under its own pressure. The violent boiling of the liquefied gas disperses the insecticide

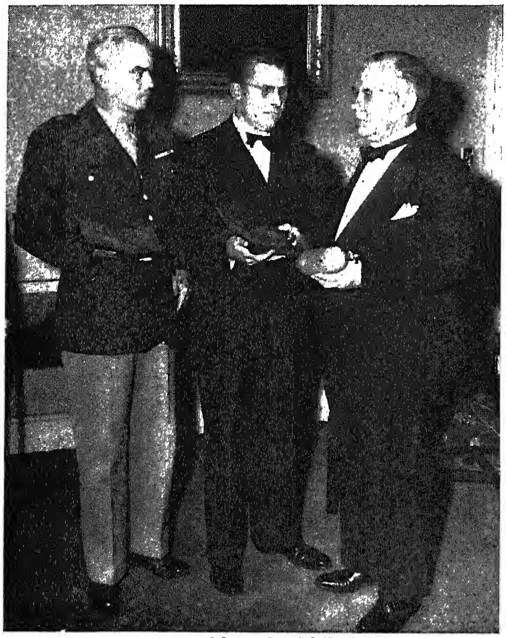


Photo by Philadelphia Inquirer

A JOHN SCOTT AWARD

CAPTAIN W. N. SULLIVAN AND DOCTOR L. D. GOODHUE RECEIVE JOHN SCOTT MEDALS FROM MR. WALTER B. GIBBONS, OF THE BOARD OF CITY TRUSTS, FOR THEIR DISCOVERY AND DEVELOPMENT OF INSECTICIDAL AEROSOLS AND THE AEROSOL BOMB, WHICH MR. GIBBONS HOLDS IN HIS HAND. ON THIS OCCASION IN THE ROOMS OF THE AMERICAN PHILOSOPHICAL SOCIETY ON SEPTEMBER 20, 1945, THEY ALSO RECEIVED SCROLLS AND CHECKS FOR $500 EACH.

in the form of a fog, or aerosol, which remains suspended and effective for long periods. The solution under pressure is conveniently contained in 1-pound bombs ready for instant use by simply pressing a button.

The insecticide best adapted to this method of application is pyrethrum. The best suited liquefied gas is dichlorodifluoromethane (Freon-12), commonly used in household refrigerators. This material, combined with sesame oil to improve its properties, gives a superior insecticide that is convenient to use, is nontoxic to man, nonflammable, and yet highly toxic to many species of insects, especially mosquitoes.

The armed forces realized the advantages of this method when the war broke out and something was needed to fight mosquitoes, which carry malaria, yellow fever, dengue, filariasis, and other diseases. The method was adopted, and at the close of the war over 35,000,000 of these 1-pound aerosol bombs had been manufactured. They were used and acclaimed[1] in many parts of the world and no doubt afforded much comfort to the men and greatly reduced the incidence of insect-borne diseases.

DDT, the insecticide about which you have heard so much, was combined with pyrethrum in the aerosol bomb about a year ago. It increased the effectiveness of the aerosol, especially against flies. Research has increased the efficiency of the aerosol many times, and improvements are still being made.

Judging from the demand by the armed forces, this aerosol method of applying insecticides is expected to become popular for civilian use. Some companies have offered aerosol bombs for sale in a limited way as an acceptance test, and the results are good. A large number of firms are interested in the manufacture of aerosol bombs, and considerable industrial research is being directed toward making improvements in all phases of the work. In a short time the aerosol bomb will probably be a standard item for public use.

The liquefied-gas method of making aerosols has many other uses. It has been employed successfully for the control of some field-crop insects on a large scale. Here methyl chloride, which is too toxic for household aerosols, can be used as the propellent.

In closing I might say that other materials besides insecticides, such as germicides, fungicides, plant hormones, weed killers, medicines, perfumes, and many other materials, can be applied by this liquefied-gas aerosol method.

Again permit me to express my appreciation of the recognition of our work.

LYLE D. GOODHUE

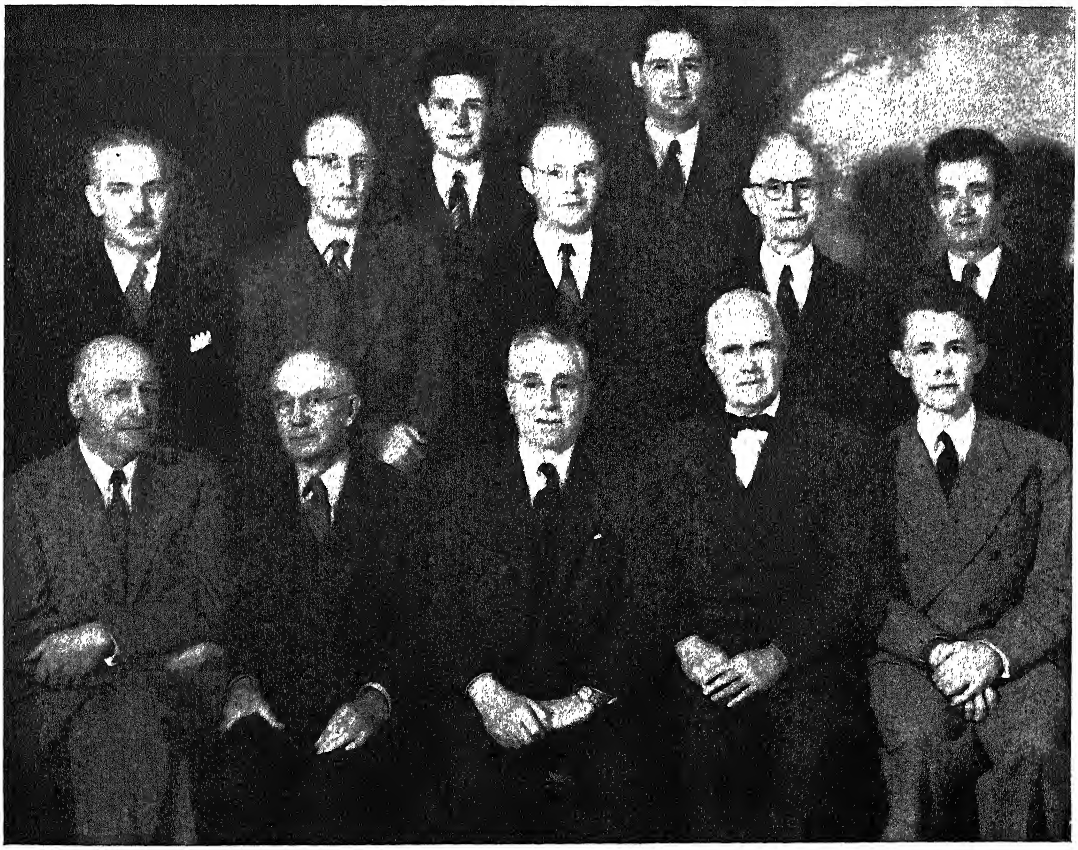
[1] For example, Sgt. Edward Wollerman on Luzon wrote to the editor: "If I return home without becoming ill with malaria, I can give a lot of credit to the convenience and protection of the aerosol bomb."

CHARLES ATWOOD KOFOID

October eleventh was the eightieth anniversary of the birthday of Charles Atwood Kofoid. In appreciation of Professor Kofoid's career as a biologist and member of the faculty, the department of zoology at the University of California in Berkeley held a reception in his library. The reception was attended by many persons who as students or colleagues had been closely associated with him; and many letters expressing good will and congratulation were sent by friends who could not be present.

Professor Kofoid was an active member of the department of zoology during the greater part of the period of its development to the time of his retirement ten years ago. Although for twenty-four years Joseph Le Conte had given a series of two lectures a week throughout the academic year in comparative zoology, the growth of the department began in earnest when William Emerson Ritter came in 1892. He began to organize laboratory work in relation to Professor LeConte's lectures, and in the remaining years of the century there was continued expansion of both the curriculum and the staff. The building that was to house the department for more than thirty years, East Hall, was built in that period.

So it was to a relatively newly organized, freshly developing, and enthusiastic department that Professor Kofoid came at the beginning of 1901. In the first few years the part of the curriculum of which he was in charge included microscopical technique, em-



McCullagh, Berkeley

FACULTY IN ZOOLOGY, UNIVERSITY OF CALIFORNIA, BERKELEY

Front row, left to right: RICHARD GOLDSCHMIDT, S. J. HOLMES, CHARLES A. KOFOID, H. B. TORREY, HAROLD KIRBY. *Second row:* E. RAYMOND HALL (NOW AT THE UNIVERSITY OF KANSAS), J. E. GULLBERG, ALDEN H. MILLER, S. F. LIGHT, S. C. BROOKS. *Third row:* RAY L. WATTERSON, RICHARD M. EAKIN. TWO MEMBERS OF THE STAFF—J. A. LONG AND SETH B. BENSON—WERE ABSENT WHEN THIS PICTURE WAS TAKEN IN THE SPRING OF 1944.

bryology, cytology, and invertebrate zoology. By 1909, however, he had discontinued this work and had introduced courses in the biological examination of water, protozoology, and general parasitology. At this time, Professor Ritter transferred his residence to La Jolla, returning to Berkeley only for a few weeks of lectures each year, and Professor Kofoid became chairman of the department. He continued to be the administrative officer until his retirement in 1936, except for a short time following his service as major in the Sanitary Corps of the United States Army in 1918 and 1919. His influential position in departmental matters contributed greatly to university development; and he played a particularly important and freely acknowledged part in shaping the pattern of intellectual growth of many who have been active in biological and medical work beginning forty years ago. This has been especially true for the sixty graduate students who, between 1901 and 1940, received the Ph.D. degree from the University of California after completing thesis work with Professor Kofoid.

An important early contribution was his work in connection with the marine biological station that was at first closely related to the department. Before 1900 Professor Ritter and the other members of the faculty of the department had made efforts to establish a place for seaside work in marine biology. In 1901, these efforts became more continuous and fruitful, and interest developed in undertaking a detailed biological survey of the coast of California. Before long it was decided that it was desirable to specialize on the plankton and pelagic life of the nearby ocean. Eventually, a marine biological station became established at La Jolla.

While acting as special investigator for the U. S. Bureau of Education in 1908 and 1909, Professor Kofoid had made an extensive study of the marine and fresh-water biological stations of Europe. He published in 1910 a detailed and well-illustrated account of these laboratories. In acknowledging the contributions which had been made to the construction and equipment of the new laboratory built in 1910 on the pueblo tract near La Jolla, Professor Ritter wrote that "into the general working plans of the station are woven the suggestions, the ideas, and the activities of Professor C. A. Kofoid and H. B. Torrey in so intimate a way that surely without them the institution could not have been exactly what it is."

Also in that period, besides his biological studies, Professor Kofoid made two outstanding contributions to plankton investigation. One of these was his invention of a self-closing water bucket which made it possible to obtain from a known level twenty liters of water for filtration and analysis. The other was the self-closing plankton net which he devised. This net, weighing about 180 pounds, could be lowered to a desired level while closed, opened and towed at that level, then closed before drawing up.

Professor Kofoid was assistant director of the Marine Biological Station of San Diego, and of the Scripps Institution for Biological Research as it was later called, until a short time before Professor Ritter retired as director. In 1925 the laboratory was organized, without further direct relation to the department of zoology, as the Scripps Institution of Oceanography.

Professor Kofoid's name has been associated especially with protozoology, but biological subjects of many sorts have entered into his interests in teaching and research, writing and lecturing. At Harvard University, before he took his degree in 1894, he studied the origin and history of the mesoderm, cleavage, and cell lineage in *Limax*. In the five and one-half years before he came to California, when he was superintendant of the Illinois Biological Station, he carried on a study of the plankton of the Illinois river, the scope of which he defined as "a continuous, systematic and exhaustive examination of the plant and animal life suspended in the waters of a river system with a view to determining its amount and seasonal changes, its local and vertical distribution, its movement and relation to the current, the effects upon it of flood and drought, of light and temperature, the organisms which compose it, their seasonal and cyclic changes, and their mutual interrelations. Added interest arises from the fact that this is the first application of this method of biological investigation to a river system and its related waters."

Removal to the coast of California was fol-

lowed by a shift of his studies to marine plankton, and the Alexander Agassiz expedition to the eastern tropical Pacific soon provided an opportunity to extend his investigations beyond the coastal waters. From the Albatross in 1904–05 he made extensive collections of plankton, and from these he subsequently made elaborate studies of dinoflagellates and tintinnids—studies aided for a number of years by Agassiz himself. He carried on research on marine plankton while he occupied the Smithsonian Institution table at the Naples Biological Station in 1908, and at the Asamushi Marine Biological Station in Japan in 1930, when the Rockefeller Foundation appointed him to a visiting professorship at the Tôhoku Imperial University in Sendai. On trips across the Pacific he took advantage of the opportunity for filtering plankton from the running sea-water system of the steamship—an indication of his enthusiasm for this work.

Professor Kofoid's service in the Sanitary Corps in 1918 and 1919 marked the beginning of a series of studies of parasites of man, resulting particularly in detailed and finely illustrated papers on the intestinal flagellates and amoebae. This group of studies culminated in the late twenties in cooperative work in the Stomatological Research Group of the University of California, in which Professor Kofoid and his associates made extensive studies of the protozoa of the human mouth. For some years following his army experience, he directed work in the parasitological division of the Bureau of Communicable Diseases, California State Board of Health; later until about the time of his retirement he conducted a laboratory for parasitological examinations. An important feature of this laboratory was the technical training of students working in it, and the upholding of standards of accuracy and thoroughness that had a valuable influence on the training of medical technicians in the state. When in 1923 he gave the Faculty Research Lecture, selection for which is regarded as the highest honor the Academic Senate of the University of California can give to one of its members, he chose the topic "Amoeba and Man."

In 1921 the staff of the proposed Bombay School of Tropical Medicine was being selected, and two professorships—one in clinical medicine and one in protozoology—were to be supported by donations from Sir Dorab Tata. It was hoped that the best men available for teaching and research in their subjects could be attracted to the new institution. Professor Kofoid was nominated to the Tata Professorship in Protozoology by the Tropical Diseases Committee of the Royal Society of London, and was appointed by the Bombay Government; but because of uncertainty about the organization of the School and desire to maintain the continuity of his research projects he resigned the appointment without leaving Berkeley.

As would be expected of a biologist, Professor Kofoid's interests in medical protozoology gave equal weight to all aspects of the problems; biological, medical, and public health matters shared his attention without unbalanced consideration of any one alone. His Faculty Research lecture expressed his point of view: "Progress in the applications of science in any field is in the long run safely and soundly made only upon the basis of some accurate knowledge of many factors basic and collateral to the subject. . . . In this realm of science no subdivisions into pure and impure, useless or applied, can exist for the discoverer."

In 1914 shipworms appeared at the north end of San Francisco Bay in a region from which it had been thought that they were excluded by fresh water entering by the Sacramento river. After 1917 damage became severe, and in 1920 a committee was appointed to study the problem, combining the interests of the American Wood-Preservers' Association and the Forest Products Laboratory. Professor Kofoid, who had for some years before been concerned with the problems of marine borers in California harbors, became chairman of the subcommittee on biological research. Members of the committee requested the National Research Council to organize a marine borer study on a wider scale, and in 1922 a council committee on marine piling investigations was appointed including Professor Kofoid as biologist. Work for both these groups was carried on by him and biologists who became associated with him at the University of California. The final reports of the work of the two groups which were published in 1924 and

1927 included a consideration of all phases of the biology of the marine borers.

Shortly after the appearance of the final report of the San Francisco Bay Marine Piling Committee, interest was stimulated in damage by termites to wooden structures on land. The destructive *Coptotermes* had been found in the wood of an ocean liner docked in San Francisco, and it was feared that it might become established in California; also there was increase in attention to damage by native termites. The cooperation of the University was sought in study under a committee which consisted in part of the same personnel as in the Teredo-group. Biological work was carried on in the department of zoology under the direction of Professor Kofoid and Professor S. F. Light, and a final report in which again all phases of the biology of termites were considered appeared as a book in 1934.

These investigations, in which biological and economic considerations were combined, interested Professor Kofoid greatly, and his enthusiasm and ability played a decisive part in carrying them on. He conducted also, in cooperation with associates and students, studies of various groups of protozoa besides those of marine and fresh-water plankton. Among these, were the striking associations of protozoa in termites; those in ruminants and the elephant, from which he secured material in 1915 and 1916 in Ceylon and India; and those in molluscs in the littoral zone. In these, and in various free-living protozoa, he was interested particularly in the structural basis for coordinated activity, designated the neuromotor system; in questions of colonial or multiple organization and individuality; and in the significance to evolutionary considerations of these situations in which the factor of natural selection did not seem to be playing a primary rôle.

From the beginning of his career Professor Kofoid has been a bibliophile, and has gathered a very large and valuable library, of books and reprints, in which the history of science, protozoology, and parasitology are particularly well represented. Mrs. Kofoid, who died in 1942, worked with him for many years in the organization of this collection, and the University of California Library has provided space to accommodate it.

In the year when Professor Kofoid came to California he began to conduct a section designated Current Zoölogical Literature in the *Journal of Applied Microscopy,* which was published by the Bausch and Lomb Optical Company. He made the abstracts and reviews that were given, until the journal was discontinued in 1903. He has continued this sort of work with scientific literature ever since, and has prepared many hundreds of reviews and abstracts for publication. From the beginning of *Biological Abstracts* to the present he has been editor of the Section on General Biology, for animals; and from 1931 he has been associate editor of *Isis,* responsible for reviews of historical literature in biology and zoology.

A large part of the research done in the department has been published in the University of California Publications in Zoology, which began in 1902 and now consists of more than fifty volumes. At first the series was edited by Professor Ritter; from the end of 1909 until his retirement Professor Kofoid was one of the editors, and he upheld standards of excellence in composition and illustration which have been characteristic of his own publications. He has also, at one time or another, been associate editor of a number of scientific journals here and abroad.

Professor Kofoid's attainments have been recognized by his election to various organizations related to his subjects; by appointment at times to the councils and as presiding officer of some of these; by his election to the National Academy of Sciences in 1922; and in many other ways. Honorary degrees have been conferred on him by Oberlin, where he finished his undergraduate work in 1890; by the University of Wales; and by the University of California. At the 74th commencement in 1937, when President Sproul presented him with an honorary LL.D., he characterized him as "A humane scientist, distinguished alike for the breadth and depth of his knowledge of zoology and its contributions to the science of medicine and public health; an effective teacher who embodies eminence in the field of science and devotion to the spiritual life; one who places scientific knowledge at the service of mankind."

HAROLD KIRBY

COMMENTS AND CRITICISMS

Prometric

It has occurred to me that THE SCIENTIFIC MONTHLY might render a service to its subscribers not resident in English-speaking countries, and in general to all, in paying more attention to the use of metric measures in the articles presented.

As an example, the interesting story beginning in the July 1945 issue, The Problem of the Amazon, which is a translation from the original in Portuguese, converts most of the metric units—doubtless used in original article—into the English. The story will doubtless be widely useful, not only in this country but abroad, the translator stating that the original edition was very small. If, as you may consider necessary, the English units—such as miles and degrees Fahrenheit—must be used in such a story, the metric could readily be placed in parentheses. In this same July number there is one example of introduction of metric equivalents in a chart on page 47.

Personally, I hope this country may eventually convert to the metric system; it is believed that this could be done over a reasonable period without serious embarrassment, and with great benefit to all concerned. In any event, if the English-speaking nations are to remain on an island in this respect—using a system of weights and measures differing from that of the rest of the world, a broad point of view should at least give recognition to the wide use of the metric, inserting metric equivalents of important quantities.

It is of course common in scientific practice and in a number of other ways, even in this country and England, to utilize the convenient metric system. I feel therefore that your periodical would be doing a service in giving some recognition to that important system.—LOUIS ELLIOTT.

Antimetric

As to the editorial policy for the MONTHLY, with reference to use of both English and metric system units, you have really a difficult problem. So have we. I have grappled with it without much consistency, I fear.

In general, I think that for a journal such as THE SCIENTIFIC MONTHLY, I would be rather inclined to take the easy course of printing units in whichever system the author of the paper employed and making no attempt in general to translate the units from the one system to the other.

The reason is essentially this. Some of your manuscripts will pertain to laboratory researches. You may perhaps have a paper on the atomic bomb in which some of the dimensions have been given in micrograms. If you have papers in immunology or endocrinology, micrograms or milliliters would have been employed. To translate these into units of the English system would be relatively meaningless. Moreover, if a man mentions an object whose dimensions are, say, 5 centimeters by 2 centimeters by 3 centimeters, the simple digits will suffice to give the number of centimeters but when you translate this into inches, fairly long decimals will have to be employed.

In other fields, as for example ecology, measurements may be given in yards or pounds. These are meaningful for most of the readers of the MONTHLY, whereas the giving of equivalents in meters or kilograms would not be any significant addition to the comprehension of the paper. It would be just an operation for editorial consistency which, in my way of thinking, would hardly yield returns commensurate with efforts to be applied.

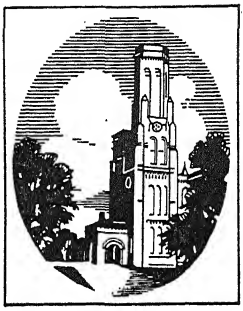
If a geographical paper in Brazil gives the length of a river in kilometers, well and good, I would be inclined to leave it in kilometers. If a similar geographical paper in the United States gives a distance from one city to another in miles, I would not translate it to kilometers, or even indicate the number of kilometers in parenthesis.

What I am getting at is the different fields of research and their own accustomed units of measurement. Those who are interested in seeing the measurements expressed in the other system, are as a rule the people who have the equivalents, or the factors for multiplication, pretty well in mind.

Part of my reaction here is due to the fact that I am not one of those who would like to see the metric system supersede the English system in general.—JOHN E. FLYNN.

Because space was lacking, the three remaining paragraphs of this letter had to be omitted. Dr. Flynn went on to say that the metric system is not so advantageous as it was when originally proposed because of the growth of the English-speaking world since that time. He concedes the superiority of the metric system in the laboratory but contends that for practical purposes the duodecimal units of the English system (easily divisible by two, three, and four) are advantageous. "It is unfortunate," he says, "that we do not have six fingers on each hand instead of five. If we had been so equipped, division would have been much easier." Professor Ore comments on "the dozen system" on page 373.—ED.

THE BROWNSTONE TOWER



ON July 12 of this year we received from Mr. Louis Elliott, Consulting Mechanical Engineer, a letter (see page 419) advocating more extensive use of the metric system in THE SCIENTIFIC MONTHLY. Our first impulse was to abandon English units — why should THE *Scientific* MONTHLY help to perpetuate an *unscientific* system of weights and measures? Then we turned to our editorial adviser, Dr. John E. Flynn, Editor-in-Chief of *Biological Abstracts*. As usual, Dr. Flynn gave us the full benefit of his experience in a carefully considered letter (see page 419). He advised us to publish measurements in the system preferred by the author without adding in parentheses equivalent figures in the other system. We were highly susceptible to this advice at the time it was received, for our assistant, Mrs. E. B. Scovill, was absent because of illness. We could not, and still cannot, undertake anything that involves additional editorial work. Unlike Dr. Flynn, we should like to see the metric system supersede the traditional conglomeration of weights and measures called "English." Our reasons for desiring this change are not theoretical but practical and immediate: we must now instruct our engraver in inches and our printer in picas, whereas we actually make all our editorial measurements in centimeters, as any scientist would do. Professor Ore, writing on the metric system in this issue (pages 373–375), predicts that the universal use of the metric system "will be viewed with considerable favor by the world organization which will eventually be created to further international cooperation and coordination." In making that prediction the professor has carefully retracted his neck, but, to use professorial language, it is not improbable that he is not averse to the universal adoption of the metric system. And he knows that it will take a long time to bring it about. So we say to Mr. Elliott, "Be of good cheer—THE SCIENTIFIC MONTHLY teeters toward you." When we get an editorial assistant, we may give roughly the metric equivalents of English units for the benefit of our Latin-American readers. In the meantime we urge everyone who favors the metric system to use it exclusively in contributions to THE SCIENTIFIC MONTHLY.

WE HAVE been wondering what arguments would be most likely to induce scientists and friends of science to contribute to our centennial building fund. Why, indeed, should a member of the A.A.A.S. part with more than his annual dues? He can easily demonstrate that he has no unallotted balance at any time, and that he is insolvent at the end of each month. If he is married—most scientists are in that happy state—there are a thousand reasons why he thinks he cannot contribute: like Mrs. Day, mother may not be able to keep a budget and the money just disappears; or the children must have new clothes; or the house needs repairs (was there ever a home-owner who could not sink a fortune in his house?); or relatives need assistance; or a new car is absolutely necessary, and so on and on. Yesterday we shook hands with a scientist who may have money to spare, but so far as we know he is unique. *You* have no money to spare, but you *can* contribute to the building fund. When you were out of cigarettes, did you buy more, if you could get them? When you wanted to see a certain movie, did you go? When you needed gasoline for that long-deferred vacation, did you purchase it? We will not ask questions that might be more embarrassing. The general answer is, of course, that you spend without hesitation whatever small sums seem essential to promote the private welfare. And small sums add up to larger sums until, as Hi Ho has demonstrated by simple arithmetic, you could retire, or something, if you could only stop the little leaks in the pocketbook. If the very thought of pinching pennies is obnoxious to you, forget it and indulge yourself in your accustomed manner. We ask only that you include the centennial building fund among your list of indulgences for which you pay as a matter of course without counting the cost until later.

"But why," you say, "should I leap to my checkbook because the A.A.A.S. needs a home of its own? Why not let Ajax carry the load?" Why not? Because the new building is to be *your* home as well as ours. It will stand before the public as a monument to your faith and pride in the importance and dignity of your profession as a scientist. Let nothing prevent you from having a hand in it.—F. L. CAMPBELL.

THE SCIENTIFIC MONTHLY

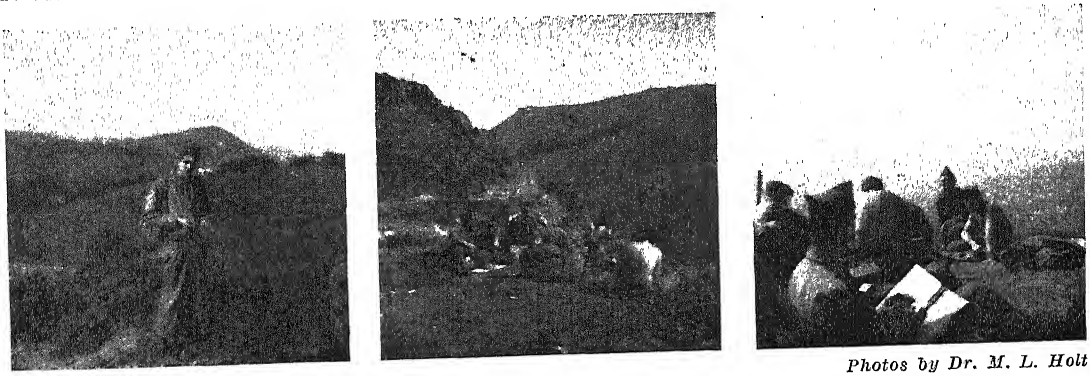
DECEMBER, 1945

PIC LA RHUNE*

By STANLEY A. CAIN

CHIEF, SCIENCE SECTION, BIARRITZ AMERICAN UNIVERSITY, BIARRITZ, FRANCE

PIC LA RHUNE, in Basses Pyrénées a few kilometers south of Ascain and west of Sare, is a typical mountain of the lower western Pyrénées. It lies on the border between France and Spain and, although it is only 900 meters in altitude, there is a magnificent view to the west and north of the coast and sea from Hendaye past St. Jean-de-Luz and Biarritz to beyond Bayonne. To the northeast the Col de St. Ignace leads into the Nivelle. In this valley and in the Nive beyond there is spread out a tapestry of small Basque farms and villages with their white stucco walls and rufus tiles. All else is an array of mountains, one behind another in rhythmical patterns until the more distant ones blend with the imagination in a confusion of soft colors, with earth and sky becoming one.



Photos by Dr. M. L. Holt

GI BOTANISTS ON PIC LA RHUNE NEAR BIARRITZ, FRANCE

Our day of botanizing on Pic la Rhune probably is of less interest than the fact that a civilian professor in uniform and a group of soldiers were on a field trip as a class of an American university abroad. It is, therefore, in order to tell a little about Biarritz American University before describing the results of the excursion.

Long before VE-day certain officers of the Information and Education Division of the Army had been making provisional plans for the establishment for soldiers of collegiate-level institutions in Europe as soon as conditions permitted. Within about two weeks after the European victory the program had gone up through channels and had received the green light from General Eisenhower, and one of the most amazing educational experiments of all time was under way. Late in May certain civilian educators who were to play leading roles were called into Washington to help the Army select the faculty and plan the programs for two universities of 4000 students each, one to be located in England and one in France. Among these key men were John Dale Russell, who became Dean of Biarritz American University, and Merle G. Coulter, who helped select the biological personnel for the institutions. Fast

* Contribution from the Botanical Laboratory, The University of Tennessee, N. Ser. No. 83.

action was requisite for all aspects of the installation if the schools were to open during the summer. How it went can be suggested by my original contact with the project.

On the morning of the sixth of June, while pulling onions from my garden at Knoxville—we have *Allium vineale* in everything down south—I was told Washington was calling, and went in to a conversation very much as follows:

"Hello, Stan. This is Merle Coulter. How would you like to go to France?"

"What is the deal?"

"The Army is setting up a university at Fontainbleau and we want you to come along. You will get your base pay plus 25 percent for overseas duty, a seven to twelve months' contract, and brass buttons to wear at Courts Martial. How about it if you can get a leave of absence from the University?"

"It sounds interesting. When can I let you know what my wife and Dean think?"

"Call me back this afternoon."

So it started, and so it has been ever since. Rapid decisions, rapid developments, rapid changes. Within thirty hours I was on my way to Boston to be processed by the First Service Command as an "Assimilated Field Grade Officer." I was the first civilian professor to be processed for overseas duty by the Boston Command, and, having no specific directions from Washington, they did everything they could think of, for me and to me. By July fourth about 150 of us from the four corners of the States were assembled at the New York Port of Embarkation for sailing on the *Queen Elizabeth* as shipment IJ B411 LP, the first of the masculine academic WACs.

We landed at Greenock on the Firth of Clyde, and those of us destined for France went down to Southampton by night train, crossed to Le Havre in the hold of a converted freighter, and on to Paris, again by night train. Part of the travel was luxurious, and part was quite the reverse, but when at dawn we could see the Eifel tower in the distance after several almost sleepless nights, we knew we would make it and were glad.

About the time we had left New York it was decided to locate the French school at Biarritz instead of Fontainbleau, and the Station Complement had moved in and set to work preparing the physical facilities for a university in a world-famous resort town where there had never been more than a branch of the Bayonne Lycée. Although that is another story, it would be unnecessarily concise not to mention that the library is located in the former gambling room of the Casino Municipal. The hazards of the roulette table are now replaced by the greater hazards of the printed page. The Physics laboratory is in the rotunda of another casino, Biology and Chemistry are in hotels, Mathematics in a small hospital, and Geology is in a private villa. The students are billeted in the better hotels, including the Palais of Napoleon and Eugénie, and the professors and Army officers are billeted in villas, some in town and some as far out as five kilometers.

When one considers the multitude of details involved in the organization and establishment in so short a time of an installation for 4000 students, 300 faculty, and about 1500 Station Complement, it is little less than amazing that the classes commenced on August twentieth as planned. It is true that at first some laboratories were not yet completed, many classes were without textbooks, some branches were understaffed, and some teachers were without their trunk lockers which contained notes and course outlines as well as spare clothing, but the faculty is composed of experienced teachers and the program went on in spite of handicaps and petty annoyances.

The University is organized in eight sections, or colleges, and forty branches, or departments. About 330 courses are listed in the catalogue and, although most of them are lower division courses, many of them are of Junior and Senior level and some are graduate. The student selection was well made, and the ability, enthusiasm, and appreciation of the soldiers, so recently from battle and the sundry duties of war, are extraordinary. Although the recreational aspects of life at Biarritz are certainly no deterrent to his desire to attend Biarritz American University, almost without exception the student

here really wants an education and earnestly does his best during the intensive eight-weeks' term.

The students on the Pic la Rhune trip are members of my class in Biology 205, Plant Classification. They have previously taken from one to several courses in botany and all are eager to learn more about plants in general and the local flora of the southwest of France in particular. The course is organized on a basis of three hours of lecture or lecture-discussion and three two-hour periods of field or laboratory work a week. In addition, an all-day excursion is taken nearly every Sunday. The trip to Pic la Rhune is, then, simply an example of the work.

We left Biarritz in a weapons carrier Sunday morning the sixteenth of September; travelling southwest along the coast through Bidart and Guéthary to St. Jean-de-Luz we turned eastward up the Nivelle through Ascain to Col de St. Ignace at the foot of the mountain. Our climb followed the route of the funicular railway which has not been in operation recently. Our attention during the first part of the climb was centered on the heathers, with each student trying to find as many types as possible. At the head of the Col, nearly half way up, we stopped to rest and identify our collections. We sat on a steep grassy slope with a rugged Triassic conglomerate hogback behind us, the whole valley below us with its cover of gorse and bracken heath and occasional small stone-walled pastures, and the distant mountains before us.

The handsomest of our heaths was *Daboecia polifolia* Don, with large urceolate lilac to rose-pink corollas and ovate to narrowly oval leaves, shining above, white-pubescent below, and ciliate on the margins. This beautiful undershrub is usually 20 to 30 cm. tall, but occasionally reaches half a meter in favored sites. Besides occurring in a few localities, especially in the southwest of France, it is one of that geographically interesting group of plants that occur in Ireland and the Iberian Peninsula. The needle-leaved genus *Erica* was found to be represented by four species. Of the two species with glabrous sepals, *E. vagans* L. was readily distinguished from *E. cinerea* L. by its exserted anthers. Of the two species with ciliate sepals, *E. Tetralix* L. and *E. ciliate* L., the shapes of the corollas permitted easy separation. The corolla of the former is globular-urceolate and that of the latter is elongate, constricted at the throat, and somewhat arcuate. All of these species existed in color forms ranging from nearly white to lavender, lilac and rose tints. These species are all common in the southwest of France and extend from the Iberian Peninsula, especially Portugal, to the northwest of Europe in the areas of the marine west-coast climate. *E. vagans* and *ciliaris* occur in Ireland; *E. Tetralix* and *cinerea* extend into Scandinavia. The sixth species of heath is *Calluna vulgaris* Salisb. In addition to the ordinary form, it is present on Pic la Rhune in two modifications: var. *condensata* Lemotte, with tightly appressed overlapping leaves in four sharply aligned rows, and var. *patula* Rouy, with leaves which extend out almost at right angles to the stem. The latter variety seems only to occur in moist protected situations and may be nothing more than a habitat modification. Variety *condensata,* however, does not correlate well with the situations of greatest exposure.

Calluna and the gorse, *Ulex europaeus* L. (apparently including some of the subspecies *U. Gallii* Planchon), are both very plastic, old plants in flowering condition sometimes being compactly caespitose and only a few centimeters tall. In fact, on the upper slopes one had carefully to select a place to sit down in the grass for the gorse, although diminutive in stature and obscured in the sward, was just as effective as to needle-sharp branches as were the plants of the lower slopes which were a meter or two tall.

The principal vegetation type of Pic la Rhune may be called Gorse and Bracken Heath because of the prevalence of *Ulex, Pteris,* and the ericaceous plants mentioned above. There is an intermixed grass cover which becomes dominant in the spots of better soil. None of the grass species was found in a condition suitable for identification by one unfamiliar with the Gramineae of the area so, unfortunately, this alternate association must pass without comment. The prevailing heath is undoubtedly what Clements

would term a disclimax, or stable vegetation resulting from abuse. Overgrazing, fire, and deforestation through the centuries have reduced a potential woodland climax to the stable gorse and bracken heath. Sheep, goats, and ponies range the slopes, exerting a considerable pressure on the grass and favoring the dominants because of their general unpalatability. Without considering historical records, there are several reasons for believing that the slopes of Pic la Rhune and similar mountains in the area were once forested. At the picturesque nearby village of Sare there is a three-centuries-old Basque church in which the main supporting timbers, said to be of local origin, are nearly 2 feet square and 30 feet long. No such timber is to be found growing anywhere in the area today. In the second place, it is true in the south of France and in the Mediterranean area generally that overgrazing and repeated fire have caused vegetational deterioration and accompanying acceleration of erosion of the uppersoil horizons. Finally, of course, at this latitude and under the prevailing climate a climatic timber line would lie at an altitude very much greater than any attained in the western Pyrénées around Pic la Rhune.

After we had climbed above the foot of the mountain at Col de St. Ignace and had entered the heath there was only a scattering of a few trees here and there in the bottoms of the valleys. These were *Salix caprea* L., *Castanea vulgaris* Lam., *Sorbus aucuparia* L., *Ilex aquifolium* L., and *Quercus pedunculata* Ehrh., and they were soon left behind. It is rather interesting and unexpected that at one spot about a fourth of the way up the mountain someone had planted two trees each of *Quercus borealis* and *Liriodendron Tulipifera* L., which seemed to be growing well. At the Halfway House we found a fairly extensive windbreak which included *Cedrus Libani* Barrel., *Cupressus fastigiata* DC., *Picea excelsa* Link, *Pinus Strobus* L., *Fagus silvatica* L., *Betula pubescens* Ehrh., *Platanus vulgaris* Spach., *Fraxinus excelsior* L., and *Pinus halepensis* Mill. Above Halfway House only the *Ilex* and *Sorbus* were noted as native and spontaneous trees, and they were rare and depauperate. Although the soil is rather more deep and moist in the vicinity of the windbreak than is general on the mountain slopes, it would seem that reforestation is feasible.

The dominant gorse, *Ulex europaeus* L., displays its papilionaceous flowers with the fragrance of apricot in the late winter and early spring, reflowering in August and September and sometimes even in early winter. The wicked spiny branches are produced in the axils of spiny leaves. In the closely related *U. Gallii* Planchon, which is common on Pic la Rhune and generally in the southwest of France, the branches are often inclined downward and strongly curved, making even more effective weapons than the straight thorns of the principal species. Although *U. Gallii* is confined to the southwest of France, *U. europaeus* is widely spread in western Europe, extending in the north to Denmark and in the south to Northern Italy. In the southwest, west, and center of France the gorse often covers vast areas as the sole dominant among woody plants.

Pteris aquilina L., which is perhaps as nearly cosmopolitan as any species, is abundant at all elevations on Pic la Rhune. It reflects moisture conditions very well, varying from a height of less than one-half meter in the drier more exposed places to as much as two and one-half meters in moist shaded spots in the valleys. No other fern is of widespread occurrence on the Pic although several species are found on moist, protected rocky slopes and cliffs at lower elevations. The following were noted: *Osmunda regalis* L., *Polystichum Filix-mas* Roth, *Athyrium Filix-femina* Roth, *Acrostichum Thelipteris* L., *Polypodium vulgare* L., *Asplenium Trichomanes* L., *Blechnum spicant* Roth, *Ceterarch officinarum* Willd., and *Scolopendrium officinale* DC. Only the *Asplenium* and *Polypodium* were seen among the rocks at the top of the mountain. All of these species are widespread, more or less common in France, nearly all are circumboreal, and most of them occur also in northern Africa. From one to ten or more varieties are known in France for these species, but none was collected on Pic la Rhune.

The students were interested to find the circumboreal sundew, *Drosera rotundifolia* L., in some beds of moist sphagnum and to see for themselves that the glandular hairs on the leaves really were capable of catching

various kinds of small insects. In the same moist place there is a considerable amount of *Juncus conglomeratus* L., a near relative of *J. communis* E. Mey. Near the top of the mountain, incidentally, we found clumps of *J. glaucus* Ehrh., as well as the former species. Scrambling through the heath at all elevations, but never in any quantity, were diminutive, delicate, and very lovely plants of *Whalenbergia hederacea* Rchb. This campanulaceous genus was entirely new to me, and the species seemed most inappropriate among its harsh and more rugged-looking companions. The genus, which is moderately large with its center of development in South Africa, is, I believe, represented in western Europe only by the above species which ranges from the Iberian Peninsula to the British Isles and across France into Denmark and western Germany. Another attractive plant in flower at the time of our excursion was *Gentiana Pneumonanthe* L., which we have found in several other places in southwestern France and which is said to occur eastward to Siberia.

In soil pockets among the rocks and cliffs around the top of the mountain we discovered *Allium fallax* Don in full bloom and the basal leaves of *Oxyria digyna* Hill, which I had last seen in the Snowy Range of the Medicine Bow Mountains of Wyoming at about 3500 meters. This species occurs widely in Arctic and alpine zones around the Northern Hemisphere. At Pic la Rhune it must be at about its lowest altitude for the latitude. In the Alps it reaches nearly to 4000 meters. Among the same rocks, beside the common plants occurring at all elevations on the Pic, we found a little *Vaccinium Myrtillus* L., *Viola canina* L., and *Solidago Virga-aurea* L. The goldenrod seems to differ in no essential respect from that which prevails at the foot of the mountain. On the dunes at Chambre d'Amour and in sandy places elsewhere along the coast we had found, however, quantities of the closely related *Solidago macrorhiza* Lange. It is probably an ectotype of *S. Virga-aurea.* Three species of sedum were collected. We were able to identify only two of them. The most abundant is *Sedum album* L., which occurs widely in Asia, Europe, and Africa, and *S. hirsutum* All. of the Iberian Peninsula, France, and northern Italy.

Some other plants of the upper slopes of the mountain which were in flower and which, consequently, we could identify were *Bellis perennis* L., *Taraxacum Dens-leonis* Desf., *Serratula tinctoria* L. (the juice of which yields a yellow dye), *Anthemis arvensis* L., *Geranium molle* L., *G. Robertianum* L., *V. nummularia* Pourr (of the French and Spanish Pyrénées), *Polygala depressa* Wender, *Scabiosa Columbaria* L., *Brunella vulgaris* L., and *Potentilla procumbens* Sibth.

Our list for the trip was concluded by such common and often weedy plants as *Sherardia arvensis* L., *Peucedanum Oreoselium* Moench., *Digitalis purpurea* L., *Hypericum montanum* L., *H. pulchrum* L., *Polygonum Hydropiper* L., *Trifolium pratense* L., *Verbena officinalis* L., *Ranunculus nemorosus* DC., *Cerastium vulgatum* L., and, of course, *Stellaria media* Cyr.

Although we collected enough to keep us busy for a while and saw many plants that were new for us, the flora of Pic la Rhune is not a very rich one and, generally, presents a monotonous aspect. However, the weather was glorious and the scenery memorably beautiful. Furthermore, we were interested in some demolished German installations. They apparently had operated weather and radar stations on the Pic, and the upper slopes had scattered lookout posts and gun emplacements which were cleverly built so as to blend into the rocks and sod of the terrain at a surprisingly short distance. We even came across the widely scattered remnants of a German fighter plane which had crashed on the upper slopes.

Happy, but tired and thirsty on our return to Col de St. Ignace (one of the soldiers remarked that he certainly would have griped had the Army made him work as hard), we drove directly to the nearby village of Sare and its tavern with tables under pollard sycamores. After a round or two of what seemed like France's best muscatel, we thought even we could understand the strange Basque language, but perhaps it was just friendliness which is understood though the words are strange.

APOLOGIA

By Clarence R. Wylie, Jr.
ASSISTANT PROFESSOR OF MATHEMATICS, THE OHIO STATE UNIVERSITY

There are among us giants whom we know
And knowing, honor; and a few perchance
Cloaked in obscurity to whom our sons
Will look with awe a generation hence,
And marvel that we recognized them not.
But count them all, those who are surely great
And those whom kindly judgment may appraise
As bearing seeds of greatness, and how few,
How pitiably few there are whose work
Time will endorse as more than trivial.

How then can we defend the weary hours,
Tired eyes, and futile thoughts with which we seek
Our small successes? What the urgency
That keeps us diligent beyond our strength?
Cynics have answers that we scorn as lies:
"Rank and its perquisites." "One's name in print."
Damned be the charge! Who claims it past belief
That men should labor at the heart's command,
And count the struggle dearer than the prize
Their uninspired hands may never hold!

It's said the French who fought at Waterloo
Under Napoleon, in their later years
Were strangely silent on the part they played
In that great gamble; and when pressed to speak
Would only say, almost with reverence,
"Oui, j 'étais la," as though the simple fact
That they had served their leader and his cause
To the last hour, was all the fame they asked.

Is it less so with us whose battle flag
Bears the word *Science,* and whose leaders are
Not blustering soldiers but men of the stamp
Of Darwin, Pasteur, Faraday, and Gauss?
This is the urge that keeps us at our work:
An inner oneness with such men as these,
Whose faith is ours and by whose side we stand.

EUGEN STEINACH, 1861-1944: A LIFE OF RESEARCH*

By HARRY BENJAMIN, M.D.

THIS is the story of revolutionary medical research. It is also the story of a scientist who dared to penetrate the primeval forest of human prejudices and sex taboos and who added immeasurably to human happiness. He earned but little gratitude in his lifetime. Through his investigations and discoveries, he laid the cornerstone for a new medical specialty: the endocrine treatment of old age. Now called gerontotherapy, it endeavors to add life to years; maybe, also, years to life. Who knows?

The closing years of the last century had witnessed a profound change in the philosophy of medicine. Medical science had outgrown the preceding period of a purely speculative natural philosophy, and a new era had begun in which life in all its manifestations was interpreted chiefly in mechanistic and chemical terms. Life was regarded as a chemical accident; the human body as an immensely complicated machine subservient to the laws of physics and biochemistry. Such was the new world of scientific thought when Eugen Steinach entered his special field of medical research by publishing the first original investigations that dealt with the physiology of sex. The year was 1894, and Steinach was 33 years old. It was the birth year of a new field of scientific research.

Physiologists already knew Steinach from numerous and varied publications, but it was a turning point in his career when his series of three articles on the "Comparative Physiology of the Male Sex Organs" appeared in *Pflüger's Archiv*. The first of these investigations can be called a "negative" one, and according to Steinach's own statement it was based on an "accident." He had seen an article by Tarchaloff in which the theory was proclaimed that the sexual "embracing reflex" of the frog was due to a nervous mechanism elicited by the tension of completely filled seminal vesicles. Steinach set out to test this theory. He removed the seminal vesicles not only of frogs but also of rats and found that sex instinct and sexual response were in no way altered. Therefore, the seminal vesicles could not possibly have the significance that Tarchaloff had ascribed to them, and so his theory was conclusively disproved.

But if this was not the explanation for the release of the sex instinct, what then could it be that influenced the psyche in this specific manner and led to the characteristic manifestations of sex? This was the question Steinach asked himself; to answer it he set in motion a chain of experiments that determined almost his entire life's work. One novel experiment followed the other, and each was the logical outgrowth of the preceding one. From then on, Eugen Steinach was inseparably identified with the biology of sex. All his previous researches in other fields of physiology receded into the background.

PRE-SEXGLAND PERIOD

Born on January 27, 1861, in Hohenems, a small town in the Austrian Alps, young Eugen grew up in an atmosphere of medicine. His father as well as his grandfather were practicing physicians. The grandfather was also an enthusiastic and successful cattle-breeder, a fact which bore fruit many years later when his grandson did research work on sterility in cows.

After finishing high school Steinach went to Geneva, Switzerland, to study natural sciences and soon afterward to the University of Vienna to devote himself to medicine. In 1886 he graduated and became doctor of medicine. During the following two years he worked as assistant in the Physiological Institute in Innsbruck and published a few minor contributions, such as "Studies on the Blood Circulation of the Kidney"; "Time-Reaction of Temperature-Sensations," and

* The illustrations of experimental animals in this biography are reproduced from Steinach's *Sex and Life* with permission of the Viking Press, Inc., New York. The autographed portrait of Professor Steinach at his desk is taken from Dr. Heinrich Meng's *Psyche und Hormon* with permission of the author and the publisher; i.e., Medizinischer Verlag Hans Huber, Bern, Switzerland.

"Experiments to Measure Temperature and Pressure-Sensation Time."

His deep interest in physiology soon brought the young research scientist under the influence of Ewald Hering, one of the greatest physiologists of his time, and in 1889 we find Steinach as his pupil and first assistant in the Physiological Institute of the German University in Prague. Here began the first period of Steinach's scientific career, a period which can be designated as "pre-sexgland." It extended roughly to 1910, and, up to this time, the only sex-physiological investigation was the one previously mentioned dealing with the male sex organs.

Steinach remained in Prague over 20 years. He became *Privatdozent* two years after his arrival and five years later was named *ausserordentlicher* professor of physiology. In 1907 the University of Prague conferred on him the *ordentlicher* professorship. Steinach, then 46 years old, was able to look back already on a distinguished, though hardly exceptional, career. He had ambition, initiative, and originality, which were particularly demonstrated in 1902 when he created and organized a laboratory for "general and comparative physiology," the first of its kind in German-speaking countries.

It was principally in this laboratory that a great number of various researches were conducted. Among other subjects they dealt with "Visceromotor Functions of the Posterior Roots of the Medulla Oblongata" and "Chromatophore Muscles of the Cephalopods." In "True Contractility in Motor Innervations of Capillaries" (*Pflüger's Archiv,* 1903) Steinach described original observations of electrical stimuli on the circular muscles of capillaries with photographic studies. In 1908 he published a noteworthy contribution on "Summation of Singly Ineffective Stimuli as a General Phenomenon of Life."

More important than these, however, were earlier series of researches on the "Comparative Physiology of the Iris," which appeared in *Pflüger's Archiv* in 1890 and 1892. They reflect clearly the influence, if not the pressure, of Steinach's great teacher Hering, who himself had contributed so much to the physiology of the eye and the problems of vision.

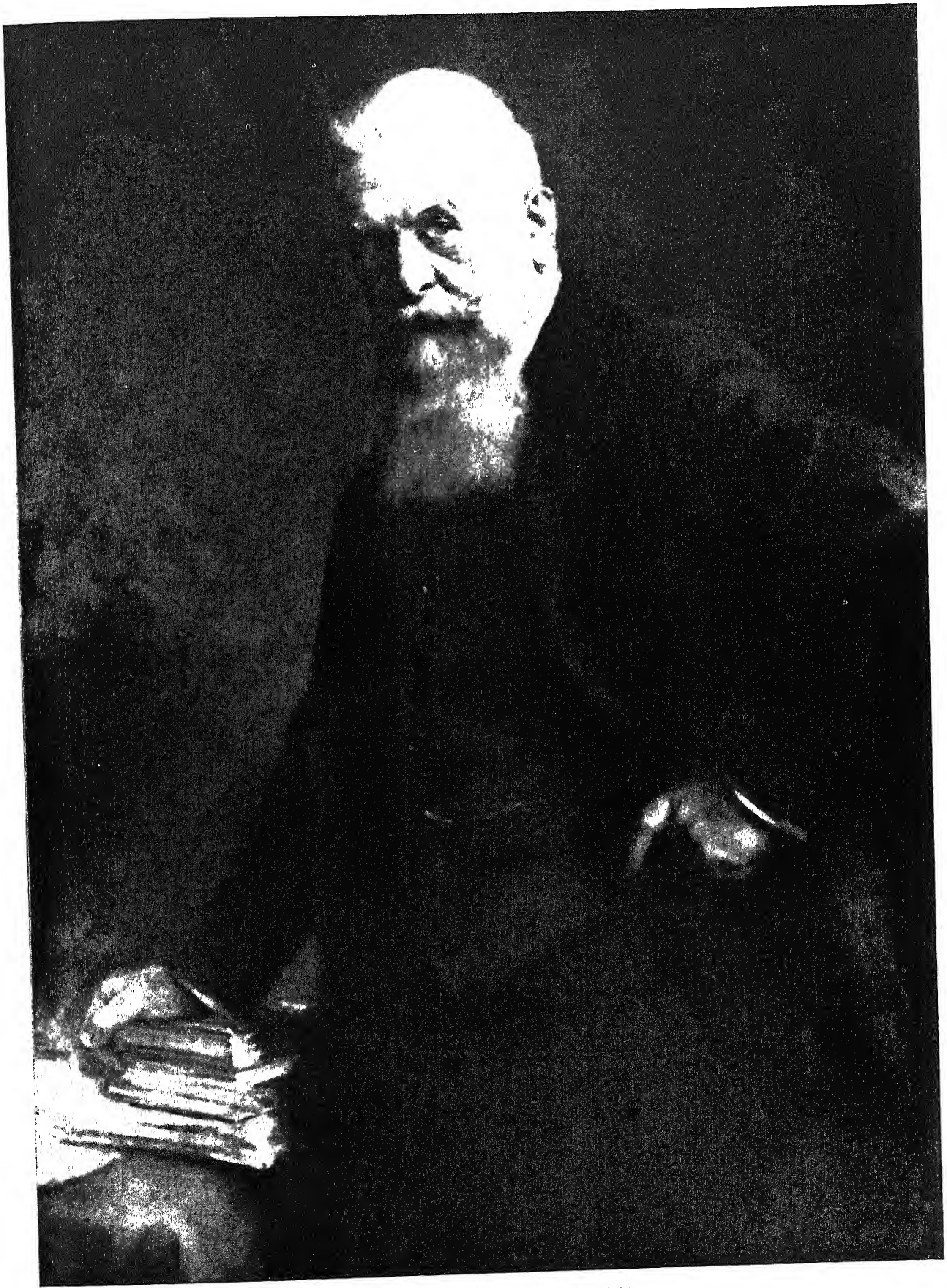
In later years, when Steinach's name had become world famous as a sex biologist, he used to refer to the work in the "pre-sexgland period" with hardly more than a shrug of the shoulders. He considered the experiments "not entirely satisfactory" and once said to his assistant and collaborator, H. V. Klein: "When there is not enough love and devotion to one's work, one can hardly expect much of a success."

Physiology of the Testis

Although this unduly pessimistic attitude toward his own work applied to the first period of Steinach's career, the extreme opposite prevailed in later periods. He was then "heart and soul" in his studies of sex biology to which he devoted more and more time during the later years of his stay in Prague. These studies, which were principally concerned with the physiology of the testis, initiated the second period in Steinach's research life. It was during this time—roughly, from 1910 to 1920—that Steinach's original mind led to his greatest accomplishments and his most brilliant success.

The year 1910 saw the 49-year-old scientist still in Prague teaching physiology and conducting or supervising experiments. Among the latter were already those in sex biology which paved the way for the coming work in Vienna and which laid the foundation of his future fame.

The consequences of castration, well known for a long time, were in these years no longer explained by a severance of nerve fibers connecting brain and testes when the latter were removed. This theory had been exploded. The physiologist von Berthold, by his transplantation experiments on roosters in 1849, had furnished evidence that something in the circulating blood constituted the link between sexgland and brain. Unable, however, to identify the particular agent any closer, Berthold could go no further, and his experiments were soon forgotten. Besides, they had one great drawback that detracted from their scientific reliability: The testes of fowls are most difficult to remove completely, as they are not surrounded by a firm capsule like those of mammals. If, therefore, even the smallest part of testicular substance re-



EUGEN STEINACH, 1861–1944

mained *in situ,* full castration had not taken place and valid conclusions could not be drawn.

Therefore, Steinach used male rats for his transplantation experiments and showed, first of all, more conclusively than ever before, how the gonads influence the secondary sex characters, including the sex instinct. This influence, however, did not mean a complete dependence. The first fundamental publication on the subject appeared in the *Zentralblatt für Physiologie,* 1910, and was called "Sex Instinct and True Secondary Sex Characters as a Result of the Endocrine Function of the Gonads." Although emphasizing the biological pre-existence of male or female sex and their respective secondary sex characters, Steinach showed that growth, maturation, and preservation of the latter were definitely controlled by the internal secretion of the testis. Remove the testes, and the seminal vesicles and prostate shrink; sexual lethargy develops. Implant a testis somewhere else in the body, and the physical as well as the psychic sex characters are restored.

All this sounds trite and commonplace now, but 35 years ago it was an important discovery, or at least a final confirmation of what had so far been mere assumption.

The Puberty Gland. The next logical question that Steinach now asked himself was: In what part of the testis does the internal secretion develop? Is the male hormone a product of the spermatogenetic tissue (that produces the spermatozoa) or is it secreted by the interstitial (Leydig) cells (forming the rest of the testicular tissue)? This question had been asked before, notably by two French scientists, Bouin and Ancel, in 1903. Through vasoligature experiments on rabbits and guinea pigs, they had come to the conclusion that the internal secretion was a function of the interstitial tissue. They called it *Glande interstitielle* and regarded it as the source of the hormone that controlled secondary sex characters. Their theory was accepted in some quarters and rejected in others. Steinach seized the opportunity presented by his own transplantation experiments (1910) to try to settle the question.

He had been the first experimenter who succeeded in transplanting a testis in such a way that it became vascularized and remained functioning in its new site. Infantile animals had been used. When they were castrated, they remained infantile. But they developed normally when the re-embedded gonad remained alive in the body. What kind of a gland was it now, that continued its function somewhere under the skin instead of in the scrotum? Years later, in his book *Sex and Life,* Steinach described the histological structure as follows:

> The spermatogenetic (germinal) tissue proved to be more sensitive and was unable to adapt itself to the altered conditions of blood supply, and therefore perished. The hormonic tissue, on the other hand, proved to be more robust and not only adapted itself readily and completely to the new environment, but also increased. The space made available by the atrophy of the germinal tissue was occupied by proliferating interstitial cells. Thus, as an incidental result of my transplantation experiments in 1910 and succeeding years, I found that I had changed testicular tissue into a gland consisting wholly of interstitial cells.

Since this gland promoted the maturing process, Steinach named it the "puberty gland."

The older, inconclusive experiments of Bouin and Ancel were now evidently confirmed, and the internal secretion of the testis had to be ascribed to the Leydig cells. In taking a decided stand in favor of this theory, Steinach entered the first of several scientific controversies in which he was a leading figure, if not the actual storm center. Objections to the "puberty gland theory" persisted in several scientific circles, where the internal secretion was still ascribed to the spermatogenetic tissue or to the Sertoli cells, which are a part of this tissue.

It would be only of historical interest to go into the details of this controversy, although it played undoubtedly a great role in Steinach's scientific trials and tribulations. It was finally decided in Steinach's favor (and naturally in favor of Bouin, Ancel, and their adherents) when two of his principal opponents withdrew their objections in 1926 and 1933, respectively. Steinach had to wait over twenty years for this recognition, but when it finally came—in a rather dramatic episode—it was one of the highlights in his

career. It was in Berlin in the fall of 1926 when the First International Congress of Sexual Research took place, and Steinach had delivered a lecture on "Sex Hormones, Their Biology, Physiology, Pathology, and Therapy." At that time he was already a world-famous figure in medicine on account of his publication on "rejuvenation" and was the "star speaker" of the Congress. The auditorium was crowded to overflowing. No discussion had been scheduled after Steinach's paper, but Dr. Albert Moll, the president of the Congress, arose and congratulated the speaker on his results, which he compared in fundamental importance to the Mendelian laws of heredity. After him, Dr. Oscar Riddle, distinguished biologist of the Carnegie Institution, who was chairman of this session, spoke briefly to recall the debt which the biology of sex owed to Steinach for his important contributions.

Everybody expected the meeting to close after these remarks, when Dr. Moll rose once more and announced that Professor Benda had asked for the floor to make a brief statement. The famous pathologist of the Berlin University had been known for years as one of the most bitter opponents of Steinach and of his "puberty gland" theory. The large audience expected another scathing attack—this time orally—and the tenseness was distinctly uncomfortable. I happened to be present, sitting next to Dr. Peter Schmidt, Steinach's foremost pupil in Germany, who whispered, "Watch the fireworks."

In this electric atmosphere Benda's words amounted to a near sensation. Instead of dissenting, he paid high tribute to Steinach and stated in so many words that, following new experiments of his own, he was ready to withdraw his objections and concede the correctness of Steinach's claims. When the two scientists then shook hands the applause amounted to a demonstration.

Benda's declaration had shaken the stronghold of Steinach's opposition, but the victory was only complete when the second great opponent, the anatomist B. Romeis, of the University of Munich, likewise revised and altered his standpoint in 1933 (*Klinische Wochenschrift,* No. 42) and in a truly scientific spirit admitted his error.

Let us return to the year 1912 when Steinach's stay in Prague came to an end. His publications had made a sufficiently deep impression in the scientific world to cause his appointment as research director in the Physiological Department of the Biological Institute of the Academy of Sciences in Vienna. At the same time the University of Vienna bestowed on him the title of *ausserordentlicher Professor.*

Steinach was 51 years old when he came to Vienna. With his striking head (on a pyknic body) and his impressive reddish-blond beard, he was soon a familiar figure in Vienna's *Prater,* where the Biological Institute was located. The Viennese called it the "Vivarium." Steinach was still a bachelor. He was a conscientious hard worker, who demanded much from his assistants. In all his scientific work he insisted upon logic in thought, clarity in expression, and uncompromising adherence to the purpose on hand. But away from the laboratory he was a man of the world, full of genuine humor and of a vitality that let him enjoy the good things in life to the fullest. Here, in Vienna, his greatest wish was at last realized: he could concentrate entirely on research and was no longer distracted by obligations to teach students.

Reversal of Sex Characters. The year of Steinach's removal to Vienna also saw the publication of his classic research work on "Intentional Transformation of Male Mammals into Animals with Pronounced Female Sex Characteristics and Feminine Psyche." These experiments were a natural outgrowth of the former investigations as to the significance of the sex hormones. Were these hormones unspecific as to sex, like the hormones of the thyroid, the adrenals, etc., or were they sex-specific? That is what Steinach wanted to know.

Infantile male guinea pigs were castrated. Into the resulting "neuter" an ovary was implanted. The developing sex characteristics were not those of the congenital masculine *anlage* but definitely feminine. The "foreign" female gland proved stronger than the male *anlage.* Milk-producing nipples appeared; the penis remained rudimentary,

resembling a clitoris. Body growth and configuration was more like those of a control sister than of the normal control brother. The forms of chest and head changed to the feminine type. The rough unkempt hair of the male was replaced by the softer, shorter hair of the female. Even the internal structures of the originally male animal, as revealed by the X-ray, were now like its sister and quite different from its twin brother. furthermore, were pursued by the normal males that attempted intercourse. They radiated genuine feminine attraction.

The opposite experiment was equally successful and striking. The young castrated female, under the influence of an implanted testis, turned into a "male" and grew a penis; even its voice assumed a characteristic masculine guttural sound instead of the well-known squeak of the female. This

CHANGING THE SEX CHARACTERS OF GUINEA PIGS
Above: MASCULINIZATION. *Left to right:* MASCULINIZED SISTER, SPAYED SISTER, NORMAL SISTER, NORMAL MALE.
Below, FEMINIZATION. *Left to right:* CASTRATED BROTHER, NORMAL SISTER, FEMINIZED BROTHER, NORMAL MALE.

More astonishing even than the physical transformation was the change in the psyche. Normally, male guinea pigs show no paternal instinct. They repulse the young ones. But the "feminized" male behaves like a mother and actually suckles the young. When Steinach demonstrated these animals and their behavior before a Physiological Congress, they created a sensation. They made medical history. These feminized males, masculinized female, like a normal male, also scents a female in heat, pursues her, and attempts sexual contact. It was this erotization of the brain in the "sexgland-specific direction" that impressed Steinach most and led to many future inquiries into the nature of the sex urge. For the first time the relationship of hormone and psyche was brought into focus. Steinach and Freud (who had met years ago as students) had now found a

contact in their respective researches—the one proceeding from the soma, the other from the psyche. The body-mind problem was moving closer to a solution.

After the appearance of classic "Feminization of Males and Masculinization of Females" (*Zentralblatt für Physiologie,* 1913) Steinach's name as a foremost sexologist was firmly established. The technique of his experiments was impeccable, combining imagination with strict discipline. In many laboratories the experiments were repeated and confirmed. Knud Sand in Copenhagen feminized roosters and masculinized hens. Their pictures can be found together with Steinach's guinea pigs in many scientific textbooks.

Studies of Homosexuality. Numerous investigations and publications followed in consequence and in elaboration of the classic experiments of sex reversal. In collaboration with the renowned Viennese roentgenologist Holzknecht, Steinach tested the effect of X-rays on the ovaries of infantile female guinea pigs (1916) and found that these rays in small ("stimulating") doses caused an acceleration of growth of uterus, mammary glands, and nipples, that is to say, an experimental precocity. In a publication "Puberty Gland and Hermaphroditism," Steinach reported the effect of simultaneous implantation of testicle and ovaries into infantile castrates and described the resulting "experimental hermaphroditism" (*Archiv für Entwicklungsmechanik,* 1916). These investigations were in turn the cause of two further groups of experiments: "Inquiries into the Nature of Homosexuality" and "Proof of the Antagonistic Effect of Male and Female Hormones."

In 1918 (*Münch. Med. Wochenschrift*) Steinach and the Viennese urologist Lichtenstern published a paper with the optimistic title "Conversion of Homosexuality through Exchange of Puberty Glands." This was a practical application of Steinach's researches. Homosexual men were castrated and another man's cryptorchic (undescended) testicle, which consists of predominantly interstitial tissue, was implanted. The homosexual tendencies disappeared and a normal (heterosexual) libido developed. Such, at least, was the conclusion that the authors drew from the report of their patients.

In the light of a modern concept of homosexuality, which sees an endless variety of combinations between homosexual and heterosexual tendencies, and between constitutional and psychological factors, this assumption of an exclusive endocrine etiology of sex inversion and its clinical cure by endocrine therapy must be taken *cum grano salis.* Organic cases of intersexuality may indeed be influenced by glandular methods, but Steinach's and Lichtenstern's generalization is hardly justified.

Another controversy resulted from Steinach's histological examination of testicles of homosexual men, which he described as showing the following abnormal characteristics (*Archiv für Entwicklungsmechanik,* 1920): "Atrophy of spermatic tissue, partial degeneration of the male interstitial cells, presence of large cells, differing widely from the former and approaching in appearance and structure the female cell types." Steinach referred to them as "F cells," but to this day their existence has not been generally recognized.

Antagonism of Sex Hormones. Also in the *Archiv für Entwicklungsmechanik,* 1920, appeared a series of articles dealing with "Artificial and Natural Hermaphrodite Glands and Their Analogous Effects." In the first and most important part, on "Antagonistic and Sex Specific Effects of Sex Hormones," the author comes to these conclusions: Sex hormones are sex-specific, that is, the male hormone develops the *anlagen* of male characteristics, the female hormone doing the same for the female *anlagen.* Hand in hand with this effect on the like sex goes an antagonistic (inhibitory) influence on the characteristics of the opposite sex. Their development is prevented, retarded, or even brought to involution.

Opposition to this theory of the physiological antagonism of sex hormones still exists but remains unconvincing in view of many corroborating experiments, especially those of Alexander Lipschuetz.

The year 1920 also saw the publication

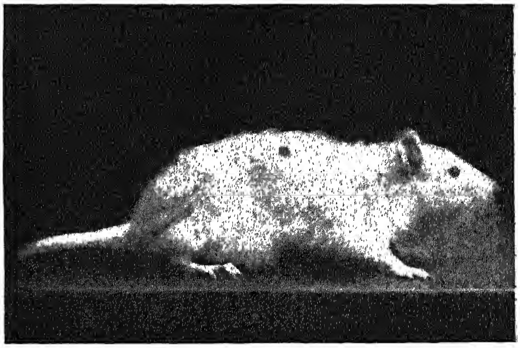
of a rather extensive study "Climate and Puberty" by Steinach in collaboration with the zoologist Paul Kammerer (whose outstanding work in heredity is as well remembered by his colleagues as is his tragic death in 1926). The authors showed that rats kept in higher temperatures developed a proliferation of interstitial cells in their testes and consequently an enlargement of secondary sex characters together with a high degree of sexual excitability. It was not difficult to find the analogy of these "heat-rats" with the proverbial sexual ardor of men and women in the Tropics. Steinach and Freud had met once more.

Rejuvenation. As far as publications are concerned, 1920 was undoubtedly for Steinach not only the most fruitful but also the most fateful year of his research life. In this year appeared his little book *Rejuvenation Through the Experimental Revitalization of the Aging Puberty Gland* (J. Springer, Berlin), and almost overnight Steinach became one of the most talked-of men of science all over the world.

In the medical profession he was soon praised as a genius by some and condemned as a quack by others, while the laity saw in him a modern Ponce de León on the one hand and a violator of sacred laws of nature and morality on the other.

Steinach's thoughts and studies on the problem of old age date back to 1912 when a paper "Investigation of Youth and Age" was submitted to the Vienna Academy of Sciences but remained buried there for nine years. Steinach had observed and, for the sake of priority, described in this early paper the striking resemblance of the effects of castration and those of senility. In both instances the lack of sex hormones produced the similarity of the clinical pictures. The inevitable thought then occurred that both conditions could be remedied by supplying the missing hormones.

The transplanting of a foreign gonad into a castrate had proved successful in curing or ameliorating the characteristic signs of deficiency. Gland transplantation into an aging body was, therefore, the first method to be considered, inasmuch as it had been successful in numerous animal experiments. The injections of hormone solutions in the manner of Brown-Sèquard was another possibility. Steinach did not favor either one. Transplantation would be impractical on a larger scale. Where should all the glands come from? (Voronoff, in Paris, tried to solve this problem by resorting to the use of testicles from anthropoid apes). Hormone solutions were too uncertain in their effects and also difficult to procure in those early days, long before the advent of biologically tested or synthetic androgenic preparations. Steinach wanted a wider human application, not one that could benefit only aging rats and guinea pigs.

REVIVIFYING EFFECT OF HORMONE TREATMENT ON A RAT
Left: SENILE FEMALE RAT BEFORE ADMINISTRATION OF A FEMALE HORMONE. *Right:* THE SAME AFTER TREATMENT.

Thus arrived the moment when this ingenious scientist had a truly "royal" thought: foreign materials should not be employed to reactivate the senescent system

but the person's own sex gland should be made to produce the necessary hormone increase. The body's own resources must be mobilized. This is the principle underlying all those therapeutic methods which justly deserve the designation "Steinach therapy."

Vasoligation was the first practical result of this idea, and the small operation soon bore Steinach's name. Bouin and Ancel had used the ligation of the vas deferens previously but for experimental and theoretical purposes only. Now Steinach wanted to apply it in the practice of medicine and to combat old age by taking advantage of its effect on the histological structure of the testis.

REVIVIFYING EFFECT OF VASOLIGATION ON A RAT
Left: SENILE MALE RAT BEFORE LIGATING ITS VAS DEFERENS. *Right:* THE SAME ANIMAL AFTER THE OPERATION.

The vas deferens is the canal through which the spermatozoa reach the outside (i.e., the urethra). If this canal is sealed through a permanent ligature, the spermatozoa are dammed back into the gland, gradually causing a pressure atrophy of the very delicate germinal epithelium from which they originate. This, at least, was Steinach's reasoning. The place of the disappearing cells is taken by the much hardier interstitial cells which—according to the Steinach school—proliferate and consequently produce more hormone. At the height of the atrophy of one tissue and proliferation of the other the testis was said to resemble a cryptorchic or a transplanted gland.

Although this theory of the histological consequence of vasoligation seemed amply supported by exact laboratory studies, microscope slides, and animal specimens, it touched off nevertheless another scientific controversy, this time especially vehement on account of its practical clinical implications, namely, the claim that vasoligation "rejuvenates" not only animals but men as well, though to a lesser degree.

Steinach had waited for years with this pronouncement, and therefore the storm in the medical world broke only after the aforementioned book on rejuvenation appeared in 1920. It dealt at length with all the preliminary animal experimentation but also contained case histories of "rejuvenated" men.

The Theoretical Controversy. The opposition to the "Steinach operation" was twofold. One was directed against the theory of the proliferation[1] of interstitial cells following vasoligation, therefore concerning the laboratory worker. The other was of a clinical nature. The reactivating effect of the operation was denied, for animals as well as men, more often for men only, thereby limiting the debate to the medical profession.

It would be quite impossible to review here the enormous scientific literature that emanated from laboratories and operating rooms all over the world. Among others, Romeis in the *Handbuch der Innern Secretion* (1931) made a complete survey of this literature, and so did Lipschuetz in his book *The Inter-*

[1] According to experiments by Romeis, Schinz and Slotopolsky, Moore, and others, this proliferation is not real but appears to be such only in comparison to the atrophied spermatogenetic tissue. The number of Leydig cells are declared to remain the same although their individual size may increase.

nal Secretions of the Sex Glands and Allen in *Sex and Internal Secretions.* It seems that a majority of the authors generally confirmed Steinach's histological findings, although many found themselves in variance as to explanation and the degree of the changes that occurred in the testicle after vasoligation. In the United States a group of zoologists in Chicago roundly denied that any hypertrophy of interstitial cells took place after the vas deferens was ligated and, therefore, declared not only that Steinach's theory was wrong but also, consequently, that no clinical effects could be expected from the operation. If anyone claimed to have seen such effects, they said, he was just fooling himself and others. This disapproval of Steinach's research claims was the determining factor for a similar disapproval of their clinical application by the American Medical Association.

In justice to Steinach and the many "pro-Steinach" investigators, one should recall an old truth in medicine which can likewise be applied to physiological experiments: Positive findings are of greater significance than negative ones. Positive findings are a reality that call for explanation. Negative findings mean little in comparison. Therefore, it seems logical to ascribe more credence to those authors who saw changes after vasoligation than to those who did not see them.

Several factors may explain the divergence of findings: The technique of the operation, the period of time that elapsed between the ligation and the examination of the testis (under the influence of an increased hormone production from the "puberty gland" a regeneration of atrophied spermatogenetic tissue is said to take place), the type and age of the animal, the method by which the changes were evaluated. In any event, the final word as to the effect of vasoligation on the structure of the testis is still to be spoken.

"Within modest limits, the process of aging can be influenced." Such was Steinach's own summarization of the result of the operation. Two points were emphasized before he recorded the effects: First, the proper technique of the operation that must seal the vas deferens permanently and must preserve blood and nerve supply to the testis. Secondly, the proper selection of the patient. A favorable response would then "revivify" the entire glandular system, and a mental, physical, and, incidentally, sexual invigoration could take place.

Steinach himself never operated, nor did he consider himself a competent clinical investigator. He was the research scientist first and foremost. He recorded merely the objective and subjective changes that clinicians had observed in their patients and depended upon their statistics which gradually began to appear in the international medical literature. The reports were by no means uniform. There were enthusiastic confirmations; there were almost complete denials of any beneficial results; and there were guarded reports with the usual appeals to reserve judgment until more experience had been gained. "Positive" reports were generally based on much larger statistics than "negative" ones, which is understandable from the physician's point of view.

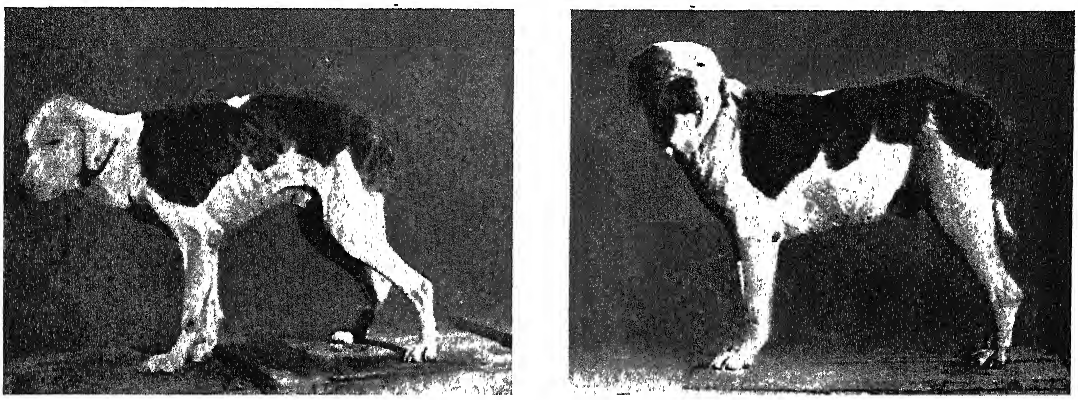
In some reports like those from Russia (Eiber and Uspensky) the benefits on physical strength and endurance of laborers were emphasized, while in the majority of others dealing predominantly with the intellectual classes the restoration or preservation of mental faculties was in the foreground.

A few individual cases received wide publicity, such as that of Professor Adolf Lorenz, the famous Viennese orthopedist, who ascribed his return to active practice at the age of 72, after his retirement, to a successful "Steinach."

Known in Vienna but not published was the fact that during the 1920's well over 100 university professors and teachers underwent vasoligation, among them Sigmund Freud, in order to improve their health and extend "the platform of their efficiency." Freud, as well as others, was emphatic that such had been accomplished, but exact scientific data are missing.

The Clinical Controversy. Exclusive of these honest and independent members of the medical fraternity who reserved judgment as to the value of the operation because they had no personal experience and did not

feel competent to express an opinion, there were roughly three different groups of physicians who took a stand in the controversy: First, there were those who roundly denied any results from the operation, simply because they had not seen any or considered Steinach's theory to be faulty. They pronounced the benefits reported by others as due to "autosuggestion," and the successes with animals were denied or ignored. Secondly, there were those who likewise believed Steinach's theory to be wrong but did not deny clinical benefits, which they ascribed, for instance, to absorption of degenerating spermatogenetic tissue. In accordance with this theory they claimed that the benefit to the patient was only of relatively short duration. Finally, there were those who believed Steinach to be most likely correct in his theory because, with it, they could best explain the invigorating and re-energizing results on body and mind that they had observed in their patients (or some of them in themselves), especially if this result seemed to last for many years.



REVIVIFYING EFFECT OF VASOLIGATION ON A DOG
Left: EIGHTEEN-YEAR-OLD DOG BEFORE VASOLIGATION. *Right:* THE SAME DOG AFTER THE OPERATION (WILHELM).

But the opposition to Steinach was powerful and persistent in its arguments. The term "rejuvenation" easily invited criticism. It was unfortunately chosen, as Steinach found to his regret, for it led to exaggerated expectations of patients and doctors and consequent disappointments, and it produced an unavoidable echo in the lay press and a publicity that wallowed in sexual implications. Later Steinach replaced "rejuvenation" by the more appropriate term "reactivation," which was first proposed by this writer in *American Medicine,* August 1922. The only "sexual" factor in "rejuvenation" was the use of the sex gland. A restoration of sexual potency was incident to the general re-energization of aging men and was only mentioned as such. But this could never satisfy a sensation-hungry press and public or quiet the vaudeville comedians who were untiring in confusing the methods of Steinach with "monkey glands." Consequently more criticism arose, and the serious scientific efforts of Steinach and his co-workers were severely handicapped.

Then came the "Steinach Film" and the wide publicity it created in 1922. A German film concern made a full-length moving picture record of some of Steinach's outstanding experiments. There was an authorized scientific version as well as a popular one. The public loved it, but the medical profession in Germany and Austria frowned. Steinach was raving mad and claimed never to have given consent for the popular form.

Some of Steinach's co-workers indulged in uncritical praise of the effects of vasoligation. Their overenthusiasm and that of lay-writers on medical subjects was then rivaled by equally uncritical opposition on the part of some medical editors and authors. Steinach and his collaborators were, for instance, blamed for a publicity and an exaggerated public interest that was clearly due to the nature of their work. Again, in justice to

Steinach, we must remind ourselves that there are always critics in every branch of human endeavor who write their comments not in the light of facts and logic but in the twilight of their individual prejudices, their own likes and dislikes.

Neither Steinach nor some of his pupils can be absolved entirely from responsibility for the fact that the controversy was often conducted in an undignified manner. Occasionally, rejuvenation claims were attributed to Steinach which he had never made (the newspapers had done so), and straw men were set up and knocked down. Unnecessarily sharp rejoinders followed. The acrimony of the debate betrayed its high emotional potential. Steinach, whose vanity was easily hurt, suffered greatly under all these attacks, some of them surely unjust. They left their mark on him psychologically throughout the rest of his life. He became unduly suspicious even toward his friends and easily antagonized people. He was too deeply hurt himself to realize when he hurt others.

Now the controversy over the "Steinach operation" has died down. The development of the synthetic testosterone has reduced interest in the operation. Steinach had the satisfaction of having watched its great popularity and of having heard from many supporters and grateful patients. He had the disappointment of its lack of universal recognition. He himself blamed this failure on prejudices and enmity in the profession, on a frequent faulty technique of the operation, and also on puritanical influences which opposed sex in all its implications, even in utilizing the sex gland for promoting better health in later years. By way of illustration, he liked to show a letter which he had received from a Swedish clergyman who threatened him with Hell and damnation if he continued any longer to interfere with the divine laws of nature.

After the portentous year 1920, Steinach published one more brief paper on the "rejuvenation" operation. He had to take issue with the argument that his rats were suffering from mange but were not old when they were used for rejuvenation experiments; that disease, not senility, accounted for their hair changes. Steinach had to bring proof that these rats were undoubtedly senile, as they had grown old in the laboratory under constant control and that their loss of hair was natural and not due to infection. ("Mange and the Combat of Senility," *Wien. Med. Woch.*, 1921.)

For the following five years no publication came from Steinach's laboratories, although research was never entirely interrupted. Those were the years of monetary inflation and of Austria's greatest economic plight. Government subsidies had practically ceased for all research work, and Steinach had to keep the laboratory with several assistants going the best he could out of his own pocket. Fortunately, patients came to him from all over the world for consultation and operation. Though he did not operate himself (except on animals), he was nevertheless present during all operations, which were performed by such competent surgeons as Robert Lichtenstern and Erwin Horner. In Steinach's waiting room, laboratory, or hospital the rich and the famous from abroad met with the poor and the famous from home. Those were Steinach's busiest years. It was often a problem to do justice to his clinical, research, and literary work and yet allow time for the many visitors, both doctors and laymen.

Steinach was now in his early sixties. A few years earlier he had married the cultured widow of a noted attorney. With her command of English and French she was an invaluable assistant. Who does not remember her as the gentle soft-spoken "secretary" anxious to please everybody, not the least (and not the easiest) the *Herr Professor?* Steinach was sometimes "difficult" indeed. He was a man who made enemies easier than friends. He was touchy, quick-tempered, and—having been "burned" too often—forever suspicious. The *Frau Professor* frequently had a hard time trying to smooth ruffled feelings.

On other occasions Steinach, in his ambivalent nature, was depressed, looked at himself as a martyr, and became resigned to being misunderstood. Fortunately, his moods were of a passing nature, and he was able to forget his irritation and his mistrust, to be again a

sincere friend, a liberal teacher, a sympathetic physician, and a gracious host. A moderate prosperity had come to him, and, being a man who forever said "yes" to life, he enjoyed his hobbies and pleasures fully. He loved horses. For many years he could be seen in the *Prater* on horseback or driving a carriage. He was a handsome man in spite of his short stature. His white hair, his graying beard, and sparkling dark eyes, which could flash so angrily and smile so kindly, were not easily overlooked or forgotten.

BEFORE THE WAR
STEINACH AND HIS FAVORITE HORSE IN VIENNA, 1937.

Biochemistry of the Hormones

The Female Hormone. The third period in Steinach's research career, which roughly extended from 1920 (or 1925) to 1938, began with an interest in the female hormone and in female reactivation.

The use of the X-rays in small doses as worked out with Holzknecht for the maturation of infantile animals had occasionally good results in aging women but was not the final answer to the problem. Nor was it the transplantation of ovaries. "Steinach therapy" (the stimulating "autoplastic" method) had to be replaced or supplemented by a "homoplastic substitution therapy." Moderately effective ovarian hormone solutions had already been made by others, but to Steinach belongs the credit of developing the first standardized preparation of high potency and also of using such female hormone for the purpose of reactivation.

The test for such an ovarian preparation was its ability to "initiate the sexual cycle, to develop sex characters, and to reactivate the senile female organism." This was essentially the title of the publication in *Pflüger's Archiv* in 1925 (Steinach, Henlein, and Wiesner) which described the experiments on female rats, using an extract derived from ovaries and placenta. This preparation could be standardized in units and tested according to the vaginal smear method of American physiologists (Allen and Doisy, Long and Evans, Papanicolaou, *et al.*). It laid the foundation for future work culminating in the production of "Progynon."

After Steinach had succeeded with his preliminary work on the female (now known as "follicular") hormone in attracting the attention of the chemical industry, the work of purification and concentration made rapid progress. It was the Schering-Kahlbaum Corporation in Berlin (Professor Schoeller) that lent help through their own research laboratories, and the result was "Steinach's Progynon," the first purified highly potent and standardized female sex hormone that was put at the disposal of the medical profession. Today's "Progynon," so popular in the United States, is a partly synthetic product. In this respect it differs from the original preparation, but it owes its existence nevertheless to Steinach's original research work, a fact now rarely remembered.

The years 1925 to 1928 were devoted principally to the study and elaboration of the effects of the follicular hormone. If it was injected into infantile males an inhibition of the development of their sex characters took place, while their retrodevelopment was observed in adults. These experiments, published in collaboration with H. Kun, confirmed once more the "Antagonistic Effects of the Gonadal Hormones" (*Biologia Generalis,* 1926).

In 1928 two publications appeared in *Pflüger's Archiv* on the female hormone. The first, written in collaboration with Dohrn, Schoeller, *et al.,* was entitled "Bio-

logical Action of the Female Sex Hormone" and described its influence on the mammary gland (its growth and milk secretion) and on the uterus. The second, with Kun and Hohlweg as coauthors, dealt with the "Reactivation of the Senile Ovary and of the Female Organism through Hormonal Influence." Here the hormone's favorable effect on hair growth and red blood cells was recorded and also the restoration of fertility. As an additional aid for the reactivation of women, Steinach somewhat later advocated the use of diathermy to the ovaries to produce hyperemia. With simultaneous injections of the hormone, substitution and stimulation therapy were combined.

The Pituitary and the Central Nervous System. In the late 1920's the pituitary gland began to assume some of the importance that later researches so fully substantiated. Steinach's interest was readily aroused and led to important studies and conclusions. In 1928 he published a paper (*Mediz. Klinik*) with one of his favorate collaborators, H. Kun, on "The Significance of the Hypophysis as Activator of Gonadal Secretions." With the then crude extracts from the anterior pituitary lobe, the authors induced precocious maturation in infantile male rats. They explained it by the activation of the Leydig cells which were said to increase in numbers. In eunuchoid and senile animals they effected a restoration of atrophic sex characters and concluded that the pituitary was chiefly responsible for the "mechanism of development" and for a "biological law of puberty."

Deviating briefly from glandular studies, Steinach and Kun conducted experiments and published them in 1929 and 1930 (*Mediz. Klinik*) which revealed the presence of a specific *Reizstoff* (excitant) in the central nervous system. This so-called "centronervin" increased the excitability in frogs and the fly-catching ability of tree toads. The authors suggested its possible therapeutic use by employing brain and spinal cord tissues.

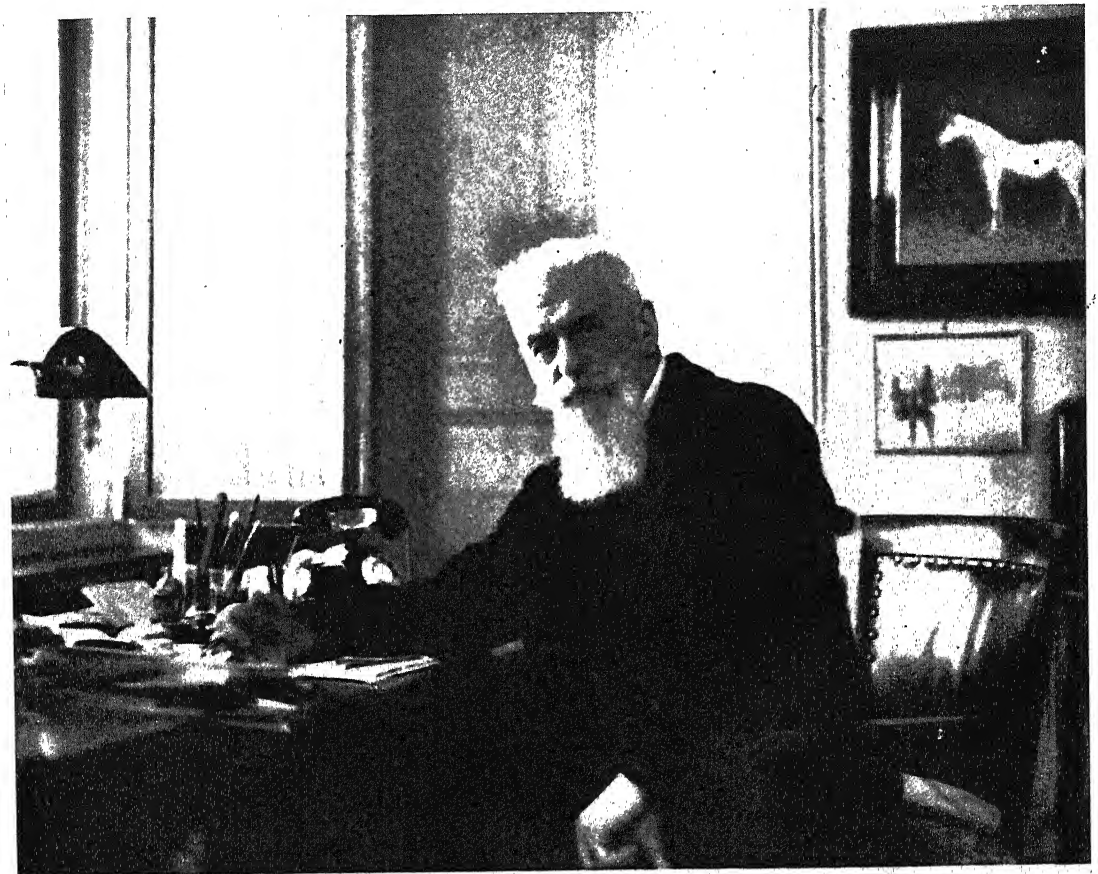
In 1931 Steinach returned to the studies of the ovary, but this time experimenting with the corpus luteum. He found two luteal hormones—one, sex-specific, affecting the uterus; the other of masculine characteristics capable of influencing secondary sex characters just like the male hormone. He spoke, therefore, of the "hormonic bisexuality" of the ovary (*Pflüger's Archiv*).

The Male Hormone. In the meantime male sex hormone preparations had been developed: first, androsterone from urine (Funk and Harrow, Laqueur, etc.) and then testosterone from testicles (Koch, Butenandt, Ruzicka). Steinach tested the latter, stressing the effect on psychic functions (erotization) in addition to the known somatic influences (*Anz. Acad. Wissenschaften,* 1933).

The following year saw Steinach in Switzerland collaborating with the veterinarians Staeheli and Grueter in the "Correction of Sterility in Cattle and Hogs" (*Wien. Klin. Woch.,* 1934). (The grandfather's cattle-breeding had planted the seed for this new interest.) One or two injections of "Provetan" (which was the "Progynon" of veterinary medicine) initiated oestrus in sterile cattle. These original experiments received wide attention, confirmation, and elaboration in veterinary circles.

Several summarizing publications on the "Analysis of Sex Hormone Effects" (Steinach, Kun, and Peczenik, *Wien. Klin. Woch.,* 1936) and on the "History of the Male Hormone" followed. In another series of articles (*Wien. Klin. Woch.*) the hyperemia produced by the sex hormones was especially emphasized. This hyperemia, Steinach thought, was the mechanism through which the results on body and mind must be explained. These studies readily led to the experiments on hypertonia in the same year (*Wien. Klin. Woch.*). A suitable combination of male and female hormone was used for the treatment of hypertension. Elaborate and interesting charts showed the reduction of blood pressure in a number of cases.

Likewise, in 1936, Steinach, Kun, and Peczenik published "Diagnostic Test for Disturbances of Male Sexual Functions Due to Endocrine Causes and Its Clinical Application" (*Wien. Klin. Woch.*). The creatine content of the urine after a test meal was used to differentiate impotence of an endo-



crine nature from purely psychic impotence. The former would be indicated by an increase of creatine output. The reactivating effects with the synthetic testosterone on men in various age groups were also recorded.

Except for a few minor articles (*Lancet,* 1937 and 1940) dealing with the male sex hormone, only two other publications by Steinach deserve to be recorded, because they appeared exclusively in the United States. One is an article in the *Medical Record* (New York), 1927, "Biological Methods against the Process of Old Age," a summarization of laboratory and clinical data, and the other is his book *Sex and Life* (*Forty Years of Biological and Medical Experiments*), which the Viking Press published in 1940. It is a somewhat autobiographical record of his life's work.

Last Years in Exile

Steinach's scientific work ended with the end of an independent Austria. When Hitler marched into Vienna (how easy it is to speak

of it now after VE-Day), Steinach and his wife happened to be in Switzerland. They never returned to their home. At 77, Steinach was an exile, like Freud and so many others. In Zurich, his new home, he bore up bravely under his personal tragedy, which was deepened by his wife's death. Fortunately, he had no economic problems, and he still had his indestructible zest for life, his science, his hobbies. His philosophy of life allowed him to make the least of what he had lost and the most of what he retained.

The Swiss Government had granted him permanent residence, but his enforced idleness was a heavy burden. He suffered under personal and scientific loneliness. His fertile mind with his ever-youthful curiosity was constantly filled with new ideas for experiments, and it was not surprising, therefore, that an old wish to visit America became a new and ardent hope. In America there would be a chance to start research work once more. Many efforts were made through universities, research institutions, and private organizations to have him invited to the United States. Promises were given but not kept. Various excuses were found for not opening the doors to him. Some said he might be too old; others said the physiologist had nothing to offer anymore. "If Steinach would be a biochemist, we would go to any length to bring him here" was one statement. In another instance fear was expressed that too much publicity would result from Steinach's arrival. "You know the American Medical Association. They would not like it," was another statement. And so Steinach never came.

It was his last disappointment. His powerful vitality, which he retained to the last (for he fought senility in himself as he had fought it in others) finally succumbed to stronger forces. A lonely, uprooted, and somewhat embittered man, Steinach died on May 14, 1944, in his eighty-fourth year, during a sojourn in Territet near Montreux. His cremation in Zurich on May 17 was the occasion for impressive services when leading Swiss scientists honored the memory and work of their great colleague.

Like every true pioneer, Steinach had relatively few immediate followers for the simple reason that he was ahead of his time. It matters little that some of his theories have not been verified and some of his experiments were not fully confirmed. Neither were Darwin's, nor Virchow's, nor Freud's. Steinach has nevertheless shown new ways and created new foundations on which subsequent researches were built, although this fact is often forgotten, especially in this country, conveniently by some, carelessly by others.

Professor Heinrich Meng, of the University of Basel, in his recent fascinating book *Psyche und Hormon* emphasizes and conclusively shows the fundamental importance of Steinach's lifework for all sex hormone research and therapy of today.

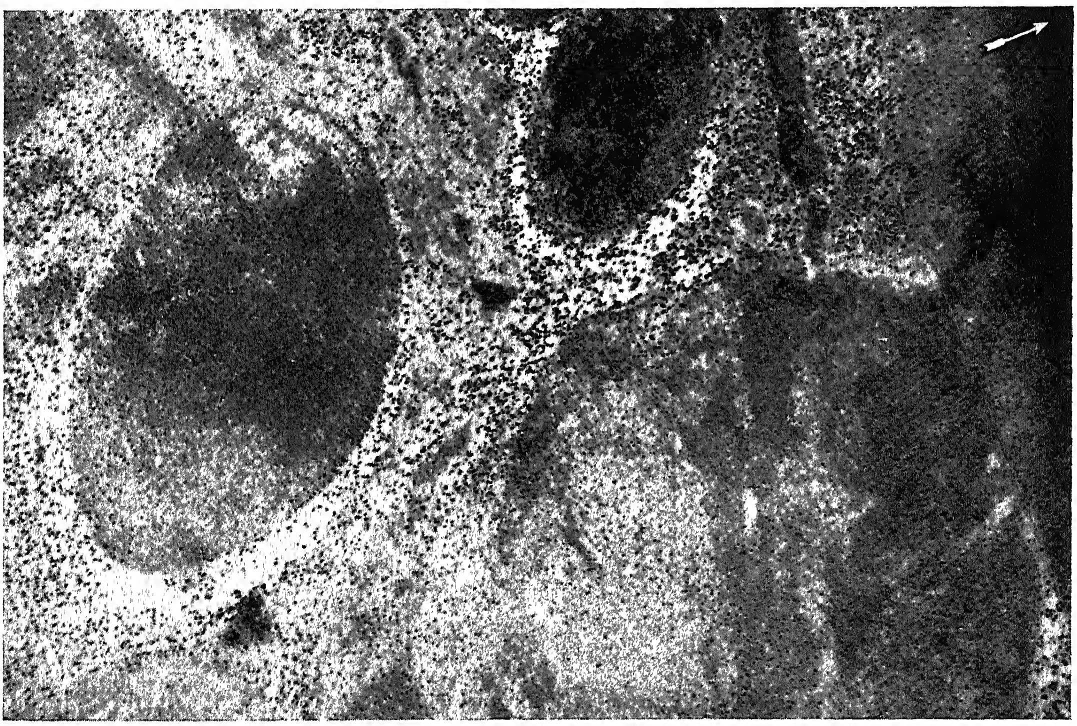
When Steinach approached the "dangerous" problem of sex physiology, all the sex taboos and prejudices of his day were arrayed against him. Many cried "heresy," just as they did in the times of Copernicus and Galileo, of Darwin, Haeckel, and Freud. Man has forever fought against the recognition of his own status in nature, and "original sin" has blighted for centuries the life of the individual. Steinach's work began in the shadow of the Victorian era, but he himself (to speak in psychoanalytic terms) had conquered the castration complex and was able to approach sexological problems with the fortitude and objectivity of a true scientist. "He found a road, because he dared to walk it."

A BIOLOGICAL EXPLANATION OF THE CAROLINA BAYS

By CHAPMAN GRANT

THE coastal plain of the Carolinas is smooth and tilts very slightly southeast toward the sea. It is covered with remarkable scars which are elliptical and parallel. These scars are shallow depressions from a few yards to over a mile in length and can best be visualized by referring to the accompanying illustrations.

Scientists, geologists for the most part, have been endeavoring to explain the origin utilize volcanism, wind, ocean currents, wind-caused currents, lakes, sink holes, drowned bays, raised pitted sea bottom, segmented lagoons, gyroscopic effect of rotating water or a combination of several of these for their explanation. The latter group base their explanation on bombardment by meteorites or the head of a comet. Not one or any combination of these theories satisfactorily explains the shape or the parallelism of the



BAYS NEAR MYRTLE BEACH, S. C.[1]

ARROW POINTS NORTH. *Left:* TYPICAL PERFECT BED ½ MILE LONG. NOTE GREATER SAND RIDGE ALONG EASTERN SIDE AND SHARPER CURVE OF NORTHEASTERN QUADRANT. *Below, right:* TYPICAL LARGE INDEFINITELY SHAPED BED.

of these scars which we will call by their vernacular name "bays." Two types of explanations have been used: the terrestrial and the extraterrestrial. The former variously bays. Of them all, the most easily disproved are those attributing the origin of the bays to volcanism or meteorites.

In the following pages we disclose a third

[1] All illustrations of this article were reproduced from Johnson's *The Origin of the Carolina Bays,* by permission of Columbia University Press; i.e., his figures 10, 27, 29, and 34, respectively.

type of theory, which has never been advanced and which seems to explain all the peculiarities existent in the bays.

Before launching into our hypothesis it is necessary to take the reader back to the not very distant past, geologically speaking, and visualize the kind of territory with which we are dealing.

The Coastal Plain was barely submerged, so that at high tide there was very shallow water over it. We will henceforth refer to the submerged plain as the "shallows." The tides rose and fell in a northwest and southeast direction. There was a dominant wind which blew from the northwest over the northern half of the area and a wind from a slightly more northerly direction blew over the southern half. The divergence of directions of the wind was due to the configuration of the Appalachians. The surface soil was as it is now found, but what is not seen today, or then either, is the subterranean layer of impervious material under which is an open or sandy water-bearing stratum originating at a higher level and possessing considerable hydrostatic pressure at sea level. The shallows received some surface runoff and in addition it had numerous artesian springs welling up through holes in the impervious layer. These springs were of various sizes, but not necessarily large.

To summarize: we had a fairly smooth, very gently sloping shallows covered by a very thin sheet of sea water gently flowing and ebbing in a northwest–southeast direction. There were many artesian springs of fresh water welling up into this sheet of sea water. There was a dominant northwest wind. Nothing to sorely tax the imagination. The waters adjacent to Yucatan present similar characteristics today.

Let us now look offshore at the fish that were there. There were several species and subspecies of vigorous fish which reproduced similarly to the present-day salmon. There were great shoals of these fish, but we do not have to visualize greater numbers than the present-day menhaden or salmon. In lieu of ascending rivers, these fish sought brackish water on the shallows in which to spawn. Some needed fresher water and were tied to the location of springs; others seem to have been satisfied with the water at any part of the shallows. These latter might have been to the former as the present-day trout is to the salmon. Some needed a greater depth of water for spawning beds than was to be found over the shallows in the vicinity of the springs. The fish that congregated over the northern half spawned in elliptical shoals whereas their southern congeners favored oval-shaped schools. The species which were more tolerant to salt water have left no clue as to what regular shape, if any, their spawning schools assumed. It is possible that some species were dependent on a certain amount of sand for spawning. This sand was for the most part mixed with the surface soil, having been brought up by the artesian springs.

The stage is now set for our biological spawning-bed hypothesis.

HEREAFTER we will call the bays "beds," but when quoting Douglas Johnson[2] we will of course use his term, "crater."

Let us inspect the preparation of a spawning bed by a large shoal of fish that needed a constant supply of spring water.

At the proper season and with the proper tide, the fish made their way through the shallow water until they found upwelling fresh water. Here the shoal halted, and if their agglomeration had been broken in their travel, they reformed into a compact elliptical mass with its head near the spring and its longitudinal axis in the direction of the flow of brackish water. Now they started to excavate a bed deep enough for the successful hatching of the eggs and for the propagation of the fry. Possibly they had to have sand to prevent the eggs from becoming smothered in mud or silt.

The excavating was done either by mouth or fins or both, as is done by various species of fish today. The fish worked close together so that the agitation of their vigorous tails would join to produce a current sufficiently strong to carry off the silt and mud. This work was aided by the flow of the tides. The fine material was carried away, but some of the sand settled at the sides and rear of the

[2] JOHNSON, DOUGLAS. *The Origin of the Carolina Bays.* Columbia University Press, 1942.

excavation where the current made by the fish slacked off. Much of the sand remained at the bottom of the bed to receive the eggs. Thus tons of silt could be moved as by a conveyor belt, or the work might better be compared to a bucket brigade.

The fish would have exactly faced the current of the tides but for one reason: the water was so shallow and the prevailing wind so strong that the resulting waves forced the fish to face into the wind. The ebb of the tide was slightly east of the direction of the wind, and hence we find more sand piled at the southeast quadrant than at the rear of the ellipses. When the excavation had reached the proper depth and there was a suitable layer of sand, if needed, spawning commenced. Parenthetically the amount of energy dissipated by the present-day salmon in ascending a stream would excavate as much or more than we have to account for in the beds. Now whether the adults remained over the nest to fan their eggs or whether they returned to sea or whether they died at or near their beds is indiscernible and immaterial. Whether the bed was used on the succeeding year by the same adults or later on by the young that had been hatched there is also immaterial. Suffice it to say that whatever school used the same bed the next time, they occasionally encountered a wind from a slightly different direction. The head of the school had to take up the identical position that was occupied before because of the position of the spring. The prolongation of the school had to be parallel to the direction of the wind, like a blimp at a mooring mast. For this reason we find superimposed beds with axes showing some deviation from one another. When this change of direction did not occur and the axes of successive occupations were parallel, we frequently find concentric rings at the offshore end of the beds. The rings are explained if the successive schools varied in size from year to year.

It appears that there was another species of fish that formed schools of a smaller size and needed less fresh water or possibly they were content with the water as found over the shallows. These schools sometimes started excavating near an occupied bed and even cut into them. Since all cutting was forward, or northwest, it appeared that the trespassing school would cut into the rear quadrant of the school to its front. When the original school was thus invaded, its individuals, seeing fish of another species or at least of a different school approaching from their rear, gave way. The infringing school retained its normal shape, but the rear of the forward school was dented, so to speak. In the few cases where the reverse is the case the explanation is that the bed trespassed upon was not in use at the time.

Beds were abandoned for various reasons such as failure or insufficiency of a spring, breaking up of a school at sea, destruction of spawn by invasion of mud from an adjoining bed, insufficiency of sand or change in tidal currents due to the damming effects of other beds or changes in climatic conditions.

Study of the aerial photographs shows that only a few of the enormous number of beds were used in one season as attested by their varying degree of obsolescence. The dim, imperfectly shaped depressions were beds of a different and probably smaller species that did not need the depth or the elaborate preparation required by the species of which we have been writing. Another possibility is that these dim beds were feeding grounds where possibly a different class of fish dug for its food. Also many of them may be the remnants of ancient beds disfigured by adjacent excavation or partially obliterated by erosion.

Johnson has made a more thorough study of the bays than anyone else. He has made a list of 29 features of the "craters," as he terms them, which must be explained to adequately account for their origin. At the risk of repetition we will take up his list in the order in which he has made it and answer it item by item.

1. Many of the craters are of remarkably perfect oval outline.

This is one of the points that made me believe that the origin of the beds was biological instead of geological. Flocks of many species of birds and schools of fish assume formations peculiar to the species. Here we have a modified case wherein the school

crowds together for the greater force given to the current. The elliptical form is for the purpose of continuing the current and of taking advantage of the current already put underway by the forward elements. The width of the ellipse is limited by the ability of the fish to tolerate salt water, the salinity increasing outward from the spring. The fish packed close together for mechanical reasons, for reproduction, for safety, and because of herd instinct. Any aggregation of animals moved by a common purpose and possessing a herd instinct would theoretically form a circle, all headed in the same direction, of course, but actually slightly longer than wide due to the shape of the individuals forming the mass. In the present case we have the individuals facing the same way because of current and wave, limited in width by the diffusion of fresh water, and packing close together for better efficiency in excavating and because of herd instinct. These components result in a nearly perfect ellipse.

2. In some localities the ovals are prevailingly elliptical, in others distinctly ovoid or egg-shaped.

Again a feature that cannot be explained by geological origin. The fact that the southern area has the ovoid-shaped beds and the northern ones the elliptical, with a few scattered ones intergrading and trespassing somewhat within the other area, is very easy to explain by postulating two subspecies that form schools of different shapes, each occupying its own area with some intergradation as is always found in subspecies. The southern species may have found greater efficiency in funneling or narrowing the excavation toward the rear, but otherwise reacting to the same stimuli as the northern schools.

3: Many craters are highly irregular in outline.

This is easy to explain by a biological origin. There are several types of irregular beds. Some are obviously very ancient beds that have lost their perfect form to a great extent by erosion and defacement by adjacent beds. Another type was obviously formed originally with an irregular outline. I attribute these to the same origin, namely spawning grounds, but made by a different species of fish which apparently did not need a spring of fresh water, but were satisfied with the brackishness of the entire area. They could therefore select any place without reference to sand, springs, or any feature except possibly depth of water. They apparently excavated to some extent and the following school selected the same general locality and did a little more haphazard excavating.

4. All gradations in form exist, from craters of the most regular outline to those highly irregular.

True in a sense only. They can all be classified in two categories; those that were originally regular and those that were originally irregular. The intergrades are always due to infringement, age, or other physical reason.

5. Oval craters have axial trends almost always directed between south and east, most of them ranging between S. 10° E. and S. 55° E.

In this statement Johnson grouped all the beds of the north and south areas together. The figure on page 447 shows two distinct groups of directions for the north and south groups respectively. See next answer.

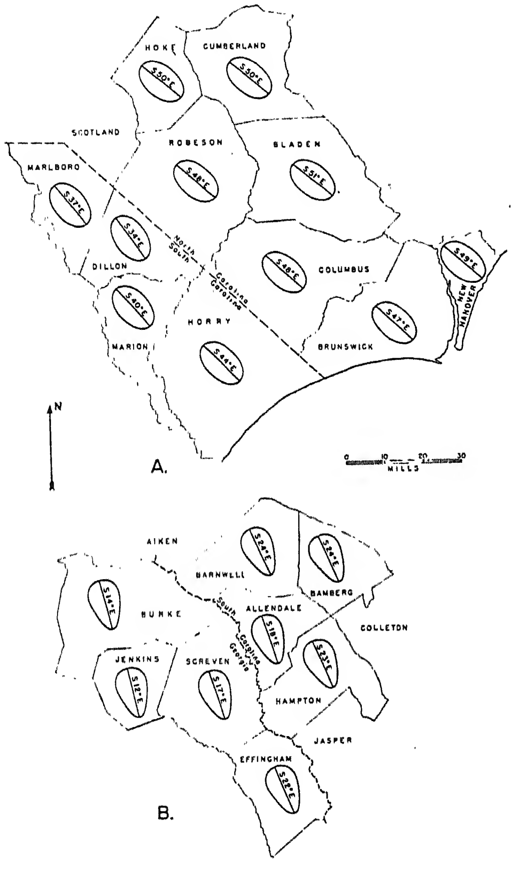
6. There are wide departures from the prevailing direction; elliptical craters are the most consistent in trend, with axes directed more or less nearly southeast; ovoid craters are most variable in trend, but as a rule have axial directions more nearly southward, their narrow ends pointing in this direction.

Again I refer the reader to the above-mentioned figure. Since my hypothesis is based principally on wind or tide direction and the two groups of directions are geographical, I believe that the explanation is that the dominant wind was from a slightly different direction in the two areas due to the configuration of the Appalachians. Also a possible different direction of flow of the tides might add an element to the result. The present general slope of the entire region may be as it was then or it may have warped. Thus the present slope and the present wind may differ from what it was.

7. Craters vary in size: some are a few hundred feet, many one or two miles, and some three or four miles in longest diameter.

Schools of fish vary in size. No stretch of the imagination is necessary to visualize schools larger than those we have today. However, I believe that several species of fish are represented by the various types of beds.

Johnson may not have thought the following problem of import, but I add it here: Very large beds are not as wide proportionately as the smaller ones. The reason is that the volume of fresh water was insufficient to spread over a wide area. Also very large schools of fish tend to become proportionately longer than smaller schools.



BAYS, NORTH AND SOUTH

SHOWING DIFFERENT AXIAL TRENDS AND DIFFERENT SHAPES OF BEDS IN THE NORTHERN (A) AND THE SOUTHERN (B) PARTS OF THE "BAY" AREA.

8. The craters are remarkably shallow in comparison with their great areal extent.

If the beds were deep, my hypothesis would lose much strength as the parallelism of the beds would depend solely upon the direction of tide flow since disturbing wave action would not be felt at any considerable depth. As it is, it is accounted for by the fact that the fish did need greater depth than existed over the shallows, but they did not excavate more than necessary. When the optimum depth was reached, provided a sufficient proportion of sand had beeen exposed, the excavation stopped and spawning commenced.

9. The craters descend below the level of the surrounding plain and below the bases of their bordering rims, as if they were depressions caused by removal of the Coastal Plain material.

Certainly. The excavation was done by the concerted efforts of the school aided somewhat by the flow of the tide. The tide flowed over the top of the rim.

10. Both large and small oval craters show considerable variation in degree of ellipticity.

True, but not more than would be expected in a fluid school that would be influenced by the vagaries of tide, wind, encroachment of other schools, texture of soil or any combination of these causes. Note that the smaller the bed the more nearly perfect it is.

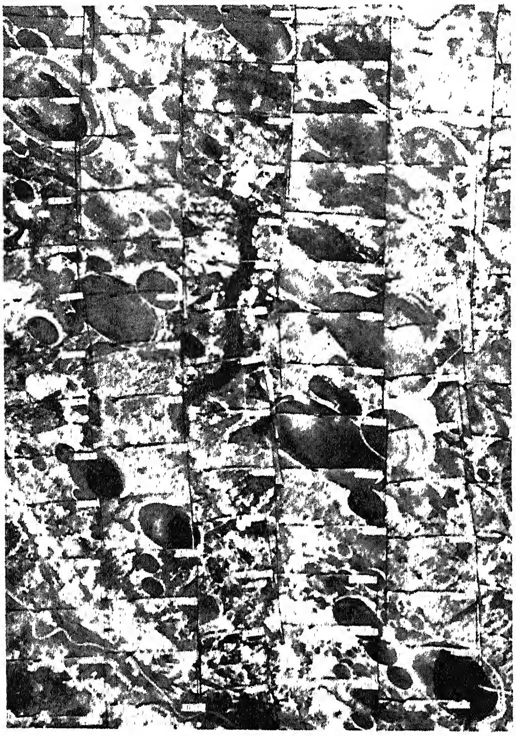
11. Oval craters normally are not true ellipses or true ovoid forms but show systematic bilateral asymmetry, the northeastern sides usually being more sharply curved than the southwestern.

The rising tide from the east was hampered by a quartering head wind from the northwest; the tide ebbing toward the east was accelerated by a following quartering wind. The net result was to carry more of the fresh spring water, that had a tendency to rise, to the northeast side of the long axis of the ellipse than in the opposite direction. Wherever there was water of proper density the fish would excavate provided the eccentricity did not disrupt their close-packed formation.

12. Craters sometimes occur in systematic groups, with a distinct group pattern apparently determined by some pre-existing topography of structure.

The schools selected sites where the conditions were most favorable. Water depth, springs, and sand were the most important

factors, and all three were very likely to occur in close association with topographical or structural factors. This question was included to confound those advocating a meteoritic hypothesis, because meteorites cannot select their striking ground.



PART OF BLADEN CO., N. C.
NORTH AT TOP. MOSAIC AERIAL PHOTOGRAPHS. NOTE PARALLELISM AND APPROXIMATE UNIFORMITY OF SHAPE OF BEDS. NOTE THAT BEDS ENCROACHING FROM THE REAR RETAIN THEIR PERFECT OUTLINE AT THE EXPENSE OF THE BED TRESPASSED UPON. NOTE MULTIPLE RIMS AROUND SOUTHEAST ENDS OF BEDS. NOTE MANY DIM, LONG-UNUSED BEDS WHICH HAD BEEN LOSING THEIR OUTLINES AT THE TIME OF EMERGENCE. BED IN SOUTHEAST CORNER 2¼ MILES LONG.

13. Oval craters, while abundant on parts of the Coastal Plain, are apparently absent from the Piedmont and other Appalachian provinces.

Another poser for the meteorite hypothesis, but hardly calling for a biologist's reply.

14. Many craters are bordered by rims of sand; but . . .

Large rims of sand are doubtless due to the fact that the area excavated contained a large proportion of sand and in order to attain sufficient depth, some sand had to be expelled from the bed.

15. Many craters similar in all other respects have no rims associated with them.

Where no sand rim occurs, it is evident that the low sand content at the site of excavation was only sufficient for nesting purposes. (Note: The irregular craters never have sand rims. I believe that the species excavating them was more tolerant to mud and did not need to be near a spring and could therefore select a location where the water was deeper and needed less excavation and hence no excess sand was exposed.)

16. The rims, when present, rarely completely surround a crater; incomplete rims are sometimes erratically distributed but normally are highest and broadest about the southeastern quadrants of the depression.

The rising tide flowing west was impeded by a quartering head wind from the northwest; the falling tide flowing east was accelerated by a following quartering wind. Therefore the eastward falling tide had more eroding effect than the rising tide. Thus sand brought into the bed from the southwestern rim would have a tendency to be picked up by the fish and passed back or laterally. Sand carried over the northeastern rim was there to stay. The weak rising tide carried less sand over the southwest rim. Erratic distribution of the rims was due to different proportions of sand at different spots of the surface which was being excavated.

17. Multiple rims, nearly but not quite concentric, and from two or three up to six or eight and possibly more in number, occur about some craters.

This shows use of the same bed at successive times, probably by the fish that hatched there, after an interval of several years.

18. Such multiple rims tend to be best developed and farthest apart toward the southeastern ends of oval or ovoid craters, converging or merging or disappearing toward the northwestern ends. In some cases the multiple rims are chiefly confined to the eastern sides rather than the southeastern ends of craters.

The upwelling of artesian water fixed the anterior focus of the bed. A slight deviation

of wind direction or current and especially a difference in size of the school accounts for the multiple rims and their divergence. Probably concentric rings were not built at a single occupation of a bed. A current with an easterly set must account for the occasional occurrence of sand predominating on the eastern rim.

19. A wide space sometimes intervenes between an outer rim or series of rims and an inner rim or series within the same crater; this distribution pattern may be repeated in adjacent craters.

Showing a decrease in the size of the successive schools, owing to lack of food or other adverse conditions. The fact that adjacent beds had similar modifications shows that they were occupied at the same period and that the causes were widespread.

20. The rims are of relatively insignificant size, the volume of material contained in them being but a small fraction of the material removed from the craters.

Showing that most of the spoil was fine material which was carried away by the tide and the current caused by the fish. It also shows that there was not a large proportion of sand in the excavation.

21. The convergence of multiple rims of the same crater and the junction of contiguous rims of adjacent craters do not give rise to combined rims of unusually large size. On the contrary, such combined rims may be unusually small and may locally disappear.

This is a double problem. First, convergence of multiple rims of one bed not showing added bulk: The convergence is always at the forward end where less total excavation is done and hence less excess sand is found. Sand was passed along with mud to some extent from fish to fish, and the excess tended to move rearward more than laterally. Second, junction of contiguous rims not adding bulk, but occasionally being of less bulk or disappearing. Johnson did not record the fact that where one bed irrupts into another it is almost always the rear bed which retains its perfect form and the forward bed which is deformed. This is accounted for by the fact that a school opening up its bed worked forward to some extent and probably also backwards. Thus beds infringed at times. When two schools found themselves together, the rear fish of the front school, seeing strange fish approaching from their rear, gave way. The trespassing school retained its normal form, but the advanced school shrank away sufficiently to leave a boundary of comparatively still water where the sand that was passed back settled. This outlined their warped bed and left a perfect outline around the rear bed. The sand that had been accumulated at that sector by the advanced school was appropriated by the trespassing school and passed back along with sand excavated in their own bed.

22. There is no systematic relation between the size of the bays and the size of their bordering rims. Large bays may have small rims or none, and small bays may have large rims.

Again a result of differential sand contents in various localities. There is also a possibility that different species might have achieved different results due to differing necessities.

23. Many rims are relatively flat-topped or broadly rounded in cross section, frequently with steeper slopes inward toward the crater and outward toward the adjacent plain.

COMPLEX BAY, LEE CO., S. C.
WHITE LINES INDICATE DIVERGENT AXES OF OLDER AND NEWER BEDS. ASCRIBED TO CHANGE IN DOMINANT WIND DIRECTION BETWEEN SEASONS OF OCCUPANCY.

The flat top doubtless represents the water surface at low tide. In cases where the wind built the sand ridges higher than the tide

it is possible that a bed would have to be abandoned because of lack of circulation.

24. The composition of the rims is remarkably uniform, the material consisting for the most part of clean, fairly coarse white or buff quartz sand.

Apparently the sand came up from a lower stratum by action of the artesian springs as Johnson has pointed out.

25. Material composing the Coastal Plain in which the craters are excavated is often strikingly dissimilar to the material composing the rims.

The converse of the last question.

26. Bedrock fragments of Coastal Plain sediments are not found in the rims.

Another question to refute the theory of meteoritic origin. Obviously fish could not bring up fragments of bedrock.

27. Coastal Plain sediments normally remain undisturbed in a horizontal position below the sandy rims and in the crater walls. Occasionally there is slight indication of slumping inward toward the craters but no uplifting of beds causing them to dip outward away from the craters.

Another question to refute the meteoricists. Their hypothesis demands an upwarping. The slumping inwards shows to what extent the artesian springs excavated material from lower levels.

28. Many craters have channels draining into or out of them.

A study of Johnson's photographs makes it appear that these channels developed after the shallows had become the Coastal Plain. Hence they do not enter into our problem.

29. These channels frequently cut across rims bordering the craters.

As the shallows became exposed the surface water found natural channels. Where this flow was obstructed by a sand ridge the water seeped through and excavated from the down stream side, finally gnawing its way back through the rim. I have seen this occur where an ebbing tide cuts through a sand spit which long shore currents had built on the flowing tide. There is no evidence that the rims were cut while in use.

Johnson concluded his list by saying: "Such is the array of facts which must receive reasonable explanation before any hypothesis of crater origin can receive general acceptance."

I hope that I have given a reasonable explanation of his 29 facts. I have added two more facts with their explanations under Nos. 7 and 21.

My hypothesis rests upon the vulnerable assumption that all the beds of typical shape used for spawning were relatively shallow and that any deep ones are due to subsequent cave-ins or sinks due to solution or subterranean erosion. Any deep spots in a bed must be accounted for in the same way. The existence of any considerable depth of water that would remove the effect of wave action, which kept the fish facing the wind, would seriously impair my hypothesis.

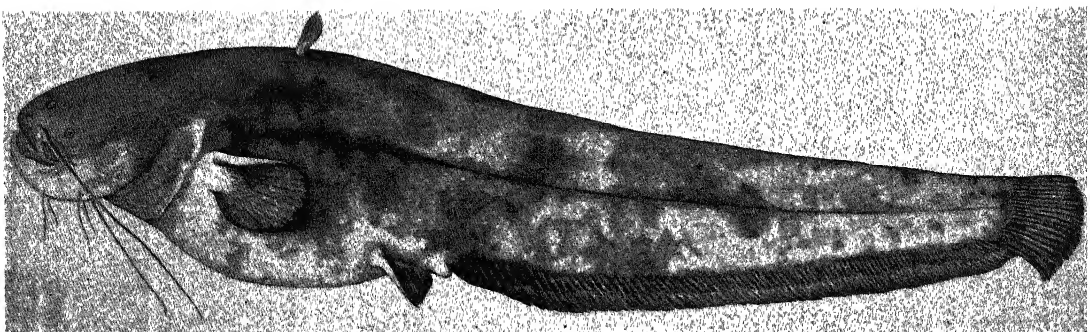
This hypothesis is so divergent from any so far advanced that geologists at least will hear it with skepticism. Ichthyologists may be more lenient.

IS THE GIANT CATFISH, *SILURUS GLANIS*, A PREDATOR ON MAN?

By E. W. GUDGER
AMERICAN MUSEUM OF NATURAL HISTORY

SHARKS are flesh-feeders and have large mouths and gullets. Indeed, in most sharks mouth, gullet, and stomach form a continuous tube of approximately the same caliber to make easy the ingestion of large masses of food. If the prey is of moderate size in proportion to the size of this food tube, it is ingested whole. If it is too large for this, then it is cut into suitably sized fragments by the large, triangular, saw-edged teeth, or, in sharks having long, pointed, conical teeth, the prey is torn into fragments of a size available for ingestion.

Teleostean (bony) fishes do not ordinarily chop or tear up their prey for ingestion, but generally they swallow smaller fishes whole. Occasionally, however, a bass, pike, trout, or some other of the fiercer forms will try to swallow a fellow fish too large for him and will be choked to death by his prey.



After Holm, 1779

FIG. 1. THE SHEATFISH, *SILURUS GLANIS*, OF EUROPE

Catfishes, which grow to large size, have huge heads and correspondingly large mouths, gullets, and stomachs. These fishes are voracious and indiscriminate feeders and can swallow large prey.

I have long known that *Silurus glanis*, the catfish of the Danube and other large rivers of eastern and northern Europe, grows to a large size. And I have read that it is very voracious and indiscriminate in its feeding, even ingesting human remains, particularly of small children. Thus D. S. Jordan (1905) says: "It [*Silurus glanis*] is very voracious, and many stories are told of the contents of its stomach. A small child swallowed whole is recorded from Thorn, and there are still more remarkable stories, but not properly vouched for."

Recently I have gone extensively into the natural history of this great catfish and have gathered data not only on its size but also on its feeding habits and particularly on its alleged predilection for human flesh. Whether these data are reliable can only be discussed when they have been set out and studied, but certain it is that they arouse interest in the behavior of this predacious fish.

Silurus glanis has a number of colloquial names, which will be used herein. It is called Wels and Schaid by the Germans and German-Swiss, Sheatfish by the English, and Glanis by others.

Most large fishes have large mouths, and such fishes of necessity are apt to be both voracious and omnivorous and to feed on sizable prey. Few teleostean fishes have heads as large and mouths as wide in proportion to the size of their bodies as the siluroids. And so, in keeping with its great

bulk and wide mouth, the Glanis is extremely voracious and equally omnivorous.

Being a big fish, it normally feeds on other fishes. In the stomach of a relatively small Wels, Holm (1779) found 19 fishes—the largest a foot long. The Glanis feeds also on frogs, crustaceans, insects, and worms. In fact, almost anything is grist that comes to its mill. The omnivorous voracity of this fish is embodied in an old Bohemian proverb —"One fish is another's prey, but the Sheatfish eats them all."

Figure 1 is a copy of the oldest and best figure of the Glanis that has been found in this study. It is from an article on *Silurus glanis* by Theodore Holm (1779). Attention is called to the heavy, logy body and particularly to the wide mouth and large abdomen—structures that make possible the heavy feeding to be described later. The size of these parts is emphasized by F. A. Smitt (1895) when he states that the breadth of the head across the opercula is the greatest width of the body and that in old specimens it is about 14 percent (one-seventh) of the length of the body. Then of the mouth he says, "The great breadth of the gape, the width of which measured straight across the corners of the mouth when closed, is about four-fifths of the greatest breadth of the head, and occupies almost the entire breadth of the snout." These statements are all very helpful in getting an idea of the size of these organs, but more to our purpose, especially in view of what is to come, would be actual measurements of a large Glanis.

The indiscriminate feeding of the Wels is attested by various authors. Their accounts have (in part) been summarized by F. A. Smitt (1895, p. 701), who begins as follows: "From older writers we have accounts of assaults made by the Sheatfish on higher animals. Gesner tells us . . . that geese, ducks [evidently caught while swimming] and animals that were being watered [at a river or lake] have been seized." Smitt states also that "long ago" a man standing on the shore of a lake in Sweden saw a Wels seize a lamb which was drinking at the water's edge. Finally, there is the statement by Heckel and Kner (1858) that in the stomach of a large Wels, taken at Vienna, was a poodle dog.

But, according to the old natural historians, the Sheatfish did not content themselves with ducks and dogs. These men go further and allege that man and his offspring are also sometimes devoured by the Glanis. Bloch (1785), who cites his authorities by book and page, quotes Aldrovandi (1613) that near Pressburg in Hungary one of these large fish had swallowed a child, which was bathing in the river, and that, shortly afterward, this or another Sheatfish was caught and in its stomach were found some fragments of a child's body. Bloch states also that J. G. Richter (1754) alleges that near Limritz, in Pomerania, there was taken a great Schaid which had a mouth and gullet so large that it had swallowed a child of seven years. Cuvier and Valenciennes (1839), who likewise document their citations by author, book, and page, state that in 1700 a Wels was taken at Thorn on the Vistula River and that when opened an entire infant was found in its stomach.

Furthermore, according to some of these old naturalists, the Wels swallows not only young children but also adults. Thus Smitt quotes Gesner (1558) that in the stomach of a Sheatfish were found a human head and a right hand with two gold rings. Then Pallas (1831, p. 82) says that "in some Russian rivers . . . this very voracious fish does not fear to lay hold of the feet and legs of adult swimmers." And Cuvier and Valenciennes quote Ker. János Grossinger (1793) that in Hungary two young girls, who had gone to fetch water (from the river Danube?) were devoured by great catfish. Grossinger also stated that he had been told that on the frontiers of [European] Turkey a fisherman took a great Wels whose stomach contained the body of a woman, together with a gold ring and a purse full of money.

But the reader is asking, "How accurate are these reports and how much of them is to be believed?" Before attempting to answer, it will be well to recall some corroboratory statements about similar feeding habits of the huge catfishes of the Amazon and its tributaries.

These are contained in my article on "The Giant Freshwater Fishes of South America"

(1943). Therein I quoted Dr. J. D. Haseman, whose field studies of South American fresh-water fishes are notable. In the Guapore branch of the Mamore River a great catfish called Pirahyba was captured. Of it, Haseman wrote me: "The mouth was amply large enough for a small Brazilian to crawl through. Indeed it is claimed that the Pirahyba does swallow swimming natives, but I was unable to confirm this."

In the same article, in corroboration of Haseman, I quoted from Theodore Roosevelt's *Through the Brazilian Wilderness* (1914) as follows:

> The huge catfish which the men had caught . . . with the usual enormous head, out of all proportion to the body, and the enormous mouth, out of all proportion to the head. Such fish . . . swallow very large prey. This one contained the nearly digested remains of a monkey, and once engulfed in that yawning cavern there was no escape. . . . Our Brazilian friends told us that in the lower Madeira and in that part of the Amazon near its mouth, there is a still larger catfish which in similar fashion occasionally makes a prey of man.

Roosevelt then quoted the expedition's doctor that at the mouth of the Madeira he had seen a huge catfish measuring 9 feet long. The doctor reported that swimmers fear these great catfish more than they do the big crocodiles. Further, Colonel Rondon (Roosevelt's guide and mentor) told him that in the lower Madeira the villagers so fear the Piraiba (another spelling of Pirahyba) that they build stockades around their bathing places to prevent its attacks.

Let us now return to the Wels and its alleged human victims and attempt critically to evaluate the accounts quoted above. They begin with two of the founders of ichthyology, the great encyclopedists Gesner (1558) and Aldrovandi (1613). They continue with Richter (1754) and Pallas (1813). Some of these allegations were summarized by Bloch (1785), who quotes author, book, and page. Others are given by Cuvier and Valenciennes (1839), who also document their statements. It would seem that these men all believed in the truthfulness of the accounts, and apparently so did Heckel and Kner (1858) and Smitt (1895).

"Where there is so much smoke, there must be some fire." These very similar accounts range from 1558 to 1858 and 1895. They are made by reputable men, who either had direct knowledge of these matters or who had reason to give credence to the men quoted and to their accounts. And it should be noted that most of these men presumably did not know what the others had written. Considering matters thus, one cannot say "Preposterous!" and reject these accounts.

In my own observations of the behavior of fishes, I have seen them do many unusual and extraordinary things. And as a bibliographer of fish literature and a long-time reader of books of exploration and travel, I have learned so much of the unusual and unexpected things that fishes do that I am prepared for almost any behavior not physically impossible. This is certainly true of their feeding habits. A hungry fish will ingest almost anything that gives promise of sustenance. Certainly these great specimens of *Silurus glanis* have mouths and gullets large enough to swallow small children, and impelled by hunger they might seize and ingest such children playing in the water near shore. But, as suggested by Smitt (1895), they are much more likely to prey on corpses, particularly of children, which in some way —by drowning or some other accident—have gotten into rivers.

With regard to the alleged ingestion of adults, the situation and conclusions are very different. If some scientific man could dissect a really big Wels and measure its stomach, he could get some idea of what a big Glanis could swallow. But no one knows how large the stomach of a great Sheatfish is. Smitt (1895, p. 698) states that "the length of the abdominal cavity measures in young sheatfish about one-third and in older ones about one-fourth of that of the body." Figure 1 shows that the Wels has a large abdomen. But that abdomen contains not only Sheatfish about one-third and in older ones testine, a large-lobed liver, a gall bladder, a pancreas, a spleen, an air bladder, and finally the excretory and reproductive organs. Smitt's statement, taken from his description of the abdomen of a Wels, needs to be clarified by actual measurements. What we need are measurements, in figures, of the mouth (the gape, both right-left and ver-

tical), of the gullet, and then of a dissected stomach of a big Wels (say 10 feet long) and an estimate of its volumetric capacity.

But I do not believe that a 9- or 10-foot or even larger Glanis could swallow and contain in its stomach the body of a large boy, and certainly not that of a grown man or woman, as has been alleged. It would seem that such accounts might have originated in the finding in a fish's stomach of parts of a dismembered floating human body—a head, as in Gesner's account, or an arm or a leg. Furthermore, if the body had been in water long enough for decomposition and disintegration to have softened the tissues, it would be entirely possible for two or more of these great catfish to have seized arms and legs and to have brought about dismembering of the body and the swallowing of its parts.

So I have seen powerful 10- and 12-foot tiger sharks lay hold of the legs of a dead horse floating out with the tide just off the Mallory Line dock at Key West, Florida. Rearing and bucking, each trying vainly to tear a leg off, they drifted out of sight, carrying the horse along. Their attempts were in vain, because decomposition had not yet set in, and the hide and muscles and tendons were still intact. But indicative of what these powerful sharks can do when decomposition and disintegration have loosened hide, muscles, tendons, and cartilage, the following evidence may be adduced:

In the stomach of one big tiger shark at Key West, Florida, I found a horse's skull, with the soft tissues all digested, and the skull so softened by the hydrochloric acid of the shark's digestive juice that the bones could be almost crushed in the hand. From another stomach I extracted the dehorned skull of a cow with the flesh gone and the bones softened. In the stomach of a third I found the leg bones of a horse and separate from one of these a hoof with its iron shoe, which I now have in my office in the Museum. Both the heads and legs were torn from the animals' bodies when decomposition had made it possible for the sharks to dismember the bodies. When men's arms, legs, and even heads are found in the stomachs of large sharks, I strongly suspect that they were not bitten or torn from live or just-dead men but from the corpses of drowned men after disintegration of the tissues had taken place.

If a large Sheatfish could swallow a duck or a poodle dog, as alleged, I can see no physical reason why such a great fish could not have swallowed a small child. And if sharks can tear apart and swallow the objects I have obtained by dissection of their stomachs, I can see no reason to doubt that a large Wels, with or without the help of a fellow fish, could have dismembered the decomposing body of a drowned man, have swallowed a part of it, and thus have given rise to the allegations recounted above. But to prove the point one way or another we should have dissections of the stomachs of a considerable number of large sheatfish and careful examinations of their contents. In this way only can the matter be cleared up.

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CAUSATION, CHANCE, DETERMINISM, AND FREEDOM IN NATURE*

By PAUL CRISSMAN
PROFESSOR, DEPARTMENT OF PSYCHOLOGY AND PHILOSOPHY, UNIVERSITY OF WYOMING

EMPIRICAL science concerns itself with the beliefs and propositions that are verified by reference to "existential" objects and events. If a proposition be empirical, it must designate some property or invariant relation exemplified in some class or domain of things and events, or else imply other propositions that terminate in such reference. In empirical science, with which we are here alone concerned, three fundamental types of invariant relations are distinguishable: (1) *numerical equations;* (2) *certain comprehensive theories or laws;* and (3) *causal relations.*

(1) Many invariant relations are expressed in terms of numerical equations. The principle of the lever states that equilibrium is obtained when the two weights vary inversely as their distances from the fulcrum; within certain limits, the volume of a gas varies inversely as the pressure exerted upon it. Invariant relations of this type are independent of the temporal order of events and are not ordinarily regarded as causal in character. They merely assert that the measured elements stand to one another in a certain invariant relation.

(2) A second type of order in nature is exemplified in such comprehensive theories as the general theory of relativity and the kinetic theory of matter. As a rule, theories of this type are not directly verifiable; i.e., some or all of the events between which the invariant relation is asserted to hold are not directly observable. Thus the electrons, their motions and collisions, the energy of their several motions, and the like, cannot be observed.

The function of such comprehensive theories is to relate many numerical and other laws previously assumed to be isolated from, or independent of, one another. Thus both light and gravitational phenomena are now subsumable under general relativity theory; thus Kepler's laws of planetary motion and the acceleration of moving bodies within the earth's gravitational field are both derivable by logical means from the law of gravitation.

(3) A third fundamental type of order in nature is the causal. "The firing of a certain gun was the cause of the man's death" and "The movement of some icecap in the past was the cause of the present morainal lake on the mountain side" are instances of causal order. Causal order is assumed to involve a temporal sequence in the occurrence of events; indeed, it is commonly believed that it is their temporal order that enables us to distinguish which events are causes and which effects.

HISTORICALLY, three concepts of causality may be distinguished: (1) the *primitive,* or *anthropomorphic;* (2) the *empirical,* or *positivistic;* and (3) the *mathematical,* or *equational.*

(1) The primitive concept of causality is an interpretation of natural changes in terms analogous to those employed in human behavior. Thus, "X pushes his office chair and causes it to move" is supposed to express a causal relation, because there is an agent X who exerts a force against the chair and causes it to move. Similarly, the past icecap is said to be the cause of the morainal lake because there was an icecap which produced the depression in which the water formed by the melting snow and ice was impounded. To common sense, all changes require "causes" to explain them, and when found these causes are construed as agents producing the change.

The definitive characteristic of the primitive concept of causality is the idea of production. The origin of this idea would seem to be the consciousness in the individual of his power to produce effects, for we have the experience of exerting a force and thereby

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tical), of the gullet, and then of a dissected stomach of a big Wels (say 10 feet long) and an estimate of its volumetric capacity.

But I do not believe that a 9- or 10-foot or even larger Glanis could swallow and contain in its stomach the body of a large boy, and certainly not that of a grown man or woman, as has been alleged. It would seem that such accounts might have originated in the finding in a fish's stomach of parts of a dismembered floating human body—a head, as in Gesner's account, or an arm or a leg. Furthermore, if the body had been in water long enough for decomposition and disintegration to have softened the tissues, it would be entirely possible for two or more of these great catfish to have seized arms and legs and to have brought about dismembering of the body and the swallowing of its parts.

So I have seen powerful 10- and 12-foot tiger sharks lay hold of the legs of a dead horse floating out with the tide just off the Mallory Line dock at Key West, Florida. Rearing and bucking, each trying vainly to tear a leg off, they drifted out of sight, carrying the horse along. Their attempts were in vain, because decomposition had not yet set in, and the hide and muscles and tendons were still intact. But indicative of what these powerful sharks can do when decomposition and disintegration have loosened hide, muscles, tendons, and cartilage, the following evidence may be adduced:

In the stomach of one big tiger shark at Key West, Florida, I found a horse's skull, with the soft tissues all digested, and the skull so softened by the hydrochloric acid of the shark's digestive juice that the bones could be almost crushed in the hand. From another stomach I extracted the dehorned skull of a cow with the flesh gone and the bones softened. In the stomach of a third I found the leg bones of a horse and separate from one of these a hoof with its iron shoe, which I now have in my office in the Museum. Both the heads and legs were torn from the animals' bodies when decomposition had made it possible for the sharks to dismember the bodies. When men's arms, legs, and even heads are found in the stomachs of large sharks, I strongly suspect that they were not bitten or torn from live or just-dead men but from the corpses of drowned men after disintegration of the tissues had taken place.

If a large Sheatfish could swallow a duck or a poodle dog, as alleged, I can see no physical reason why such a great fish could not have swallowed a small child. And if sharks can tear apart and swallow the objects I have obtained by dissection of their stomachs, I can see no reason to doubt that a large Wels, with or without the help of a fellow fish, could have dismembered the decomposing body of a drowned man, have swallowed a part of it, and thus have given rise to the allegations recounted above. But to prove the point one way or another we should have dissections of the stomachs of a considerable number of large sheatfish and careful examinations of their contents. In this way only can the matter be cleared up.

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CAUSATION, CHANCE, DETERMINISM, AND FREEDOM IN NATURE*

By PAUL CRISSMAN

PROFESSOR, DEPARTMENT OF PSYCHOLOGY AND PHILOSOPHY, UNIVERSITY OF WYOMING

EMPIRICAL science concerns itself with the beliefs and propositions that are verified by reference to "existential" objects and events. If a proposition be empirical, it must designate some property or invariant relation exemplified in some class or domain of things and events, or else imply other propositions that terminate in such reference. In empirical science, with which we are here alone concerned, three fundamental types of invariant relations are distinguishable: (1) *numerical equations;* (2) *certain comprehensive theories or laws;* and (3) *causal relations.*

(1) Many invariant relations are expressed in terms of numerical equations. The principle of the lever states that equilibrium is obtained when the two weights vary inversely as their distances from the fulcrum; within certain limits, the volume of a gas varies inversely as the pressure exerted upon it. Invariant relations of this type are independent of the temporal order of events and are not ordinarily regarded as causal in character. They merely assert that the measured elements stand to one another in a certain invariant relation.

(2) A second type of order in nature is exemplified in such comprehensive theories as the general theory of relativity and the kinetic theory of matter. As a rule, theories of this type are not directly verifiable; i.e., some or all of the events between which the invariant relation is asserted to hold are not directly observable. Thus the electrons, their motions and collisions, the energy of their several motions, and the like, cannot be observed.

The function of such comprehensive theories is to relate many numerical and other laws previously assumed to be isolated from, or independent of, one another. Thus both light and gravitational phenomena are now subsumable under general relativity theory; thus Kepler's laws of planetary motion and the acceleration of moving bodies within the earth's gravitational field are both derivable by logical means from the law of gravitation.

(3) A third fundamental type of order in nature is the causal. "The firing of a certain gun was the cause of the man's death" and "The movement of some icecap in the past was the cause of the present morainal lake on the mountain side" are instances of causal order. Causal order is assumed to involve a temporal sequence in the occurrence of events; indeed, it is commonly believed that it is their temporal order that enables us to distinguish which events are causes and which effects.

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The definitive characteristic of the primitive concept of causality is the idea of production. The origin of this idea would seem to be the consciousness in the individual of his power to produce effects, for we have the experience of exerting a force and thereby

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producing an effect. The primitive concept implies *efficiency, creation, production*.

Moreover, the primitive concept of causation assumes contact between "agent" and "patient," between a cause and its effect. Yet many causes seem spatially to be separated from their effects. How, then, may such actions be explained?

In accounting for actions at a distance, three explanations have been proferred: (a) The causal action was once interpreted as pure action at a distance. A century or more ago, gravitational, electrical, and magnetic actions were in fact so described. However, the concept of pure action at a distance is now wholly abandoned. (b) Distant action has also been construed as continuous action after transport. Thus a projectile fired from a gun traverses the intervening space and sinks a ship five miles away. (c) The favorite interpretation of action at a distance has been to explain it as action through a continuous medium. Thus ether waves within the range of 365 to 800 millionths of a millimeter in length originating in a distant star traverse the intervening ether and at some later time produce an electrochemical change in the retina of an observer here on the earth.

(2) Unfortunately, it is extremely difficult, if not indeed impossible, to make clear what is meant by causes "producing" their effects. Upon reflection, all we seem to observe in all the alleged instances of causation is some invariant relation of occurrence between two or more changes or events. Hence it is the *invariance in occurrence* that we identify with the "empirical," or "positivistic," concept of causation. Moreover, the empirical definition of causation is logically simpler than, and hence for purposes of systematic science preferable to, the primitive concept in at least three respects: (a) it frees causation of all anthropomorphic elements; (b) causal relations need not be restricted to mechanical action between cause and effect by means of direct contact or through some connecting medium postulated for the purpose; and (c) temporal order or asymmetry ceases to be an essential characteristic of the causal relation. These last two points require elaboration.

The belief that all causation is essentially mechanical in character is very widespread. A corollary of this belief is the assumption that the cause must be equal to, or greater than, the effect. Thus if a moving billiard ball A collides with, and imparts to, a second ball B a given amount of kinetic energy, ball A must possess a kinetic energy equal to, or greater than, that imparted to B. Granted that this principle holds within the domain of perceptible bodies, it by no means follows that mechanical causation of this "push and pull" type is descriptive of causal interaction between electrons or other constituents of matter, especially when these latter are no longer conceived as spatial in character. But if the ultimate constituents of matter be stripped of all properties save energy or mass, a trend already strong in submicroscopic physics, the concept of one solid, extended body imparting motion to another solid, extended body is clearly inadequate as a generic definition of physical causation. The rise of relativity and quantum mechanics, in which physical causation is expressed in terms of differential equations of mathematics, even now bids fair to supplant the Newtonian mechanics based on the concept of matter as ultimately spatial in character.

To common sense, as well as to many scientists, temporal order is also deemed a necessary attribute of the causal relation. Yet, as we have seen, it is possible to define causation without reference to temporal order. For time is not the precondition of the occurrence of events; rather, time is a function of events. Time does not generate events; rather, events generate time. Without events there is no time; events can be defined without reference to time, whereas time cannot be defined without reference to events. Time is one type of asymmetrical order among events. That the psychological awareness of time is a function of discrimination within or among events, or else the awareness of succession between events, seems now well established. That time is a function of events is even more obvious if we turn to chronometer time, for a chronometer, whether a clock or successive transits of some celestial body across the meridian, is but a name we give to some regular

and recurrent set of events with which all other changes or events may be correlated.

That time is in no necessary sense the essence of the causal relation may be seen from another side. While in many instances causal order does exhibit a temporal sequence or asymmetry, from whence it is commonly supposed that the "cause" must precede the "effect" in time, yet, in many other instances of causation the temporal span is absent or else impossible of accurate specification. Hence it is the invariance in occurrence, not the temporal sequence, that constitutes the essence of the empirical concept of causality. Two events or specific sets of events are causally related when neither occurs without the other.

Yet few or no cases of the actual occurrence of two events or sets of events are such that neither occurs without the other. While a certain degree of frost seems always to be followed by the death of the flowers in the garden, flowers often die in the absence of frost. On the other hand, while yellow fever seems always to be contracted through the agency of certain microorganisms deposited in the blood stream of the victim through the bite of a certain mosquito, many persons are bitten by infected mosquitoes who do not contract the disease. In light of this, we may, following Cohen and Nagel, define causation in a somewhat less rigid form. "By the *cause* of some *effect* we shall understand, therefore, some appropriate factor invariably related to the effect. If A *has diphtheria at time t* is an effect, we shall understand by its cause a certain change C, such that the following holds. If C takes place, then A will have diphtheria at time t, and if C does not take place, A will not have diphtheria at time t; and this is true for all values of A, C, and t, where A is an individual of a certain type, C an event of a certain type, and t the time."[1]

Moreover, causal relations may be either unilinear or multilinear; indeed, in any given situation they are *both* unilinear *and* multilinear. In other words, precisely which events are inferred to be causally related is in part determined by the context of factors within which the relation is considered. Suppose that at a given time t I depart from my office and turn down a certain walk and pass under a given tree, and that at time u a given leaf detaches itself from a certain twig overhead and lodges on the brim of my hat. Which of these conditions is the cause of the last-named event?

Depending on the context of factors within which the question is considered, any one of them, or any portion of them, or even all of them may be described as the cause or causes of the event. For, had my purpose of the moment been otherwise, had my teaching schedule called for a class meeting at times t or u, had no gust of wind arisen then, had the walk been laid out elsewhere, had not some condition of temperature, sap, parasite or other factor affected leaf and twig, in all probability the particular leaf in question would not have lodged on the brim of my hat. Since this particular event was contingent on a certain conjunction of many other events, any one of these latter may be exhibited as a cause of the event. Indeed, we may say that any one of them is not only *a* but *the* cause of the event, for, assuming all the causal conditions of the event to remain constant save one, then the presence or absence of that one will determine the occurrence or nonoccurrence of the event. No event can be the cause of another event in the absence of which the effect occurs.

The determination of the particular events between which the causal relation is affirmed to hold is also conditioned by the purpose of the inquirer. The answer to the question, "*Who* killed Abraham Lincoln?" must be of the form, "A certain person, John Wilkes Booth, was the cause of his death." On the other hand, the question, "*What* killed Mr. Lincoln?" must be answered in terms of the type of causal order of which we are in search. One answer might be, "A certain pistol was the cause of his death." Another might be, "Loss of blood was the cause of his death." Moreover, what is an adequate answer to one question will not in general be adequate to another question.

But if causal relations are conditioned by the context of factors within which the rela-

[1] Cohen, M. R., and Nagel, E. K., *An Introduction to Logic and Scientific Method* (New York, N. Y., Harcourt, Brace and Co., 1934), p. 248.

tion is considered, as well as by the purpose of the inquirer, so also, though in a different manner, are the "events" between which the relation is affirmed to hold. For events, in so far as significant for scientific purposes, are "constructs;" they gain their specificity, not because of any natural cleavages or boundaries inherent in them independent of our experience of them, but only because they represent some one or another aspect of the totality of process or change singled out in attention for purposes of understanding and control. Both the *nature* of the event, as well as the *limits* that mark each event from every other, are relative to, or are in part determined by, both the act of perception and the purpose of him who perceives or thinks about them.

That all events, at least in their qualitative phase, are functions of the perceptual process of him who perceives them is easily shown. Consider, for example, the qualitative characters of the series of events that we call a sunset. That the particular colors perceived own no existence apart from some perceiver is evident the moment we discover that the colors perceived are the result of a complex process of interaction between the perceiving organism with its brain, cortex, optic nerve, retina, cones, etc., on the one hand, and certain ether waves or moving particles external to the organism on the other hand. That the colors perceived are "psychological," not "physical," is now generally recognized by natural science. The color perceived is not the physical stimulus to the perception of itself; it is a "sensory" quality, not an attribute of external nature as such. In like manner it can be shown that the other qualitative characters of events are in similar case. Yet this does not imply that things exist and events occur only in the mind of him who perceives or thinks about them; it merely asserts that the initial data of empirical science—the things, events, and relations between them observed in sense perception—are relative to the observer and are functions of his interactions with certain forces in the world of physical nature objective to him.

There is, however, another sense in which the character of events is determined by the inquirer. We refer to the limits or boundaries ascribed to events. Here it is the purpose of the inquirer that so determines them. Consider the event or, more accurately, the series of events, known as the Battle of Stalingrad. When did the battle begin; when did it end? Did it begin when the first German entered the city limits of Stalingrad? Or, if not then, in the fighting on the approaches to the city, and, if so, at what remove from the city? Did it end with the formal surrender of von Paulus, or only when the last German was safely rounded up, or in some other event? Obviously, any answer to either question is arbitrary, arbitrary because dictated by the purpose of the inquirer.

Nor is the case otherwise if we consider the battle as a series of more circumscribed events. However simple or complex any event be taken to be, the discrimination of it as an event is always dictated by the purpose of the inquirer: This is as true of the motion of a single electron through one millimeter of space as it is of the Battle of Stalingrad. In short, scientific inquiry is no mere staring at the continuum of flux or change that is nature, in which a given event magically detaches itself from the flux and thrusts itself before attention to the exclusion of all other events; rather, scientific inquiry is *purposive,* which is but to say that only such aspects of nature's processes are sought out or are instituted by us as are relevant to the purpose of some inquirer.

(3) We turn to the *functional* concept of causality, in which the statement of order in terms of the familiar language of causation is coming slowly but surely to be replaced by mathematical functions or equations. While physicists still speak of "bodies acted on by forces," and the like, yet, when they express the changes described by this phrase in quantitative form, we find actions replaced by changes in certain physical coordinates or parameters, and "force" denotes that aspect of the change that varies as Ma, the product of the mass by the acceleration. While the tendency to reify or hypostatize such expressions as Ma is still strong (a source of much error and mystification in science), the whole tendency of modern

experimental and mathematical physics is to eliminate the concept of matter as an ultimate substance, and to find the elements of permanence among events in the mathematical relations.

The translation of causal relations into the language of mathematical equations may be illustrated in the fields of classical dynamics and quantum physics. A typical example of the causal process in classical dynamics is the motion of a particle projected vertically upward in the earth's gravitational field. If we know the mass of the particle, its distance from the surface of the earth, and the time and direction of its movement as measured from a fixed origin, then the causal process can readily be translated into a differential equation of the second order, in which time again enters into the determination of the concept. Time reappears, however, not as a perceptual relation, but as an independent variable expressed in arithmetic form. And, since all the determining variables are expressible in terms of number, the idea of efficiency or production is entirely eliminated in description of the process; indeed, the changes described by this equation are no longer stated in the familiar language of causation. Yet the equation contains all that is essential to a causal description of the process, for, given the position and state of motion of the particle at any instant, we can calculate its position and state of motion at any earlier or later time. In short, the differential equation gives a detailed causal analysis of the process.

We now give an illustration of the formulation of causality in terms of the concepts of quantum physics. Quantum physics deals with the submicroscopic, or elementary, processes in nature. The classical kinetic theory assumed determinism for all elementary processes; that is, the invariant causal laws of macroscopic physics were assumed to hold within the submicroscopic realm. Since, however, the initial conditions of an atomic system are not determinable, a statistical causality is now assumed to hold for elementary processes, and elementary quantum processes are expressed by a probability function. And, if the initial values of the probability function are known throughout a region and if the values in the surrounding region are controlled during some interval of time, then the probability function is determined throughout the region during that interval of time.

The same sequence of values of a probability function is determined if the same relative initial conditions and values are given, or are assumed to be given, for some other region at some other time. Yet this does not imply that the totality of processes that condition any event exactly repeat themselves in nature; indeed, this seems possible, and then but approximately so, only under laboratory conditions. But it does assume, as does all scientific investigation, that if nature's processes did actually duplicate themselves, then the same set of conditions would always yield the same results. Thus the mode of determination of the probability function expresses the basic principle of causation.

An illustration will aid in clarification of these statements. Suppose we take one gram of uranium I, the time of whose half-decay is 4.5×10^9 years. If a be the number of atoms in one gram of the substance, then the number of atoms of it that will disintegrate within the time specified is $\frac{1}{2}\,a$.

Unfortunately, just as life insurance mortality tables enable us to make no prediction as regards the time when any particular individual will die, the probability function $\frac{1}{2}\,a$ predicts nothing as regards the time when any particular atom of uranium I will disintegrate. Just as we are unable to isolate all the factors that condition longevity, much less assign numerical value to each of them, we cannot isolate and measure all the factors that determine the exact time of decay of a given atom of uranium I. As regards the exact time of their decay, the sole prediction we can make about atoms of uranium I is that, given some finite number of them, a certain proportion of this number of them will disintegrate within some specified time.

Thus in the functional concept of causality we come full circle. Or, in terms of the three principles of order distinguished at the outset, causal order becomes one with mathematical equations, and causality ceases

to be a unique and independent type of order in nature, a long stride in the direction of greater simplicity in systematic science. Yet this does not imply that we should dispense with the common-sense concept of causation; indeed, so long as men are concerned with control of nature's processes, the concept is both useful and descriptively true.

In any case, it is in this functional sense, more than in any other, that causation identifies itself with the principle of intelligibility of nature. In truth, that nature is an ordered cosmos, or that nature is intelligible, is the basic postulate of science. While the evidence in support of this postulate is as yet fragmentary and far from conclusive, the rapid extension of experimental methods to domains of natural events hitherto unexplored supplies cumulative evidence of its truth.

That some measure of faith is a necessary ingredient of all beliefs in empirical science is easily shown. Suppose we affirm that all fish swim or that the law of gravitation holds true throughout the Universe. Granted that, of all the fish thus far observed, all do swim; granted that all relevant observations and experiments thus far made confirm the universal truth of gravitation. Yet in any literal sense these observations "prove" nothing: i.e., all inferences from "some to all" are logically fallacious. Since scientific hypotheses and laws always ascribe some property or relation to *all* members or instances of some class of things or events, and since we must always do this by the technique of sampling, the propositions, "There are some fish which do not swim," and "There are domains in the Universe wherein the law of gravity does not hold true," must always retain *some* probability value, however slight. In sum, empirical science cannot demonstrate the nonexistence of negative instances, save only by the artificial device of definitional exclusion.

WHAT, now, is "chance;" what does the term connote, and of what is it descriptive? "Chance" possesses a number of meanings, four of which deserve mention.

(1) A first meaning of "chance" may be dismissed with brief comment. Here, chance is identified with the feeling of perplexity and uncertainty incidental to indecision when some situation demanding action is viewed prospectively, or else with the sense of indeterminateness or uncertainty resulting from ignorance of the causal nexus of events when viewed apart from human choice and action. In this the "subjective" sense of the term, chance signifies only our ignorance of the causal order of events. And, it need scarcely be added, ignorance is evidence only of ignorance.

(2) A second meaning of "chance" may be illustrated by the question, "What is the chance of obtaining thirteen spades in a given hand at bridge?" Here, chance is identified with probability value. Yet to affirm that the probability of the occurrence of this event is $(\frac{1}{4})^{13}$ is to make no prediction as regards the specific character of the hand in question, it merely asserts that, in the ideal case of an infinite number of hands at bridge, the hand in question will occur with a frequency expressed by the fraction. Moreover, the assignment of mathematical value to probability functions of this type is possible only if three conditions are fulfilled: (a) the alternative events must all be known or be assumed to be known; (b) the alternative events must be mutually exclusive of one another or be assumed to be so; and (c) the alternative events must be, or be assumed to be, equally probable.

In contrast to this mathematical, postulational, or a priori technique, is the empirical or experiential method of determining probability values. What, for example, is the probability of getting a head on a single toss of a single penny? In one experiment, in which a single penny was tossed 10,000 times, heads turned uppermost 5,258 times and tails 4,742 times. And, we may suppose that, as the number of trials increased, the proportion of heads that will turn uppermost will more closely approximate $\frac{1}{2}$. Thus the determination of probability value based on assumptions seems to be verifiable by appeal to experience or to work well in practice.

In any case, and the only point we need make here, the ascription of some law of frequency to the occurrence of an event is no evidence of indeterminism in nature, but,

rather, is itself a kind of order in the occurrence of events which makes possible prediction of the frequency of their occurrence.

(3) "Chance" also denotes zero correlation or absence of concomitant variations between two or more sets of events. Suppose the correlation between variations in sunspots and fluctuations in the price of men's hats in Kalamazoo is zero. Yet the absence of positive or negative correlations between them in no way implies that either set of events is uncaused, much less that each is no part of some determinate order of events; it merely suggests that the two sets are not causally related. For causal order is determinate; any event is not causally related to all other events, but in any significant sense only to certain ones of them.

(4) "Chance" also denotes *objective* chance. By objective chance we mean, not the frequency with which some event may be predicted to occur, nor yet the vague sense of possibility incident to ignorance of the causal order of events, nor yet the absence of correlations between events, but the ascription of indeterminism to certain events apart from all subjective or other factors. Indeterminism, however, does not imply that events deploy themselves *in vacuo,* but, rather, affirms that certain events have no determinate causes, or that, given the determinate causal conditions $a, b, c, \ldots, n$, any one of an indeterminate number of effects $x, y, z, \ldots, n$, may occur. Needless to add, scientific observation and experiment disclose no evidence in support of indeterminism *in rerum natura.*

We turn, lastly, to the controversy over freedom and determinism in nature.[2] Are all events wholly determined, or are at least some of them at times and places indeterminate or free?

Historically, three meanings of "freedom" may be distinguished: (1) indeterminism, or objective chance; (2) free will; and (3) self-determinism. Four meanings of determinism also come to light; (1) necessity; (2) the determination of desire and will by causes antecedent to them; (3) coercions and compulsions exerted by environment upon an agent in opposition to its will; and (4) causal or scientific determinism.

(1) Is nature, at least at times and places, indeterminate? One well-known writer asserts that "a world that is at times and points indeterminate enough to evoke deliberation and choice is a world in which will is free" because "will and choice are determining factors." Yet to affirm that deliberation and choice are determining factors is no evidence of indeterminism in nature; at most this but shifts the locus of causal determinism to human deliberation and choice. Natural law is not abrogated when employed as means to human ends. Indeed, it is only in virtue of determinism in nature that deliberation and choice exert causal control over things and events. Yet this is but to affirm that the course of events may be controlled in such a way as to fulfill our desires and purposes. In short, cognitive inquiry is instrumental to supplanting ignorance and impotence with knowledge and control, the very success of which implies determinism, not indeterminism, in nature. By determinism we mean that, given any event A, or determinate set of events $a, b, c, \ldots . n$, only one event or determinate set of events r will occur.

Nor, as we have seen, does Heisenberg's "principle of indeterminacy" afford evidence of indeterminism in nature. Granted that, of a given number of radium atoms, we can predict what proportion of them will disintegrate within some specified time; granted that this type of prediction is purely statistical in reference and enables us to predict nothing as regards the time when any particular radium atom will disintegrate. Yet it is far more probable that our inability to predict that future behavior of a given atom is due, not to actual indeterminism in its behavior, but rather to our inability to specify its age, the physical milieu in which it exists, etc., or, in general, to our failure to devise experimental operations which isolate, measure, and control the course of its interactions with other things. This, however, is but confession of ignorance,

[2] This part of my paper appeared in somewhat altered and more detailed form, under the title of "Freedom in Determinism," in *The Journal of Philosophy,* Vol. 39, 1942, pp. 520–527.

not evidence of indeterminism, in the behavior of individual radium atoms and their constituents.

While there is no conclusive proof that nature is not at times and places indeterminate, and while some measure of faith must always sustain us in denial of indeterminacy, yet, that all events, both physical and otherwise, have natural causes and natural effects conformable to invariant law, seems now so highly probable that the burden of proof rests upon him who would deny it. If this be so, as we believe it to be, then freedom must be found in a world of things and events *all* of which are strictly determined and hence cannot be identified with indeterminism or objective chance.

(2) The ascription of freedom to the "will," or the identification of freedom with the causal agency exerted by desire and volition, likewise points to the truth of determinism. That these processes do occur, if only in the behavior of living organisms, is a fact. But as mere facts in isolation they are neither free nor determined. For both freedom and determinism are assertible only in terms of relations of occurrence between things and events. Hence the question is not whether or no desires or volitions occur, but whether, in their occurrence, they have natural causes and natural effects conformable to invariant law. That all "physical" events are thus describable is the basic postulate of natural science. That all "psychological" events, desires and volitions included, are likewise both determined and determining factors is the basic postulate of psychology as natural science and may now, we believe, be accepted as highly probable.

More specifically, any event may be viewed from any or all of three points of view: *retrospectively,* in relation to events that precede it; in *simultaneous occurrence* with other events; and *prospectively,* in relation to events that follow it. Hence the basic question is whether the relation of occurrence between events is invariant or chance in character.

(a) Viewed retrospectively, all events seem to have causes; indeed, experience discloses no events that are uncaused. The difficulty is not whether or no events have determinate causes; it is the determination of what precisely their causes are. Observation and experiment alone can supply the answer. By "observation" we mean, not the mere parade of events before awareness, but "scientific" or highly refined and controlled observation, in which tested operations are employed in search of evidence in verification of some hypothesis. In this, the one sense alone in which it is cognitively reliable, observation does in fact disclose the specific causal antecedents of an ever-increasing number of events, in consequence of which the hypothesis of indeterminism becomes increasingly less probable. Moreover, when we turn to events amenable to experimental control, it is discovered that *all* such events have definite and determinate causes. Indeed, we believe that those who "argue" most vigorously against scientific determinism will be found actually to oppose, not determinism as such, but chiefly or only the ascription to certain events of causes which, in virtue of emotive, cultural or other "Idols of the Tribe," are felt to be repugnant.

(b) Simultaneous events are also discovered to be causally related in specific ways. Events are simultaneous when no temporal difference can be distinguished between them. When two simultaneous events or sets of events occur in such wise that the occurrence of either is always accompanied by the occurrence of the other, we have grounds for inferring or at least suspecting causal relations between them. When, in addition, variations in one are observed to be accompanied by variations in the other, we have further and, we believe, more conclusive evidence of causal relations between them. While simultaneity in occurrence and concomitance in variations between events do not of themselves "prove" the existence of causal relations, much less distinguish which are causes and which effects (a distinction which can be made only by appeal to the body of invariant relations already independently established), they nevertheless constitute added evidence of the truth of determinism.

(c) Viewed prospectively, a given event or set of events seems always to be followed by another specific event or set of events.

This is always true of events when experimentally controlled; moreover, it is as true of "psychological" as of "physical" events. With the rapid extension of experimental methods to an ever-increasing number of events in all domains of existence, it becomes increasingly less probable that there occur in nature events whose effects are indeterminate.

It may, of course, be objected that the evidence now available in support of universal determinism in nature is as yet too fragmentary to enforce conviction of its truth. True, nature is under no compulsion to conform to any kind of order or invariant law, deterministic or otherwise. Nevertheless, all the evidence we have points to the truth of determinism; moreover, as experimental methods are extended to domains of events hitherto unexplored, a rapidly increasing number of them are discovered to exhibit a determinate order in their occurrence, even though no two events will be found to have precisely the same causal antecedents. In truth, nature exhibits many different kinds of order: We need only recall the famous proposition of Poincaré that every set of phenomena, even though they be infinite in number, cannot only be fully explained by a theory that can be developed, but also by infinitely many different theories.

(3) We come, now, to the identification of freedom with *self*-determinism or *self*-determination. Self-determinism, however, possesses two meanings, and it is important that we distinguish between them. In its broader sense, it denotes the fact or belief that every event has effects such that the nature of the effect is "determined" by the nature of the cause. In this ubiquitous sense, it is, as we have seen, highly probable that all events are self-determining. A variant of this view is the conception that all things act in accordance with their own nature. Yet empirically, the "nature" of a thing is essentially the modes of its interaction with other things; to ascribe a behavioral trait to a thing in isolation is mystical, not empirical. The attribution of some quality of behavioral property to a thing as such would seem to be little more than a convenience of language.

Self-determinism possesses also a narrower meaning. It denotes the fact or belief that "volition" is a dynamic agency operating as a cause in fulfillment of the desires and purposes of a self. It is not merely that volitional acts have definite effects conformable to invariant law, but also that *the effects be such as to constitute the ends willed.* It is in this sense that we would identify freedom with self-determinism, self-fulfillment, or "free will."

The meanings of determinism may now be dismissed with brief comment. One exception alone need be noted: Necessity is not determinism; rather, it is irrelevant to both freedom and determinism. Two meanings of necessity may be discriminated, the sense of compulsion and logical implication. The former is psychological, the latter logical, in character. Determinism is not the feeling of compulsion; it is an hypothesis to be tested by objective evidence. This evidence, we believe, points overwhelmingly to the truth of determinism. Nor is determinism to be confused with logical implication; implication is a purely formal relation between propositions and involves no necessary reference to material facts. One event may lead us to expect another event; one event never necessitates or implies another event.

As regards the other meanings of determinism, they are all true, or at least are so highly probable as to be accepted as true. For, as we have seen, all things and events are the outcome of definite causes; indeed, they are both causes and effects. Moreover, all events amenable to scientific observation and experiment are such that the causal relations discovered between them are found always to be invariant in character. Hence scientific determinism is true, or at least highly probable, and, since this implies that form of determinism which determines only from behind, both of these determinisms are true. As regards the remaining meaning of determinism, a qualification must be made with regard to it. The identification of determinism with frustration of desire and impotence of will does not exclude freedom. For only in case *all* desires were frustrated, *all* volitions impotent, would freedom be an illusion. Yet desires and volitions are in fact neither wholly impotent nor wholly

omnipotent: Some desires and volitions are frustrated while others are not. *Where they are fulfilled there is freedom; where they are frustrated or balked there is unfreedom.*

Freedom, thus, is a property of desire and volition. A living organism is free in the degree to which it fulfills its desires and volitions; it is free *in the degree to which its acts fulfill what it wills.* Will is *both* determined *and* free. As a natural effect of prior causes it is wholly unfree and wholly determined; as a motor-affective act which has determinate effects, at least some of which are desired and willed, it is both determining and free. If it were not a cause it would be wholly unfree. Yet always to have determinate effects does not make it free, for, if this alone were the case, then freedom would be the same as determinism. *Those wills are free that have just those determinate effects that are desired and willed.* This is the empirical meaning of freedom.

Thus it is that there is freedom only in determinism. Freedom is but a special case or species of determinism. Hence there is no antinomy between freedom and determinism. All desires and volitions have both determinate causes and determinate effects; yet, though rigidly determined, some of them are free. Thus there is no issue between freedom and determinism; the issue is solely between freedom and unfreedom, or, possibly, between freedom and indeterminism. But since indeterminism is highly improbable, there seems to remain only the question as to whether desires and volitions are free or unfree.

OUR HOME

This evening with the setting sun
I have walked out to another world
Where to the west the day in leaving
Sends back a summer storm,
Which, letting down long banners,
Rustles toward me over the wide plain;
And fields of gold-brown wheat and green corn
Sweep away to a far purple ridge and a paling azure sky.

Somewhere a last bobolink is singing,
A meadowlark toasts the retiring day once more
And the weeds around hum pleasantly—
Insect universes bedding down;
From the fast advancing cloud bower
Sounds the first thunder, shaken about
And echoed back in a scarlet pheasant's raucous shout;
And a somber hen cowers expectantly on her wondering chicks.

Now the first cool drops spatter the dust
And the sun is disappearing beyond the world,
With upflung splendor embracing the storm like a friend
And making it for me a small and friendly thing;
And in a few moments when it is over,
I hear the Angelus of The Sixth Symphony
Played by the storm, the sunset, the lark, and my heart
And Jupiter announces the night.

—Pvt. T. A. Morrill, Biggs Field, Texas.

THE TROPICAL GRAPE

By JOSEPH L. FENNELL

INTER-AMERICAN INSTITUTE OF AGRICULTURAL SCIENCES, TURRIALBA, COSTA RICA

For hundreds of years people of the Tropics have tried hopefully, almost desperately, to cultivate the grape. Literally hundreds of domestic varieties and every conceivable method of culture have been tested without success. But tropical varieties had never been planted.

Since climate is a controlling influence in the growth and adaptability of all plants, it is advantageous to consider the grape with respect to its geographical distribution. It is found that all species and domestic varieties, wild and cultivated sorts the world over, fall into three climatic groups: the arid-temperate, the humid-temperate, and the humid-tropical.

The arid-temperate group is composed of those species and domestic varieties that are native to regions having hot, semiarid summers and moist, cool (freezing) winters. The well-known European vineyard kinds (*Vitis vinifera*), which are believed to have originated in the region of the Caspian Sea, are examples. They are the grapes of antiquity and the Bible, so frequently mentioned in all recordings of the ancient past. Just when man first focused his attention on the original wild type or made the initial efforts at domesticating and improving it is beyond the horizon of recorded history. Viticulture was flourishing in the time of Homer and doubtless came into being long before his day.

Hundreds and even thousands of improved varieties of this climatic group are known, a few of the more prominent of which are: Malaga, Emperor, Sultania, Cabernet, Muscat—all pure forms of *vinifera* species.

The humid-temperate group is native to moist, temperate climates where the growing season is short, moist, and warm and the winters are long and cold. All the wild species native to the United States east of the Rocky Mountains and north of central Florida belong to this climatic classification, as do a few others from eastern Asia. Within this group there are many improved, domesticated varieties, largely derived from the species *Vitis labrusca,* which have been developed in North America during the last 150 years. A few sorts, such as Concord, Delaware, Niagara, Beacon, Catawba, and Isabella, are widely grown in the United States east of California and are popularly known as "slip-skin, or American, grapes," in distinction from the meaty, solid-textured European types.

The humid-tropical group comprises the kinds of direct interest here. Although there have been in existence no improved or domesticated grapes of this class, there has been an interesting and valuable assortment of basic wild material. Fourteen or more wild species having humid-tropical adaptabilities are known, and there is a wide and varied range in the horticultural factors involved.

Within the humid-tropical group of wild species there are practically all of the fundamental qualities needed for the development of a new class of improved tropical vineyard kinds. The qualities of rugged health and disease resistance, high productivity, large berries and clusters, and sweet, delicious flavors can be found in one or another of the wild selections.

Since all species of true grape can be intercrossed with little difficulty, there remains but the matter of a methodical apportionment of the desired characters in conformity with specialized *Vitis* genetics until we realize our ultimate goal—fine vineyard grapes for the Tropics and Subtropics.

In the autumn of 1935, in pursuit of a general interest in botany, I became deeply immersed in the taxonomic complexities of the genus *Vitis.* Along the margins of the glades and streams and in the tropical hammocks of southern Florida there were various wild grape species that were faultily described and in some cases unknown in the reference manuals. Coincident with my difficulties in the identification of these wild

Florida species, I became interested in bringing about their horticultural improvement.

In less than a month my project was roughly outlined and under way. The beginning was slow and difficult. To attempt to bring into being a new race of fruiting plants with superior qualities in a climate and on soil where only local wild types could survive to the flowering age is naturally a difficult undertaking. The acquisition of that initial "toe hold" is generally the most difficult part of any such program.

The first six years of work was performed in the tropical climate of extreme southern Florida, south of Miami. Early in 1942 the project was transferred to Mayaguez, Puerto Rico, for a brief period. Then, in 1943, it was moved to the Institute in Costa Rica, where it is now (1944) officially under way.

Procurement of pollen of the finer temperate-zone domestic sorts constituted the hardest problem in the early stages of the effort. Of more than 150 varieties of the best European and American cultivated kinds that were carefully planted in the test vineyard as potential pollen vines, only six or seven lived to reach flowering age and they blossomed too late in the season to be of value. Without a local production of pollen no breeding work could be done. Because of the earliness of the tropical season and the low viability of grape pollen when stored, it had become obvious that the success or failure of the entire program hinged on the solution of this difficulty. The handicap was ultimately overcome through the development of a technique that rendered it possible and easy to make any cross desired.

Our tests of the many domestic, temperate-climate grapes alongside the wild tropical selections have afforded a valuable opportunity to study the factors responsible for success or failure in tropical viticulture. While the European and North American cultivated sorts struggled under hopeless climatic and disease handicaps, the wild tropical kinds grew and fruited luxuriantly. This fact has lighted the way through the entire effort.

The collection of the best wild vines from river swamps and tropical jungles for use as breeding parents has been an even slower and more difficult task. Yet in many ways it has been something of a colorful adventure.

Many exploration trips afoot through sultry, snake-infested woods and by canoe down treacherous jungle streams have been highlighted with moments of excitement, disappointment, and joy, along with a varied assortment of discomforts and hardships. The quest for the imaginary and ever-elusive ideal wild vine, which the mind's eye might vision as decorating some secluded tree or canyon ledge with a profusion of fine black grapes, has almost led to Eldorado.

I shall not forget the circumstances associated with the discovery of Number 24. The picture of it is most vivid: the lone canoe journey down many miles of treacherous river, flooded by summer rains; the darkening forest, crowding the channel with jungle growth; the colorful leafy streamers of the Calloosa grape that canopied the treetops; then, in the faint light of a setting sun—a glorious sight! In a reclining tree along the east bank there hung beautiful clusters of large black grapes that suggested Concord. They were sweet and good, and I was overjoyed.

The variety Sable is another parent vine taken from the wild which has an interesting history. It has been used perhaps more than any other in the author's latest breeding experiments. This selection is obviously a midway, or hybrid, form between the large Calloosa (*Vitis shuttleworthii*) and the Redshank-Figleaf grape (*V. rufotomentosa-V. smalliana*) and holds its present horticultural significance mostly through a peculiar act of chance.

It had been previously reasoned that wild hybrids with all-round superior qualities could and should exist within a certain region where the range of two superior species overlapped. It was carefully decided just what was wanted and just how good a grape in all respects might be possible from the best blending of the two species. With this goal in mind a determined search was undertaken.

After long days of search, by canoe and afoot, through the river swamps and forests and after inspecting literally thousands of vines, nothing outstanding had been found. True, there were many supposed hybrids but,

strangely, all of those examined seemed to inherit only the very worst qualities of the parent species.

Late in the afternoon of the fourth day, the large blue-green foliage of another of these midway types was observed sprawling over a group of palms and small trees along the left bank of the stream. A somewhat hasty inspection was made, a few moderately large and sweet grapes were tasted, but the vine was adjudged as of no especial value due to an apparent smallness of the fruit clusters. As it was late in the afternoon and there were many miles yet to be navigated through regions where camping for the night would be impossible, the journey was resumed.

Two days later while we were encamped on another stream many miles distant, the realization came that perhaps we had hastily and unfairly appraised this vine. The conviction became so hauntingly acute that we were soon packed and on the way back for another inspection.

A second and more thorough examination revealed typical fruit clusters seven to eight inches long from top berry to bottom. Secluded among the branches there was an occasional bunch that rivaled in size and appearance many of the better vineyard varieties of the temperate climates.

✓A selection of the wild Calloosa grape, referred to here as "Number 5," has been employed extensively in first-generation crosses. This vine was originally taken from the woods near Miami, Florida, some fifty years ago by J. J. Soar, of that city, and planted in his garden where it is now of immense size and canopies the top of a giant live-oak. Though the large berries of this wild parent plant are of poor edible quality, it has given some very good offspring.

✓Another wild selection, which we refer to as Number 18 and which belongs to the novel *Vitis gigas* species, has been much used in later crosses. It is healthy, vigorous, and extremely productive of large clusters with medium sized fruit of good quality. Its value was ascertained more by deduction than by the actual observance of characters and then only after sweltering, insect-plagued hours of discomfort. At the time of its discovery just two undersized berries could be found that had escaped the animals and birds, but later performance in the vineyard more than fulfilled every confidence placed upon it. The vine was discovered in southeastern Florida, in general association with such tropical plants as the mango and avocado tree and the coconut palm.

Ten or twelve additional wild selections have been used as parent vines, one of the most important of which is the Number 10. This is a supposed natural hybrid of the large Calloosa grape with the big clustered Redshank-Figleaf type. It has moderately-large sweet fruit, maroon red in color and a vine of great vigor and resistance to disease.

Another, the Number 6, seems to be a natural cross of the Calloosa and the pure Figleaf species. It has medium-large black grapes of moderately good quality, but the vine is a little subject to disease.

THE improved varieties derived from the wild vines of the jungle have afforded an inspiring sight. In the extremely humid tropical climate and waterlogged soils at Mayaguez, Puerto Rico, where all other domestic grapes were impossible to grow, the new kinds have made healthy and luxuriant growths. On the dry and impoverished rock-lands of southern Florida their growth has been equally good. Even under the tropical rain-forest conditions (110 inches precipitation annually) at Turrialba, Costa Rica, where grape culture in any sense has been thought impossible, our select-types in the Institute vineyard show beautiful and healthy vines with a normal set of fruit. It might be of interest to add, moreover, that these plants have never in their entire existence known the protection of fungicides.

The first fruit from any of the tropical hybrids was borne in the summer of 1940. One of the most interesting of these is the Fairchild (Number 106), which came from a cross of the wild "agras" of lowland Central America and the large-fruited European variety, Alphonse Lavellee. This unusual grape has a healthy vine with self-fertile flowers and medium-sized (one-half inch) sweet fruit that is several times larger than the berries of the mother species. The va-

riety is completely immune to the difficult grape-leaf rust, which is one of the most destructive grapevine diseases of the Tropics. It has been employed in various secondary crosses.

The new variety Tropico (Number 240) is one of the best descendants of the wild Calloosa grape that has yet borne fruit. It has a strong, productive, disease-free vine with large purple-black berries that are larger and of better quality than the well-known Concord variety of the United States. The fruit is sweet and tender with a thin skin and a delectable, mild, almost *vinifera* flavor. Tropico was originated from Number 5, crossed with a complex hybrid of *V. champini, V. labrusca,* and *V. vinifera.*

Marco (Number 3) is a medium-sized, dark-red grape, quite like the variety Delaware. It has in fact a sweet, mild Delaware flavor and an equally melting and tender pulp with small seeds. It is a juicy and truly delicious fruit. The vine is healthy and productive of moderately large compound clusters. Though the berries are rather small (average one-half inch) and a bit seedy for a fresh dessert fruit, they would in all probability make an excellent light-colored wine. The variety has three parts tropical wild parentage and one part temperate cultivated blood. It is a descendant of Number 9 (*V. rufotomentosa*) crossed with a hybrid of the wild Calloosa grape and the northern cultivated variety Niagara.

Another hybrid selection, Wachula (Number 126), has the same parentage as the preceding. It has compact clusters of sweet, medium-sized (one-half inch) black grapes of good mild-vinous flavor. The vine is healthy and very productive.

Crosses between Number 9 and the finer European varieties have given some interesting progeny. One of these, Biscayne (Number 113) has dark red, translucent berries that average one-half to five-eighths inch diameter. They have a sweet, delicious, *vinifera* flavor and are borne in moderately large clusters. The vine is strong, healthy, and productive.

Some combinations between the wild tropical selections and the North American vineyard sorts (humid-temperate group) have given fruit with the Concord, or *labrusca,* taste.

Throughout the course of the project more than 100,000 hybridized grape seeds have been planted. Many thousand seedlings have been grown and hundreds have already borne fruit. Carefully planned secondary crosses, between the best of these newly developed sorts, as again with the better of the wild selections, have been made and are fruiting for the first time. There is every reason to expect some good things from these later crosses.

Plant diseases and other pests are a factor that must always be considered. Under humid, tropical conditions some diseases of the grape are very severe, even fatal, unless the variety possesses special resistance to them. In the development of our novel tropical types we have given much consideration to this matter and have obtained kinds that have health and disease immunity to a remarkable degree. As a result, fungus diseases have never troubled our better selections.

Insects, birds, and animals are occasionally a problem, as in the cooler climates. In Central America the *zompopo*, or leaf cutting ant, may do much damage if not controlled. It is easy to eradicate, however, and would never exist in a well-kept vineyard.

Though it is beyond question that these recent humid-tropical originations are incomparably better adapted to hot, humid climates than are any other of the known grape varieties planted at present in the Tropics, the full limit and range of this superiority must yet be determined. Their value for the manufacture of fine wines and as tropical market grapes presents highly interesting and ever-enlarging possibilities.

Good natural wines (without addition of sugar or alcohol) have been made from a few of the new sorts, but their individual and finer points in this regard must await later investigations.

These new tropical grapes have not as yet been made available to the public. Usually it requires several years after a variety is developed before it can or should be released. Still, we hopefully believe that within a relatively short time it may be possible to make a limited first distribution of a few sorts.

THE RELATION OF NATIONAL PROHIBITION TO MENTAL DISEASE

By CARNEY LANDIS and JANE CUSHMAN
NEW YORK STATE PSYCHIATRIC INSTITUTE AND HOSPITAL

THE Eighteenth Amendment to the Constitution was intended to be a reform and something of a panacea to the social ills of a nation. It was neither proposed nor passed because of scientific curiosity—and yet there has never been either a medical or psychological experiment on so large a scale. Its subjects numbered millions, its duration more than a decade. Essentially, the hypothesis behind it was: can authoritarian control prevent or curb a habit which in many cases becomes a disease? Neither the propaganda nor the political motivation which led to this amendment was aimed at the occasional or moderate drinker. The avowed goal of the legislation was the alcoholic proper, the habitual drinker who fell down on the job if he had one, who filled the city and county jails, who caused his family untold misery, and who made up at least 10 percent of those entering our mental hospitals.

No scientific report of many of the results of this experiment has been made. For a time the corner saloon disappeared, bootlegging became a specialized and highly lucrative means of livelihood, and an indeterminate number of law-abiding citizens locked up their wine cellars. The exact extent of these developments is not known and probably never will be. Speculations have been reported, but speculations without evidence are likely to follow too closely the personal bias of the speculators. There is a body of evidence which does throw light on one aspect of the effect of prohibition, and that is in the published statistical records of state mental hospitals, which, among other things, give in some detail the annual admissions for alcoholic psychoses and for other mental conditions which are more or less related to the intemperate consumption of alcohol. Not only is this information factual rather than theoretical, but also it bears upon one of the main arguments which was advanced in favor of National Prohibition. Because New York is the most populous state in the Union, it has the largest number of hospitalized mental patients in the country. Furthermore, New York has the longest existing system of uniform statistical reporting. For these reasons we have based this inquiry on the reports of New York State since New York's figures provide an adequate representative sample of the effect of National Prohibition on mental disease.

Population statistics can be used to "prove" almost any point any propagandist desires to make. The following evidence, however, is based on simple, legitimate procedures. Most of the *rates* which we have obtained are rates for the particular age or sex in question. This is both important and necessary since there is a vast difference between the rates of alcoholic mental disease for women and men and for the different age groups. For example, if one should include all ages, then the 43 percent of the total population of the nation below age 25, who are but slightly affected by alcohol so far as the mental hospitals are concerned, would make all rates much lower by their inclusion.

We will presently show that the rates of hospitalization for any or all forms of alcoholic mental disease are quite different for men and for women and that this difference holds true in both the age range 20 to 40 years and that 40 to 60 years. If National Prohibition affected the rates of hospitalization, then each trend for alcoholism should be compared to the trend of the rates for *all* first-admissions to mental hospitals for all causes for the four age groups. (Since some individuals are admitted to hospitals once and others have several admissions to their credit, nearly all mental hospital rates are based on first-admissions, meaning that any individual case is counted but once, no matter how often he may have appeared.)[1]

The trend for *all* first-admissions to the

[1] Certain of these data have appeared in an article by the authors in the *Quarterly Journal of Studies on Alcohol*, 5: 527–534, 1945.

New York State mental hospitals, 1913 to 1943, of men aged 20 to 40 and 40 to 60, and women aged 20 to 40 and 40 to 60, per 100,000 of the general population of the state of the same age and sex, is shown in Fig. 1. These trend lines are more or less flat and level until 1930 when they began to increase. Investigations have shown that this increase is due, for the most part, to the erection of additional hospital facilities and, in the case

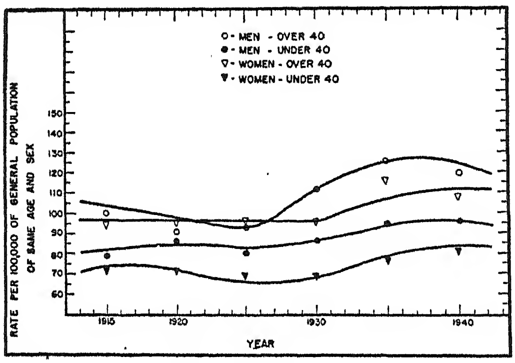


FIG. 1. FOR ALL MENTAL DISEASES
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1913–43.

of the older groups, to the steadily growing tendency for the hospitalization of people over 50.

The trend of the rates for the New York State mental hospital admissions for all those reported to be suffering from alcoholic mental disease between 1909 and 1943 has been analyzed by several different investigators. All agree that from 1908 to 1920 there was a relatively constant decrease in the rates of first-admissions to mental hospitals for alcoholic mental disease for both men and women. In so far as entrance to a mental hospital is concerned, in 1920 there were proportionately fewer suffering from an alcoholic psychosis than there were before or have been since. Between 1920 and 1934, however, more and more alcoholics entered the hospitals until the rate reached the level it had known in 1911. Since 1934 the rates have been either static or decreasing.

A scrutiny of the figures given in these annual reports indicated that not only were there different trends with respect to sex but there seemed to be differences with respect to the age at which these alcoholic patients first came to the hospitals. Since age is reported in 5-year interval groups, viz., 20–25 or 45–50, it was possible to work out rates and trends for any five-year age period. On doing this, we found that the trends for rates by age tended to fall into two groups. The trends for ages 20 to 40 and those for 40 to 60 were similar and could be added together. This was true for both men and women.

The trends of the rates for these four groups are shown in Fig. 2. These rates were computed per 100,000 of the general population of the state of the same age and sex. These curves show that each year between 1910 and 1920 proportionately fewer patients in each age group came into the hospitals with alcoholic mental disorders. From 1920 to 1934 there was a gradual increase in rate for each of the groups, which became much less pronounced between 1934 and 1943, actually falling off for the women and for the men aged less than 40. The curves make it clear that the bulk of the variation in the number of alcoholic patients

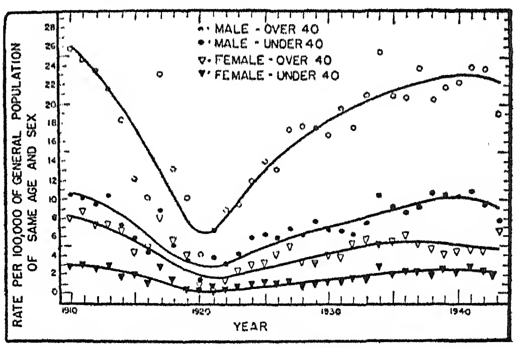


FIG. 2. FOR ALCOHOLIC PSYCHOSES
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1909–43.

is attributable to changes in the hospitalization rates of men in middle life, aged 40 to 60. In one sense, this only confirms common knowledge, namely, that it takes 10 to 20 years of heavy overindulgence in alcohol to achieve a mental collapse. Fig. 2 clearly shows that something—possibly National Prohibition—actually did decrease the rates of alcoholic psychoses in men between 40 and 60 for a time; that it had a minor effect on

men aged 20 to 40, and little or no effect on the rates for women.

Compare the trend lines in Fig. 1 and Fig. 2. In Fig. 1 there is no sharp 1910–1920 drop, and the lowest point shown occurs in 1925 or 1926, not in 1920 as in Fig. 2. Now if the pronounced variation in rates shown in Fig. 2 were a true drop—that is, if the patients in question were not only no longer being diagnosed as alcoholic psychoses, but were not coming to the hospitals at all—then there should be a clear reflection of these changes in the trend lines in Fig. 1, since alcoholic psychoses made up about 10 per cent of all first-admission patients between 1910 and 1917 and between 1925 and 1943. (The percentages for the years 1918 to 1924 were, respectively, 5.7, 4.0, 1.9, 2.8, 3.3, 3.6, and 5.5 per cent of all first-admissions.) What happened to the men who did not come to the hospitals in the years between 1918 and 1924—or did they come and did the doctors give them a different diagnosis? This was worth following further.

There are different kinds of mental disorders caused by chronic alcoholism. For the most part these are named in accordance with the particular symptoms displayed. These subvarieties go by the medical names of delirium tremens, alcoholic hallucinosis, Korsakoff's psychosis, chronic alcoholic deterioration, alcoholism without psychosis, etc. When the annual rates, 1912–1943, for the subvarieties among men aged 20 to 60 per 100,000 of the general population were computed, little annual variation in rates was found for the well-defined subgroups, viz., delirium tremens, alcoholic hallucinosis, or Korsakoff's psychosis. The great variation existed in the miscellaneous category. This category is made up of alcoholism without psychosis, alcoholism complicated by other forms of mental disease, and several of the less frequently reported subvarieties. The implication of this point depends on other evidence which we will next consider.

The *intemperate use of alcohol* among *all* first-admission patients of all diagnoses, expressed as a rate per 100,000 of the general population of the same sex between the ages of 20 and 60, is shown in Fig. 3. The groups represented by these curves had a steadily

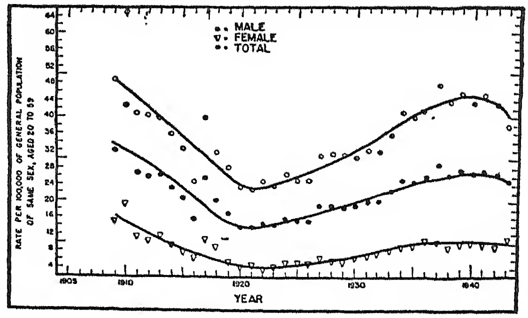


FIG. 3. EXCESSIVE DRINKING ALLEGED
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1909–43.

declining rate before 1920, but between 1920 and 1934 their proportion mounted, and after 1934 remained practically static. If, however, Prohibition were prohibiting even partially, then alcohol could not be used intemperately at all.

Since 1889, New York State has published the figures giving the total number of new mental patients admitted to the mental hospitals together with the "cause" or principal precipitating factor as reported by the family, by friends, or by the court. The trend of these figures for alcohol as the principal cause, expressed as a percentage of the number of patients admitted to the hospitals, is shown in Fig. 4. These curves show that the percentage of patients whose illness, according to the relatives or friends, was caused by excessive drinking, decreased between 1889 and 1891; increased 1892 to 1909; decreased 1910 to 1920; increased 1921 to 1934; and has been decreasing again since 1934. The highest percentages were in 1889, 1909, and 1934, and the lowest in 1891, 1920, and 1943. If these curves mean anything, they mean that the use of the explanation that alcohol causes insanity is not a constant excuse. The waves of the trend lines are not associated with the official figures giving the amount of alcoholic beverages sold in the state, with economic conditions, nor, in more than a vague fashion, with Prohibition. As soon as Prohibition went into effect, the use of the explanation, "alcohol, principal precipitating cause," started to increase, but it did not go out of style for several years after prohibition had been repealed.

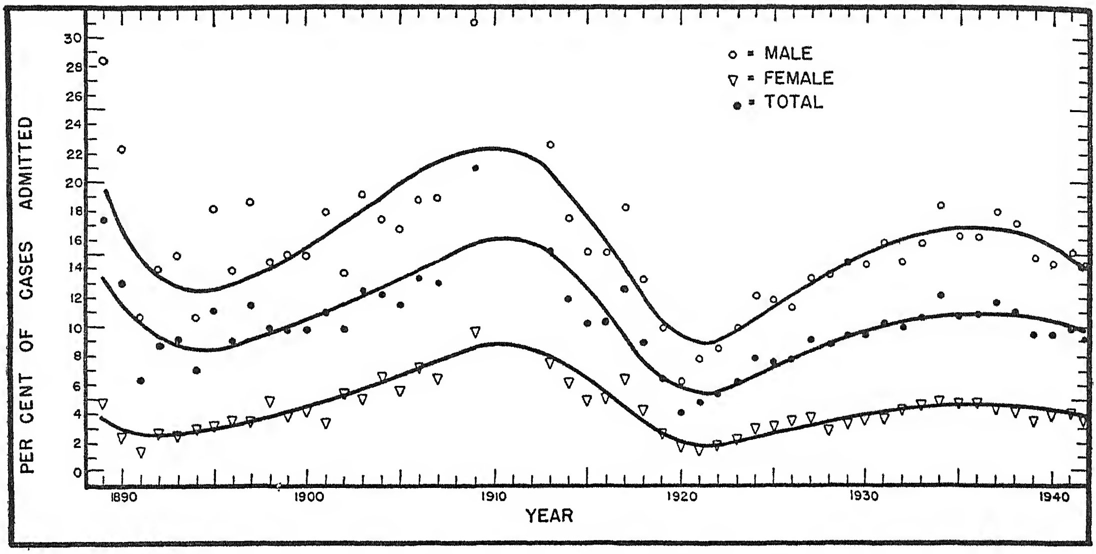


FIG. 4. ALCOHOL SAID TO BE PRIMARILY RESPONSIBLE FOR HOSPITALIZATION
TREND IN PERCENT OF CASES ADMITTED TO THE MENTAL HOSPITALS OF NEW YORK STATE FROM 1889 TO 1942.

The evidence from these trend studies indicates the following:

1. The rates at which persons were admitted to mental hospitals and diagnosed as suffering from an alcoholic psychosis were decreasing between 1909 and 1920, and increasing between 1921 and 1934, after which year they have been relatively constant.

2. The main group who caused this decrease and increase is that of men aged 40 to 60.

3. The variation in rate is chiefly attributable to the "miscellaneous" grouping of alcoholic disorders. Those forms of alcoholic mental disease which are clearly related to alcohol and only to alcohol, show little increase or decrease in rate.

4. Although there was a marked decrease and increase in the rate for alcoholic psychoses between 1910 and 1943, there was little or no reflected change in the rates of admission for all mental disorders. This means that candidates for mental hospitalization presented themselves at their regular rate despite Prohibition, but the hospital physicians saw fewer whose symptoms were complicated by alcoholism.

5. The trend of the rate of admissions to mental hospitals of those who were said to be intemperate in the use of alcohol closely paralleled the trend of those for whom a diagnosis of alcoholic psychosis was made. That is, it decreased from 1909 to 1920, increased 1920 to 1934, and has been level or declining since 1935.

6. The percentage of patients entering mental hospitals for whom it is said that intemperate use of alcohol was the principal cause leading to hospitalization has varied widely between 1889 and the present. Between 1909 and 1920, before Prohibition was effective, there was a constant decrease in this percentage while from 1920 to 1934 there was an increase, though most of this time saw Prohibition legally in effect.

These are facts which justify some conclusions and certain questions. It is a safe conclusion that National Prohibition did not affect the rate at which patients enter mental hospitals. If it did anything, it accelerated the rate.

Alcohol and mental disease bear a complicated relation to each other. Many persons who are experiencing the first symptoms of mental disorder resort to alcohol as a means of drowning their otherwise inexplicable feelings of guilt, anxiety, unreality, disorientation, etc. Others drink because the disease process weakens their previous good judgment, self control, and normal inhibi-

tions. In either case the alcohol may make the symptoms acute—act as the major precipitating cause, and even conceal the true disease when the person first reaches the hospital. In such instances the effect of Prohibition would show up in statistical reports in two ways. First, if there is real difficulty in obtaining alcoholic beverages, then the case will eventually reach the hospital uncomplicated by alcohol. This is the simplest explanation for the immediate decrease in 1919, 1920, and 1921. There was no real decrease in alcoholic psychoses, but there was a decrease in cases in which alcohol was a complicating factor. Second, the physicians who admit the patients to the hospitals and who fill out the statistical report cards were aware of National Prohibition and hence did not "see" so much evidence of alcohol in the patients who came to the hospitals.

The true alcoholic psychoses are achieved after ten to twenty years of steady heavy drinking. If prohibition had affected this sort of mental disease, then the real decrease should have shown itself in a decrease in the rate of chronic alcoholic deterioration and Korsakoff's psychosis cases during the 1930's. There was no such decrease.

We are not interested in reviving the old propaganda battle for and against Prohibition. The facts here put forth must be explained, and if any explanations are to be credited, they must bear on three main questions. First, if temperance propaganda caused the 1910–1920 drop in the rates, what caused the drop back in 1890, before the social pressure resulting in the Eighteenth Amendment had gained effective headway—and why have the rates been decreasing since the late 1930's? Second, why were increasingly more alcoholics admitted to mental hospitals between 1920 and 1934, an era when legal restriction of liquor traffic was part and parcel of the Constitution? And finally, why, when repeal was put through, did the rates proceed to level off or decrease instead of increasing suddenly and mightily?

Certainly no simple cause and effect relation of National Prohibition to mental disease provides a satisfactory answer to any of these. It is safe to say that the argument that legal prohibition of the sale of alcoholic beverages will decrease the number of patients entering mental hospitals was decidedly not substantiated by the experience of New York State mental hospitals—and that, within the limits of our data, the "noble experiment" gave more negative than positive results.

HI HO AND HO HUM

Old Hi Ho first appeared in the SM on page 243 of the March, 1945, issue. His words of wisdom on how to make "everything all right" struck one of our oriental readers as too sanguine. He felt that Old Hi had been paying more attention to Pollyanna than to Confucius and was tempted to reply in a vein that would call for the signature "Ho Hum." Then the editor stepped in and persuaded these two doughty sages to prepare themselves for verbal combat. In keeping with the Christmas season he suggested that they argue the perennial question, "Is there a Santa Claus?" So, Virginia, you will find their thrusts on page 502 of this issue.

As both Hi Ho and Ho Hum are loyal members of the A.A.A.S., their thoughts naturally turned to Santa Claus as a potential contributor to the centennial building fund. Old Hi tries to persuade us that Uncle Sam and Santa Claus are identical (though everyone knows one is tall and thin and the other short and fat). He expects Sam Claus to put a thumping check in the Association's stocking. If you believe Old Hi, Sam Claus will provide for us a building not unlike the modest little structure he erected for "The Nine Old Men." To this day-dreaming our other oriental friend says, "Ho Hum." But he really believes in Santa Claus too, not as a mythical head of the body politic but as a "polypus" of 25,000 hard-working scientists. It is not too much to hope that our polypus has white whiskers and a twinkle in his eye, that he knows our great need for a building in which to work for him, and that he will surprise us this Christmas.—ED.

TRANSFORMATION IN SCIENCE*

By J. D. BERNAL, F.R.S.

UNIVERSITY PROFESSOR OF PHYSICS, BIRKBECK COLLEGE, UNIVERSITY OF LONDON

One of the paradoxes of the present time is that people may be able to change the world so rapidly that they fail to understand what they are doing. Another is that, while more has been found out at large and in detail about nature and man in the past thirty years than in the whole of history, there is less general appreciation of this knowledge and worse use of it than ever before. This is true partly because modern science has become more complex, but as much because it has become professionalized. Since some people are paid to understand it, why should the rest bother their heads about it? But ignorance of science means a failure to understand the factors underlying the critical events of our time. The history of the last few years should have shown that it is no longer optional, but absolutely necessary, for science to be understood, appreciated, and effectively used.

The tragedy of the present struggle is that the ends for which people are striving—food, work, security, and freedom—are gifts which science has put within reach of all. The resources, the knowledge, and the ability to build a new world are there, but instead we have danger and bloodshed, want and misery. If people could understand at least something of the possibilities which science offers, they would become more reasonably impatient of their present state, and more capable of changing it. For this, science needs to be expounded, and expounded in a new way which emphasizes its relation to this changing world. It is no use any longer attempting to present science as a series of pictures of the beauties or the mysteries of the universe and of nature. People have had enough of that already; it belongs to a time when individual and social security and the general running of society could be taken for granted. Indeed, the public is very justifiably irritated with the idea of the pure scientists' leisurely and secluded search after minute and remote things, when the world all around is being bombed to pieces; especially as airplanes, guns, tanks, and other engines of destruction seem to be the most noticeable products of scientific research.

But in any case the scientists themselves are no longer anxious to present a merely academic picture of a disinterested search after truth combined with a sublime indifference to the results of discoveries. Science has long been much more than this. It has become an integral part of productive industry and agriculture, it maintains health, it is increasingly involved in business administration and government. The methods and ideas of science are the dominant forms of thought and action in our time.

Science is no longer a spare-time occupation of a few dozen gentlemen of leisure, it is the whole-time job of some hundreds of thousands of research workers in nearly every country of the world. Science has become an industry, a small, but key industry. The cost of scientific research is borne directly or indirectly by industrial contributions, and already there are far more scientists working for industry than in universities or independent institutes. The very progress of science itself would be quite impossible without that of industry. The great discoveries of the present century were made possible by the industrial application of nineteenth-century discoveries. Without the mechanical technique or the ready availability of instruments of the chemical and electrical industries, modern physics and chemistry could not exist.

The war has given a terrible urgency to the problem of the proper relation of science to human affairs. It turns out that although science has been used very largely for the development of weapons, it is needed and used almost as much for the problem of preserving the life and health of the military forces and the civil population under the most difficult circumstances. This brings to the foreground the essential function of science, which is to find the means of satisfying

* A radio address, British Broadcasting Company.

the most elementary human needs. What has been seen as a necessity in war, was no less a necessity in peace. If the function of science had been fully realized then, the want and misery which led to the war would have been removed without the need for a struggle that can only waste human resources and destroy the powers of human thought. Modern science was created by the same movement that made capitalism. It is strongly attached to ideas of individual initiative and freedom of thought. However, the result of the combination of scientific technique and capitalist economy has been the creation of national and supernational monopolies, in the growth of which the old individualistic methods of industry have largely disappeared. Modern science, with its expensive equipment, its need for elaborate organization, and its close relation to industry, did not, indeed, even before the war, conform to the liberal idealist picture. Independent scientists had almost disappeared.

The war has already resulted in bringing science in every country in the world, America included, into the orbit of national defence on the basis of organized planning. Liberal scientists have a very natural fear that this will result in the destruction of the spirit that made science possible and in the loss of the ideals of free inquiry and free application. Some are even willing to acquiesce in a situation in which science will be quite a minor and ill-rewarded human occupation, provided that it is left alone by the state and industry; but this hope is as certain to be disappointed as that of the more thorough-going admirers of the past. Science is too useful, indeed, essential, to the day-to-day running of modern industry to be allowed to sink into safe obscurity. Science can only live when it is the forefront of human activity. What is needed is a more thorough analysis of those characters in scientific work that make for initiative in discovery and theory, and for critical thoroughness in the establishment of facts. It has already been found in practice that it is possible to retain these characters, combined with quite expensive organization, as long as the scientists are given responsibility and allowed to arrange their own work. What has been done and is being done for war could be done equally well for peace. The world of science has fortunately always been free from many of the mercenary motives that hinder cooperation in other spheres of life. Democratic collaboration is the essence of the work of a laboratory or the study of a whole range of natural phenomena.

One implication is that science can no longer stop short at establishing facts. It must go on to see that its discoveries are adequately and rationally utilized. It was in the Soviet Union that this was first realized. There, science has for many years taken a leading and recognized part in planning the utilization of national resources to the best advantage. The assessment of human needs has led to the rational study of the best ways of meeting them and given a broad direction to the progress of scientific research. Many scientists of the old school have feared that this would lead to the destruction of pure science in favor of applied. This has not proved to be the case. Pure science is probably being studied as intensely and over as wide a field in the Soviet Union as in any other country in the world, and certainly more so than in wartime England. In war, indeed, all countries are obliged to push forward with pure and applied science together, and the very critics of planned applied science are often in the forefront of this effort.

In practice, the intellectual and material concerns of the most active leading group in the community dominate the form and content of scientific thought of the time. The seventeenth century was the age of mercantile adventure, and sciences connected with navigation and gunnery held first place. At the end of the eighteenth century the rising manufacturers directed science towards chemistry and the study of heat. In the nineteenth century, the lead passed over to electricity. In every case, science served the interests of the limited group, and its benefits to the rest of the community were incidental. The essential difference between the present and the past is that we now have the possibility, and indeed the necessity, of organizing consciously what had before merely occurred by the interplay of social forces.

To organize consciously the machinery of civilization puts a much greater responsibility on human beings than they have had in the past. As long as no one is capable of tracing out the general effects of human actions the most terrible consequences can occur, and no one will be to blame. Indeed, the classical economists had always been able to demonstrate that crises were quite accidental by-products of a fundamentally sound economic system. But once man consciously takes charge of the general organization of production and distribution, the governing powers can rightly be held responsible for any failure. But we are still far from an ordered economic system, planned for the general good, and a long struggle lies between us and its achievement. Nor can the benefits that an ordered society will bring be achieved all at once. The task is one enormously greater than any man has before attempted. That any solution is possible is due only to the development of scientific technique and scientific methods. The technical possibility of human organization on a world scale is already there. We know how to make the goods, how to distribute them, and how to ensure the necessary communications. Even more valuable is the knowledge science has brought as to how to study and measure such a vast and complex thing as the changing needs of a human society.

The consciousness of the unity of mankind as an effective working community can only be realized by the use of science. The danger inherent in the present situation is lest an attempt should be made merely to utilize science in a limited way to serve special ends. But science used in this way can only result in increasing the misery and difficulties through which civilization is passing. Knowledge is not something to be harnessed like inanimate power. If it is, the stupidity of the end outweighs the technical excellence of the means. To reap the full benefits of science, there must and can be an intimate relation between science and social processes at every stage; in assessing needs, in studying and modifying social forms, in production and distribution problems, and finally in keeping guard over the results of its application to see that they do not turn in unforeseen and undesirable directions.

For that, the scientist must be in close, free, and friendly relation with the democratically ordered state machinery, and the people at large must have an adequate understanding of the possibilities and limitations of science. At present, science is far too much regarded as a mysterious production of magical results. The object of any attempted popularization of science should no longer be, as it rightly was in the nineteenth century, to acquaint the public with the mere facts of scientific discoveries. Far more important now is to relate those discoveries to their applications in ordinary life. This is a matter for education and publicity. Science has never taken the place it should in our educational scheme. It needs to be worked in at every stage and related throughout to the interests of each age of student.

Any realistic picture must, however, point out not only the possibilities of science, but the factors preventing those possibilities from being realized. There is today a growing concern with a better world after the war. But to consider the shape of such a better world without considering the obstacles that lie in the path of its realization is simply escapism. We can study those obstacles best, not projected in an imaginary form, but here and now.

Private and institutional greed, the desire to preserve orders and ranks in a society that has outgrown them, have been potent factors in the past, and are potent factors still, in delaying progress. Unless they are dealt with, and dealt with now, there is no chance for any better world.

That is the major practical problem of our time, and it is a social and political one. It will be solved by the people themselves. But the technical forms of the solution and the rapidity with which it will be possible to achieve a better world will demand science; and, for that reason alone, the people need to know and to understand, possibly better than the scientists themselves, what modern science is and how it works.

FREEDOM OF RESEARCH

By F. W. PRESTON

PRESTON LABORATORIES, BUTLER, PA.

THE practical applications of science have reached such a development that they may fairly be said to be the dominating feature of our civilization. Progressive industrial firms are engaged in research in a big way. A large number of technological societies has grown up, whose immediate purpose is to promote the welfare and progress of particular industries by encouraging research in appropriate fields and providing media for publication and discussion.

To be technological does not involve being unscientific. In fact, in *method* one has to be scientific. The distinction between science and technology is one of *purpose*. Technological research is consciously directed to utilitarian ends: it seeks a profit. The profit may be financial profit of an industrial firm, or more broadly the enrichment of society. The "profit" may even take a negative form, so that the motive becomes not the enrichment of our own national society but the impoverishment of an enemy society, which is the basis of much technological research today. The profit, whether positive or negative, resides in the application or physical embodiment of the results of technological research, for which reason it used to be called "applied" science, a term that seems destined to be superseded by the term "technology," which is Greek for the science of the arts and crafts.

There is a growing, and dangerous, school, which comprises most of the people who know no science and some who know at least a little of it, which argues that science originated in social needs, is justified only by its works, and that its works have to be utilitarian in character to be justified. Thus unless there is some clear national benefit to be derived from a research, it is to be regarded as unsocial, unwarrantable, and in fact intolerable. This view is immature, uninformed, and pagan; it is the view of the collectivists, fascists, communists, and politicians.

There is a maturer school, less vocal, which considers scientific research to be a totally different thing. This I might call the Liberal school, as opposite to the Collectivist. According to this older school of thought, the study of science is an education, scientific research is a mental discipline, and an understanding of Nature is the first duty of man as a rational being.

In this latter aspect, the discipline of science is one of the humanities: it is the study of an ancient language, a language older than Latin or Greek or that still older language that Adam spoke: it is the language of Creation itself, the language of the Creator.

This therefore is the principal reason for studying science: it is the most direct approach to truth. And since, to quote Keats, "Beauty is truth, truth beauty," the study of science is also the most direct approach to other sorts of perfection. In this sense science is a spiritual thing, and need not be, and cannot be, justified by utilitarian arguments. It can in fact be flatly denied that it originates in material human needs and is justified only by its social services. The reason some of us study science rather than Greek is because we are thereby in the presence of greater beauty and closer to truth, not because we can make a better living out of it.

It follows that the pursuit of science is a personal and individual matter, a matter of the conscience, closely akin to a personal choice of religion, and no more to be invaded by the State, or by society, than the right of freedom of worship, which indeed it not merely resembles, but with some men of science, actually is.

But there is a curious and special feature about science, considered as a religion, which sets it apart from Buddhism or Mohammedanism. It is not merely emotionally satisfying or even intellectually stimulating; this language of Creation is so near to the truth that Creation talks back, and offers all sorts of gifts of a natural as well as of a spiritual character. The greedy worldlings

have seen these gifts and shared in them and their appetites are whetted. It is the only part of the process they *can* see: it is the only part in which they are interested, or *can* be interested. Science can be put to practical use: there will be subsidies, and men will be allowed to work only at those scientific problems assigned to them by politicians. It is like trying to buy a religion.

The practical results that flow from science are a by-product. No other known activity produces a comparable by-product. Such riches in a by-product is certainly astonishing, but a by-product it is, nonetheless. The purpose of scientific research, of itself, is a mental or spiritual product: a mental uplift, an emotional satisfaction, an artistic creation. In this it is analogous to a great musical composition or a great painting. It needs the same frame of mind, the same exceptional ability, the same deep insight, and its primary influence on others is the same. What is produced is a message: a few cents worth of pigments on a canvas, a few black marks on a musical score, a scientific paper. To most people it means nothing; to a few, perhaps, it means a great deal.

There is a difference, however. The painting is a painting. You can sell it perhaps for a high figure—or, more likely, someone a few hundred years later will sell it for a high figure. You can hang it on a wall, and make copies of it. And that is about all you can do with it.

The musical composition can be copyrighted. You can sell it. You can play it. You can make recordings of it on a phonograph disc or on a magnetized wire. And that is, again, about all.

The scientific paper you cannot sell: you may be successful in giving it away. If it can be made into a book, you may be able to sell it. You cannot hang it on a wall in an art gallery; you cannot play it, or recite it, over the radio. Generally speaking, you cannot in any direct way exhibit it to the general public. It is most unlikely that the newspapers, the movies, the radio, or any medium effectively reaching the general public will even mention it; and if they do, they are apt to put out a garbled account of it. But that doesn't matter, because the thing is *alive,* and has a fair chance of continuing alive. It is therefore subject to the laws of living things: it will change, it will grow, it will proliferate and multiply; and humanity will reap the increase.

The consciousness that there is this long-range social implication is widespread among men of science. They are aware that they are sowing, and that others, not men of science, will reap. They do not quite know what the crop will be like, whether it will be timber or grain, for the seeds are often strange, but we know they are not tares. This confidence that the seed, though strange, is good, as God found the Creation good in the Beginning, is the basis of the scientific concept of "progress." According to the men of science, the more knowledge the better; for knowledge is power, power over evil and ill-health, power over the forces of nature; and the greatest of all evils is ignorance.

But though the deep-seated belief is there, they cannot *prove* that bread cast upon the waters will return after many days. They can point out that it often has done so in the past, and will presumably do so again. But the course it follows and the currents that carry it are beyond their knowledge and may change every day. Nor can they prove that a totalitarian will not poison the bread before it returns. They only know that normally he fails to do so. But since they cannot prove these things, and men of science like to talk only about what they know and not about what they believe, they are somewhat unanimously mute on the subject, and give no reason for the faith that is in them.

This has resulted in the charge levied against them, that in the present era the men of science are the only ones that have anything to say, and the only ones that don't say anything.

An unfortunate situation then results. Since the talkers are the only ones that get listened to and those that talk from balconies and other high places are regarded by the mob below as especially oracular, false prophets are the order of the day. And one of their favorite heresies is that science is a fairy godfather, not a stern and exacting tutor; a dissipation, not a discipline; a cart-

horse to be harnessed wearily to the dull, dumb weight of a fat and indolent society, not a Pegasus coursing across the skies to do battle with the powers of darkness.

I do not pretend that every young chemist, physicist, and biologist is animated by wholly idealistic considerations, but it is certain that some of our most outstanding ones were. The greatest experimenter of them all, Michael Faraday, sought such a career because he thought it noble and idealistic, whereas the life of commerce, to which he was apprenticed, seemed sordid and trivial. To put it mildly, it would be unwise to underestimate this streak of idealism; science is the best outlet that adventurous and idealistic youth has in the present world. It would be foolish in the extreme to assume that these men are dull clods to be regimented by totalitarian-minded jurists and politicians.

From the point of view of the men of science, neither governments nor industries have shown too great an interest in science, or in men of science, except as something that can be exploited. Both governments and industries are far too largely dominated by men whose chief skill is in a battle of wits with other individuals, not in testing their brainpower against the problems of nature; their experience, as a rule, has been largely, even mainly, with strictly self-seeking individuals and groups. Perhaps this is necessarily so, and must continue to be so, for they have to deal with the whole public, and not much of the public is either altruistic or intellectual. Men of this lawyer type of mind therefore serve useful purposes in both places, but they are, with rare exceptions, automatically unfitted to deal with men of science, whose motives, so far as they are men of science, are quite different. It was said of Napoleon that he could see through any man, except an honest one, and most politicians and many "practical" men are in the same predicament, though they are not so smart as Napoleon.

But this shortcoming works both ways. If the lawyer cannot understand the man of science, neither can the scientist understand the lawyer, or that element in the population which is the lawyer's special province. Neither, as a rule, can the man of science understand the dull-witted part of the population. He may dismiss them as stupid, but more likely he dismisses them as just uninteresting: and even if they are of average intelligence, he may still find them boring. This has led to an aloofness on the part of scientists, which doesn't do them any good. The man in the street may feel that the lawyer and politician are not above exploiting him on occasion, but he tolerates them because they are good mixers. He nowhere feels that men of science are his exploiters, but he can hardly tolerate them at all, because they are superior, snobbish, and aloof. In a contest between lawyers and scientists, the man in the street will vote for the lawyers, because though they may bear careful watching, they are more comprehensible. We'd rather bear those ills we have than fly to others that we know not of. And this judgment of the average man is often right.

Now the pursuit of science for its own sake may be a noble career, but it will not of itself keep the wolf from the door. Therefore it has usually to be combined with other activities. Until very recently, I think it would be fair to say that half of England's outstanding men of science were amateurs; noblemen or lesser country gentry, who devoted themselves to science as a hobby—such men as Darwin and Lyell, for example; army officers, naval officers, and merchant venturers; and not least, parsons and clergymen. Others were professionals, in the sense that they were university professors, though some might be professors of Greek doing scientific work in their spare time; and some were true professionals, teaching and doing research in the same subject, or employed in the great museums and institutions. In our own country a not dissimilar state of affairs existed, though on a smaller scale. There were some very able amateurs, country gentlemen like Jefferson, tradesmen like Benjamin Franklin.

But today, on both sides of the Atlantic, science has been largely professionalized. There are many more people devoting their whole working day to it than could be the case unless they were paid for so doing. This of itself is not to be regretted. It is no worse than paying Congressmen or Members of Parliament for their time, a thing that was

not done till recently. It opens the field to the poor, though at the same time it lowers the average caliber of the members, or at least may tend in that direction.

The greatly increased number of research workers, remunerated research workers, comes about through the discovery by industry more particularly, and by the general public and government to a less extent, that "research pays." It pays, in their eyes, not in improved culture, in a wiser world, but in a more material fashion. It produces electric lights, telephones, and motion pictures; radio, radar, and television; automobiles, airplanes, and helicopters; X-rays, sulfa drugs, penicillin; fewer discomforts, less ill health, longer lives and more entertaining ones. The people are prepared to pay for these things, and industry is prepared to buy research as one of its raw materials, on the same basis as it buys coal, oil, or ore. It doesn't buy them as a speculation, but with the definite intent to turn them into something else and sell the product at a profit.

This sort of research is technology. As a rule, a new method, however profound and intellectually stimulating which research might disclose, of determining the distance of the moon, would not be technology, because it has no commercial possibilities. From the point of view of the Collectivists, such a research, even if successful, is a foolish waste of time, and ought to be discouraged, or even forbidden: a man clever enough to do such research ought to be doing something more useful, something to improve the lot of the poor, or to land projectiles with greater precision on enemy cities.

A research, such as is now active in many observatories, to plumb the farthest depths of the universe and try to draw a meaning from the extragalactic nebulae; an expedition to verify the prognostications of the Theory of Relativity, or, indeed, the whole Theory of Relativity itself; these are folly, and akin to crime, according to this school of thought. A scientist should have his field of investigation assigned to him by the politician: if he goes outside that field, he should be "liquidated."

Now it *is* true there is a desperate amount of short-range research that needs to be done: it *is* true that the poor are too poor, that the richest of nations are too poor. It *is* true that health is not what it should be, that nutrition is a neglected science. It *is* true that all sorts of doodads and marvels from crooning radio singers upwards can be brought into our home by television and electronics. But it is also true that a man who wants to work on the extragalactic nebulae had better be allowed to do so. He may not do any good at nutrition or be very enthusiastic about crooners. And if he isn't enthusiastic, he won't get far.

What is more, we have no means of knowing that study of the remoter stars may not, in the long run, provide the key to most important industrial and social developments. It may provide the key to our sources of power when coal has run short or is needed for more subtle purposes than burning in a furnace. There is no means of telling which researches will prove, in the end, to be the most important. A chance observation by an alert and intelligent observer following his own thoughts, and not having his thoughts prescribed for him by a politician, is apt to be more important than any set program. That is how penicillin came into the world.

Therefore there is needed a maximum of freedom to investigators, and no outside authority to say that some researches are a waste of effort. Some researches will be a waste: some scientists will exercise poor judgment. But they will, on the average, not exercise such bad judgment as outsiders.

Now the moral of all this is that the great numbers of technologists who are doing research must not be directed too closely. They must turn out enough "pot-boilers" to earn their keep, but they must be allowed to create permanent works of art also, if they can. I do not advocate coddling them, or making life too easy. Masterpieces have been produced in a garret and worse places. But any industrialist or any politician who should undertake to say in advance that a research worker must apply himself solely to a given line of thought, or directed solely to a particular end, would be both presumptuous in the extreme and an enemy of society. For the great discovery will not come to those who say "Lo, here," or "Lo, there," but as a thief in the night.

Years ago, when I first began to do re-

search work in a very amateurish fashion, I had the good fortune to be associated with another young man, a brilliant mathematician. We were working on separate problems, for the same company, and we both made the same discovery. Most often the things we worked at didn't work out: the by-products did. We decided to spend about half our time on the things the firm thought it wanted and half on anything else which was suggested to us, as a promising line of investigation, by these primary researches. I suspect we have been working on some such basis ever since, and that the lines we picked up ourselves have been socially and industrially the more important.

This discovery was not something peculiar to ourselves, nor were we the only people who ever made it. Many others have made it independently. Some of the outstanding industrial research laboratories have discovered and endorsed it. When you have a first-rate man who sees a problem that attracts him, don't try to switch him to something that looks more important to you. Leave him alone, encourage him, and leave him alone some more. "Something is bound to happen," as Dr. Whitney once told me. This policy has proved itself many times, but only with first-rate men. Not all the men who drift through our scientific schools and finish perhaps with a Ph.D. can be regarded as first-rate. They are not all Langmuirs and Steinmetzes.

Anyone who is doing really original research is of necessity a man of very high I.Q. There is, in fact, no other occupation that calls for so high a rating. It is intellectually the most gruelling course there is. It is pioneering: it is exploring. And those who wander off into the trackless wilderness, not knowing whether their obstacles will be mountains or swamps, hurricanes or doldrums, cannibals or microbes, need to be a particularly tough and resourceful lot.

In institutional or group research, as in any other group activity, a fundamental human craving comes into play. The leader must be the same sort of man as his followers. Brave men cannot be led by a coward; highly intelligent men cannot be led by one of only average talents; research workers cannot be led for long by men who have not themselves won their spurs in research. The leader of a band of soldiers must himself be a soldier; a cavalry charge cannot be led by a man who cannot keep his seat; airmen need an aviator at their head; and the principle is very generally true. A banker or promoter may be able to get money for a research organization, but if he does not understand research, including the technical aspects of it, he cannot inspire his men with confidence, still less with enthusiasm.

Robin Hood is said to have made it a rule that he would accept no followers who were not better men than himself, and this careful culling of applicants for a life in Sherwood was no doubt wise. He would otherwise have had a lot of ne'er-do-wells and weaklings on his hands. But it is a safe bet that Robin's rigid specifications and tests prove him to have been himself a very exceptional leader, and capable of directing the best men in the greenwood.

A leader is much more important than money, important though money is. How much money would have replaced Churchill in the Battle of Britain?

I have stressed some of these factors because there is still a tendency to assume that research is something that can be bought, like a drill-press or a drill-press operator. Industrialists frequently fail to understand the point, and so far as politicians are concerned the error runs clear through the first Kilgore Bill. When you can call a Churchill into existence by voting so many million dollars, then you can call true scientific research into existence by the same route: but not before.

We come back then to the same central problem: the public, the industrialist, and the politician appreciate technology but do not appreciate science as such. Probably they never will; but if they can be induced to keep their hands off, and not wring the neck of the goose that lays the golden egg, the golden eggs will continue to be forthcoming. Penning the goose up and stuffing it may produce *pâté de foie gras,* but it will not increase the egg supply.

A more favorable attitude towards freedom in science, including technological science, on the part of both industry and government is the best guarantee of progress. Industry is "sold" on chemistry, and to a

less extent on physics. The electronics of this war have pretty well "sold" it on physics by this time, and physicists are in short supply. But there is still an almost complete lack of appreciation of biology, except where it deals with microbes, and even that field is left largely to the medical, rather than the industrial, world to worry about.

I am of the opinion that any laboratories set up by individual firms, or by groups of firms, should have the broadest possible terms of reference. The country will benefit. The possibility that a research organization supported by the glass industry may discover something useful to the refractories industry, the mining industry, or even to people who are not industrialists at all, is not a calamity. Even if the plaudits should come from so remote a group as the astronomers, no disaster has occurred. The bread has floated off a long way, but it may still come back—buttered.

By giving a maximum of freedom to investigators, by encouraging them to take the widest—and perhaps the wildest—views, the greatest benefits will accrue. By going off in exactly the wrong direction, as nontechnical minds see it, the greatest discoveries are made. That is how Columbus discovered America.

THE BOWMAN COMMITTEE SPEAKING

The committee foresees that an increased measure of Federal support will raise new problems. We have, therefore, carefully considered the possibility of increasing Federal aid for scientific research without, at the same time, introducing undesirable paternalism. For, in order to be fruitful, scientific research must be free—free from the influence of pressure groups, free from the necessity of producing immediate practical results, free from dictation by any central board.

Many have been impressed by the way in which certain fields of applied science have benefited, during the war, from an increased measure of planned coordination and direction. It has thus been very natural to suppose that peacetime research would benefit equally from the application of similar methods. There are, of course, types of scientific inquiry that require planning and coordination, and a large degree of control is inevitable and proper in applied research. However, there are several reasons why pure science in peacetime cannot wisely or usefully adopt some of the procedures that have worked so well during the war. War is an enterprise that lends itself almost ideally to planning and regimentation, because immediate ends are more rigidly prescribed than is possible in other human activities. Much of the success of science during the war is an unhealthy success, won by forcing applications of science to the disruption or complete displacement of that basic activity in pure science which is essential to continuing applications. Finally, and perhaps most important of all, scientists willingly suffer during war a degree of direction and control which they would find intolerable and stultifying in times of peace.

It is the belief of this committee that increased support of research in American universities and nonprofit institutes will provide the most positive aid to science and technology. But we do not believe that any program is better than no program—that an ill-devised distribution of Federal funds will aid the growth of science. Our concrete proposals seek to augment the *quality* as well as the quantity of scientific research. We believe that there are historical precedents of Government aid to research, both in this country and abroad, which show the possibility of providing, within the framework of sound administrative practice, sustained nonpolitical grants which would operate in such a manner as to call forth from existing institutions even greater initiative, effort, and accomplishment.

The organization or instrument finally set up should not attempt to play the role of an all-seeing, all-powerful planning board trying to guide in detail the normal growth-processes of science. The first and most essential requirement is that the groups administering a program of research assistance be composed of men of the highest integrity, ability, and experience, with a thorough understanding of the problems of science. The committee believes that an independent Government body, created by the Congress, free from hampering restrictions, staffed with the ablest personnel obtainable, and empowered to give sustained and far-sighted assistance to science with assurance of continuing support, would constitute the best possible solution.

It is our belief that the desired purpose can best be served and the possible dangers minimized by centering the responsibility for this program in a new organization, a National Research Foundation, whose function should be the promotion of scientific research and of the applications of research to enhance the security and welfare of the Nation.—From *Science, The Endless Frontier. A Report to the President* by VANNEVAR BUSH. Washington, July 1945. pp. 73, 74.

THE GENES OF PLENTY

EARLY in the present century the shadow of black rust lay over the wheat fields of South Dakota. The disastrous rust epiphytotic of 1904 left a profound impression on the farm lad Edgar McFadden in this, his first year of farming. He resolved to find some means to avert the rust scourge, and to that end he sacrificed to acquire such knowledge of farm science as was offered by the state agricultural college and further extended this by assisting in the work of the South Dakota Agricultural Experiment Station. Convinced that the only prospect of gaining real relief from rust damage lay in the development of rust-resistance in the wheat plant itself, he cast about for some means of incorporating such resistance into the high quality but rust-susceptible northern wheats.

Yaroslav emmer, a feed wheat quite worthless for bread but highly resistant to the two destructive species of wheat rust, seemed to offer some promise as a source of rust-resistance, and in 1916, another of the years of devastating rust, McFadden, then an undergraduate student, delicately removed the unopened anthers of an emmer blossom and dusted the stigma of the blossom with pollen from a flower of the high-quality but rust-susceptible Marquis wheat.

Emmer has 28 somatic chromosomes and common wheat 42, so, as would be expected, such a hybridization resulted mainly in sterility or poorly developed seed. When these were planted, only a single seed developed into a wheat plant, but it was a precious plant, the descendants of which are growing today on millions of acres.

For the next nine years McFadden, mainly on the mortgaged South Dakota farm where he was striving to earn a living under the adversities of drought, hail, and rust, carefully tended the growing progeny of this single seed. There were times when the farm was neglected or when the rust which he artificially liberated to test the resistance of his hybrids escaped to the fields upon which his living depended. But by 1925 the best selection of this hybrid progeny, H49–24, had shown a consistent record of high rust-resistance, and this record was maintained in subsequent tests at 50 experiment stations in the United States and Canada. McFadden named his new wheat "Hope."

Combined with some desirable qualities, Hope had certain serious defects such as low yields, low test weight, and frost susceptibility, so that it has never become of commercial importance. But as a breeding parent, compatible with common wheats and passing its rust-resistance to hybrids with the latter, Hope has an unparalleled record in contributing to worldwide wheat production. Many of the newer American wheat varieties, such as Austin, Mercury, Merit, Mida, Newthatch, Pilot, Regent, Renown, Rival, and Vesta contain the blood of Hope or its sister selection, H–44.

The enormous contribution that Hope, through its offspring, has made to the American food supply and farm income is brought out in figures recently released by the Canadian Department of Agriculture. It was shown that in two Canadian provinces alone the development and introduction of rust-resistant wheats, principally Hope descendants, has increased the *annual* value of the wheat crop by $27,242,000, an amount more than thirteen times greater than the *total* expenditure ever made by Canada on rust research.

Wheat is but one of many crops that have been saved or made more productive through the recent development of disease-resistant varieties. The sugar beet is another. With the stimulation of the founding of the beet sugar industry in France under the aegis of Napoleon, pioneering attempts were made to establish a similar industry in America, and while some of these failed, notably that of the Mormons in Utah, successful beet sugar production began in 1870, and slowly increased.

But beginning in 1898 this promising young industry began to feel the throttling influence of a new disease of beets, curly top. The growing devastation of the curly top virus soon gravely threatened the future of beet sugar production in the western states.

Factories valued at millions were closed and began to fall into ruin as abandonment of sugar beets, a crop sorely needed for diversified farming, rapidly increased. Vast areas suitable for beet culture remained undeveloped. In the Twin Falls area of Idaho some 21,000 acres were planted with sugar beets in 1934 and nearly 19,000 of these were abandoned, largely because of curly top, while the yield from the small acreage harvested was only one third that of a healthy crop.

The beet sugar industry owes its rescue from impending extinction largely to the successful breeding and selection work of plant pathologist Eubanks Carsner and his associates in the U. S. Department of Agriculture. After many years of research the first variety of beets resistant to curly top, "U. S. No. 1," was made available to farmers and planted on 35,000 acres in 1934. This was only moderately resistant but it revitalized the beet industry of the Intermountain area. It was soon followed by progressively more resistant varieties, culminating recently in "Improved U. S. 22." The acid test came in the curly-top year 1940 when the Improved U. S. 22 yielded 16.6 tons per acre while the old types of beets were a total failure, and in a year of light curly-top, 1941, Improved U. S. 22 yielded 29.39 tons as compared with 5.16 tons for the old type beets. Today practically the entire American sugar beet acreage, which otherwise could hardly exist, is planted with the new disease-resistant varieties.

With sugar cane a similar story may be told. This crop, too, is plagued with a virus disease, mosaic, that causes losses sometimes exceeding 50 percent of the crop. So important is disease in sugar cane that disease reaction stands second only to sugar content in determining the value of a cane variety.

In 1914, before its cause was known, the mosaic disease was introduced into American cane plantings. In Louisiana, where conditions were favorable for the spread of the virus, production dropped steadily from 200,000 tons a year to 47,000 in 1926, a loss to the sugar and syrup industries of the South, due to mosaic, of $100,000,000.

But again plant pathologists and breeders were awake to the situation and even before this all-time low had been reached, they were busy at the long, slow task of incorporating mosaic resistance into high-yielding cane varieties. A world-wide search was made for the genes of resistance to mosaic. Intensive hybridization, testing, and selection have resulted in new, mosaic-resistant cane varieties that have not only re-established the Louisiana sugar industry but have even made it possible to produce more sugar at a lower cost than was possible before the mosaic disease appeared.

Disease-resistant varieties that have saved or increased production of numerous other crops are milestones along the march of science: varieties of watermelons, tomatoes, cotton, oats, beans, cucumbers, flax, indeed of nearly every commercial crop. And now, not content with developing high quality varieties that are resistant to some one major disease, the resistance-breeders are forging ahead to new achievements. Among the latest are varieties of beans that are at once resistant or tolerant to five major diseases, and varieties of cabbage that combine good adaptation, high productivity, and resistance to the deadly yellows disease, with an unusually rich content of ascorbic acid (vitamin C).

The development of these new varieties is far from being a haphazard process; it is as carefully planned and blueprinted in advance as is the construction of a complex building by a skilled architect. The new variety, to be acceptable, must have many desirable qualities. It may be necessary to search the world to find the genes bearing these qualities. When found, these may reside in varieties that contain a predominance of undesirable genes that must be eliminated from the finished product. The crossing itself may be a difficult process, as when two potential parents are sexually incompatible, requiring thousands of attempts to produce a single fertile seed, bombardment of the nucleoplasm of one of the parents with X-rays to render it more tractable, or subjecting it to the action of colchicine. Once a fertile hybrid is obtained, years of culture are required before the new variety will breed true, and these must be years in which it is subjected to all the tortures to which it may be exposed in nature. In this acid testing thousands of the hybrid progeny fall by

the way, and only a keen eye and persistent spirit can recover from the population of inadequates the rare individual which contains the desirable genes and lacks the undesirable ones of parents and grandparents.

The plant pathologist-breeder is at once the architect and the builder of the crops of tomorrow. His record is one of unparalleled achievement in the rescue and rejuvenescence of waning agricultural ventures, and his work points to far greater accomplishment in the forward march of agricultural science.—K. STARR CHESTER, Oklahoma Agr. and Mech. College.

CHARACTERISTICS OF LOCOMOTION OF WHITE BLOOD CELLS

THE motility of the white blood cells is manifest only under special circumstances. Normally, they are carried passively through the vessels by the circulating blood, in which they possess little or no active motion of their own. However, in the tissues of the organism, they are actively motile. For example, under the influence of a local stimulus, such as an infection, they leave the blood vessels and migrate into the tissues where they disperse themselves by active locomotion throughout the area of inflammation. Here they play an important role in the elimination of the injurious agent. The locomotory properties of the white blood cells may also be demonstrated by experiments in vitro. This was first done in 1846 by the English anatomist T. Wharton Jones. Since then numerous investigators have confirmed and also extended Wharton Jones' original observations.

There have been many attempts to use the locomotory behavior in vitro as an aid in identifying certain types of white blood cells, particularly the immature forms at the sites of blood formation. The white blood cells are divided into two main groups: the granulocytes, whose cytoplasm contains numerous small granules, and the agranulocytes, which have no granules. The granulocytes are subdivided into three groups: heterophile leucocytes (65–75 percent), eosinophile leucocytes (2–5 percent), and basophile leucocytes ($\frac{1}{2}$–1 percent). These three groups are distinctly separated on the basis of their nuclear form and the morphology and staining reactions of the granules. The agranulocytes include the lymphocytes (20–25 percent) and the monocytes (3–8 percent). In contrast to the granular cell forms, the differences between the latter two groups are primarily quantitative. Many cells in the blood are of an intermediate character and cannot be classified with certainty as either lymphocytes or monocytes. The matter of classification becomes even more difficult when one considers the precursors of the white blood cells. The granulocytes are formed in the bone marrow from nongranular stem cells, which closely resemble the larger lymphocytes. In fact, absolute criteria for distinguishing them in all instances from the lymphocytes are lacking. Thus, the question arises whether the stem cells in the bone marrow are identical with the lymphocytes or whether they are to be classified as separate cell forms. As mentioned above, attempts have been made to clarify these problems by studying the locomotory properties of the various blood cell types in vitro. Such work has been done on drops or thin films of blood, with or without the help of supravital staining methods. However, according to the present consensus, the motility of the cell is not a reliable diagnostic criterion in such preparations. For example, such investigations have failed to produce unanimity of opinion regarding the motility of the lymphocyte. Some workers believe this cell to be entirely nonmotile, whereas others feel that the lymphocyte can produce pseudopodia and change form but cannot migrate progressively. Still others are of the opinion that this cell can migrate very slowly. Apparently many uncontrollable factors influence the motion of the white blood cells in simple drops or in thin films of blood. In contrast to this, observations on the motility of white blood cells in tissue cultures, with which we will deal here, are more consistent. In tissue cultures the cells are suspended in a clot of blood plasma and tissue extract. This medium provides the nutrient substances and also serves as a solid substratum for the cells to move on. In such cultures it has been definitely established that all white blood cells are capable of active motion. Furthermore, it has been found that in tissue cultures different types

of white blood cells exhibit constant and characteristic types of motion. Another important technical advance was the introduction of time-lapse motion photomicrography by means of which an accurate analysis is possible of phenomena which are not accessible for study by direct observation.

The characteristics of motion of the various blood cell types can conveniently be expressed in terms of polarization of activity of the cytoplasm, as will be seen from the following descriptions. It is emphasized, for reasons to be discussed later, that these descriptions refer to the motion of the white blood cells on a flat surface.

The migration of the *lymphocytes* in tissue cultures is characteristically intermittent. Periods of locomotion alternate with nonlocomotory phases. During the locomotory phase, the lymphocyte has the general shape of a hand mirror. There is a round cell body, which contains the nucleus, and an active anterior part where the pseudopodia are formed. A small amount of cytoplasm drags behind like a tail. This indicates that the cytoplasm of a lymphocyte in motion is differentiated into an active anterior part and a passive posterior part. In other words, the cell is polarized. The strict polarization of lymphocytes manifests itself also in their pathway of migration, which has the appearance of a wavy line with smooth curves. Abrupt changes of direction rarely occur. During this locomotory phase, lymphocytes have been found to move at a rate of 33.09 ± 0.78 micra per minute. Such high rates of locomotion have previously been thought to occur only among heterophile leucocytes. In the nonlocomotory phase the cell changes its form. The anterior pseudopodial area and the "tail" are withdrawn, and the cell, which was previously shaped like a hand mirror, assumes a rounded contour. However, this nonlocomotory phase is not merely a "period of rest." Small pseudopodia continually arise from this rounded cell body; the pseudopodia are extruded, not from a limited area, as in the locomotory phase, but from all sides of the cell body. Since in this state the cytoplasm of the lymphocyte is not differentiated into an active anterior part and a passive posterior part, this phase has been termed "depolarized." No progressive locomotion takes place during this depolarized phase, but the cell moves irregularly about over short distances which rarely exceed the width of the cell.

The direction of the motion of the lymphocyte in the locomotion phase seems not to be related to an external stimulus. In any given area of the culture, the directions of the movements of the cells are completely haphazard, frequently crossing one another's path or moving in exactly the opposite direction. After a depolarized phase, migration may proceed in any direction and the new pathway of migration may form a sharp angle with the previous one.

The *heterophile leucocyte* in motion has essentially the same shape as the lymphocyte, but characteristically is not as strictly polarized as the latter. Although the cytoplasm of these cells in motion is differentiated into an active anterior and a passive posterior part, these areas are not as distinct from one another as in the lymphocytes. The pseudopodia are not limited exclusively to the anterior area of the cell, but often appear at its sides. The tail is relatively smaller and not as constantly present as in the lymphocytes, but is frequently withdrawn so that the cell body assumes a blunt end. The result is that the hand mirror form, although fundamentally present, is less pronounced than in the lymphocytes. The less rigid polarization of the cellular activity is also shown in the pathway of migration. Sudden changes of direction, and even complete reversals occur frequently so that the pathway of migration has many sharp angles.

The *macrophage* is a close relative of the blood cells. This highly phagocytic cell is present in most of the tissues of the body. it has been shown, however, that in inflammatory processes and also in tissue cultures, macrophages can develop from agranular blood cells. In contrast to the above discussed agranular cell types, the macrophage has no progressive locomotion to speak of. The cytoplasm of these cells is continually depolarized. Pseudopodia arise from all sides of the cell body and are subsequently withdrawn. During this process, the cell moves irregularly about and changes its position very slowly, covering distances of about 15 micra or less in one hour.

These types of motion may all be classified as random movements. Even the motion of the lymphocyte, which in the locomotory phase has a directed character, is a random movement, since after a depolarized phase the motion may proceed in any direction.

In order to move, the cells require a solid substratum. If they are suspended in a liquid, no locomotion is possible since white blood cells do not swim but crawl on a solid substratum. This may account for some of the divergence of opinion on the motion of leucocytes in drops or films of blood. In a tissue culture the substratum can be either the flat surface of the glass coverslip on which the culture is made or the fibrin network of the plasma clot. The above-described movements are seen only when the cells are adhering to a flat surface. Within the plasma clot their motion is entirely different. Here the lymphocytes as well as the heterophile leucocytes have an elongated cylindrical form and move in a twisting, writhing manner, which has been termed "wormlike." The nature of the fibrin substratum forces the cell to assume this elongated shape, while the movement around the fibrin threads through the spaces available in the three-dimensional network determines the wormlike character of the motion. The anterior pseudopodial area and the passive posterior part are still present, but are less conspicuous owing to the narrowing of the cell body. The complexity of the fibrin network makes it unsuitable as a substratum for studies of locomotion.

The fact that different types of blood cells have characteristic types of motion has been applied to one of the most fundamental problems in hematology: the nature of the blood stem cells in the bone marrow. According to one group of investigators, these cells, from which the granulocytes and also the red blood cells arise, are identical with the lymphocytes, which develop from pre-existing lymphocytes in the lymphatic tissue. This view, that the bone marrow cells and the lymphoid cells have a common stem cell, is the basis of the "unitarian theory" of blood formation. Other workers uphold the "dualistic theory," that the bone marrow stem cell is a specific cell type and that both cell lineages have separate stem cells called, respectively, myeloblast and lymphoblast. A great many histological and cytological techniques have been applied in order to find sufficient morphological differences to justify such a separate classification of the two stem cells. Studies on the motion of these cell types on a flat surface have shown that the locomotion of the myeloblast, as well as that of the lymphoblast, is identical with the motion of the lymphocyte. This supports the "unitarian" rather than the "dualistic" point of view.

The changes in locomotion which occur during the development of stem cells into mature blood cells have also been investigated. In the development of heterophile leucocytes from bone marrow stem cells, only relatively minor changes in locomotion occur. As indicated above, the motion of heterophile leucocytes differs from that of the lymphocytes only in its degree of polarization. Since the movement of the bone marrow stem cell is identical with that of the lymphocyte, the intermediate cell-forms merely have an intermediate degree of polarization. The red blood cells and their immediate precursors do not exhibit any active locomotion. However, the cytoplasm of very young red blood corpuscles with only a small amount of hemoglobin (polychromatophile erythroblasts) shows some activity. The nuclei of these precursor cells have a reversible rotating movement through a short arc, while the nuclei of the cells containing more hemoglobin do not show this phenomenon, but are completely quiet.

More striking than the changes in locomotion during the development of blood cells are the changes in locomotion which accompany the transformation of lymphocytes into macrophages, which occurs in tissue cultures and also in inflammation. The fundamental morphological change in this transformation is a gradual hypertrophy of the lymphocyte. Physiologically, a change in type of motion and also the acquisition of phagocytic properties are outstanding. In this transformation, intermediate cell forms occur which have the same intermittent motion as the lymphocytes. During the locomotory phase their motion is identical with that of the lymphocytes, while in the depolarized phase, it progressively assumes the characteristics

of macrophage motion. During the later stages of this transformation, the locomotory phase disappears, and the continuously depolarized motion characteristic of macrophages remains. It is interesting to note that the movement of the lymphocyte in the depolarized phase, although superficially greatly different, is fundamentally identical with the motion of the macrophage. Both movements are characterized by their depolarized nature and the lack of progressive motion, the difference between them being primarily quantitative. The macrophage possesses absolutely and relatively more cytoplasm and extrudes more and larger pseudopodia than the lymphocyte in the depolarized phase.

This work on the movement of blood cells may be considered a result of the growing trend in histology toward the study of the physiological behavior of cells by observing them in the living state. The inaccessibility to observation of the cells, in their normal environment of the tissue, has led to the application of the tissue-culture technique. There is no doubt that the conditions in tissue culture are different from those in the living tissue. One has but to point to the artificial nature of the substratum, which serves the cell as a support in the culture, to realize that all the results thus obtained cannot be assumed for cells in their natural environment. However, the fact that differences in type of motility occur in tissue cultures may suggest that such differences, although perhaps of a different nature, also exist in vivo.—P. P. H. De Bruyn, Department of Anatomy, University of Chicago.

ARMY UNIVERSITY CENTERS IN EUROPE AND THE SCIENCES

The shift in the enlistment of science from that for physical destruction to that of mental restoration was put under way by the establishment of the Army University Centers in Europe. One opened in Shrivenham, near Oxford, England, to about four thousand soldiers of prewar university caliber in late July; a second one had its formal opening in France in mid-August under the name of Biarritz American University; and a third is in action at Florence, Italy.

It is unique that these University Centers should come into full-being within a few months, and each with a faculty of over two hundred, of which about 30 percent are teaching scientific subjects to almost six hundred students, as at Biarritz, for example. When 65 percent of the faculty are drawn from civilian life in universities and colleges, when more than 50 percent have earned the doctorate, and when all are above the age of forty-two, such experience and abilities of the faculty assure successful science teaching in this new institution. This faculty—obtained by selection and not by application, or even political appointment—is offering almost one hundred different courses in the sciences. Classes are in sections of few enough students to facilitate student-teacher contacts and free exchanges of ideas.

The students, too, are selected with reference to their educational backgrounds for university work. Their attitudes are excellent, their minds open, their diligence beyond expectation, and already their shifts from wartime to peacetime thinking are evident. They have each enrolled for no more than fifteen classroom hours per week. They indicate a distinct mental maturity in consequence of their time in the Army. For them the war experience has also been an education, and they are now transferring the better values from it to this classroom venture in areas where recently the war raged. Though after eight weeks they go to parts of Europe to be replaced in each center by another group, they are after all equipping themselves to go back into competition with the society at home.

Shortages of apparatus, crowded and unfinished classrooms, insufficient reference books in the library, and other similar material handicaps at the outset, or even personal discomforts, are not stopping the march of this education for peace. Here is democracy on its way back, as captain and private match wits in their discussions of lessons in science. —Wm. A. Albrecht, Biarritz American University, France.

BOOK REVIEWS

SCIENCE THROUGH THE AGES

The Autobiography of Science. Edited by Forest Ray Moulton and Justus J. Schifferes. 666 pages. 1945. $4.00. Doubleday, Doran and Company, Inc., New York.

THE purchaser of this interesting volume will be the wiser, first, from studying it as an anthology of not only fine but highly significant literature and, second, from reading its preface carefully.

The chief problem in an anthology is that of selection. Dr. Moulton and Mr. Schifferes have done well. Their original outline contained some three hundred names with material enough to fill a dozen books. The difficulty of meeting their standards is implied. Too many conflicting conditions had to be met. It was desired to offer the most important contributions in science. At the same time it was realized that much that is scientifically significant is beyond the capacity of the general reader for whom the volume is devised.

About ten dozen authors and sources are considered in varying detail from a few lines, as in the case of D. I. Mendelyeev (1834–1907), to twenty or more pages, as with G. Galilei (1564–1642). Choice was made on the basis of readability as well as from the standpoint of scientific significance. The result is uneven, as far as scientific interest is concerned. Many important scientific contributions which would occur promptly to any historian of science are omitted.

The editors remark on the difficulties of choosing representative passages from great scientific contributors. Their dilemma is pleasantly illustrated by reference to their consideration of von Helmholtz (1821–94). Choice was influenced by their desire to refer the relationships of scientific activity to other interests of cultured people. Significant scientific contributions were thus sacrificed for more general philosophical or literary efforts expressing the scientific spirit.

While the editors realized the difficulties of making a representative selection of significant scientific contributions, they apparently failed to realize the significance of making such a selection at all, particularly with respect to scientists still living. An anthology is the first step in the establishment of a canon. Unsophisticated readers get the impression that what is included in the selection is the only material in the field that is worthwhile, and that whatever is omitted is not worth including. The result is that canonical tendencies develop and unfortunate consequences, wholly foreign to the spirit of science, appear. So many factors are operating today to establish a "scientific canon," disagreement with which is heresy, that every care is justified to prevent such an undesirable possibility.

The material in the volume is arranged chronologically. It also deals exclusively with the development of what is termed "Western science." This is an unfortunate distinction to attempt to draw. The science of antiquity is covered in thirty-three pages: from the speculations of Genesis to Galen (A.D. 130–201) and including extracts from Egyptian medical papyri, Hippocrates (460–377 B.C.), Aristotle (384–322 B.C.), Archimedes (287–212 B.C.), Lucretius (98–55 B.C.), Pliny (A.D. 23–79), and the great Roman architect Vitruvius.

The sleeping period of science includes matêrial from Roger Bacon (1214–94), Guy de Chauliac (1300–1370), and the famous "Regimen of Salerno." The reawakening of science in the Renaissance covers some seventy pages and offers extracts from Leonardo da Vinci (1452–1519) to William Gilbert (1540–1603).

The increasing accumulation of scientific activity is shown by the devotion of nearly a hundred pages to the growth of science in the seventeenth and early eighteenth centuries, from Francis Bacon (1561–1626) to Carolus Linnaeus (1707–78). The maturity of science is illustrated by abstracts from publications during the revolutionary period of the eighteenth and early nineteenth centuries, proceeding from Joseph Black (1728–99) to Edward Jenner (1749–1823). The progress of science in the early nineteenth century includes reference to the brilliant

chemists of the period and extends from Michael Faraday (1791–1867) to Charles Lyell (1797–1875).

It takes a hundred and twenty pages to offer selections from the assertions of science in the age of evolution from Charles Darwin (1809–82) to William Thomson (1824–1907). And then it takes nearly two hundred pages to offer representative contributions from twentieth century scientists from Samuel P. Langley (1934–1906) to Donald C. Peattie (1898–). The volume concludes with a brief but uncritical bibliography in the history of science, acknowledgments, and a topic and name index.

The acceleration of scientific activity is clearly evident from this brief summary of the volume. Pertinent to the editors' hope that the volume will introduce many readers to the history of science are well-prepared but brief biographical sketches of the many scientists whose writings are quoted.

For satisfactory orientation in the development of science it might have been wise, however, to have included further reference to many of the great scientists which the editors felt called upon to omit. Here the problem of choice and discrimination enters. Might it not have been worthwhile to have used one instead of three quotations from Francis Bacon in order to include a single short quotation from, say, Marcello Malpighi (1628–94), who did so much to establish modern botany and zoology? One quotation from Gilbert White (1720–93) might have been enough, so that a single one of the short brilliant demonstrations of Euclid might have been added in an earlier section. Perhaps a single quotation from Darwin, Galton, or Florence Nightingale might have been enough, so that something might have been said of Virchow, Claude Bernard, Koch, or Ehrlich.

The volume seems weak in quotations from mathematicians and astronomers. Perhaps these were omitted because they are frequently so hard to understand. The editors say they were guided chiefly by two criteria: first, importance as "turning points" in the history of science, and, second, readability. But still it might have been wise to include at least a short note from Kepler, Brahe, Laplace, Chamberlin, the Herschels, Euler, Gauss, or even Cardan. Certainly it might have been wise to have quoted something from George Sarton on the humanizing of science. This might have been a little more scientifically appropriate than even the very splendid (and brief!) quotations from Donald Peattie's "Almanac for Moderns."

It is clear that the editors have been attracted more by the readability of scientists than by the strict intellectual significance of scientific contributions. The Egyptian medical papyri do contain genuine contributions to scientific progress, in addition to an "advertisement for a remedy." Hippocrates on "Fractures" or the "Sacred Disease" is probably more significant scientifically than the Aphorisms or the Oath. Valerius Cordus (1515–1544) is more significant, but duller, than Paracelsus (1493–1541) on the transition of alchemy to chemistry. Paracelsus was, however, a pioneer in psychiatry and industrial disease. Cushing on the pituitary is probably more important scientifically, but less entertaining, than he is on the facetious career of the surgeon. One of the passages of Maeterlinck, however lovely, or one of the quotations from Freud, however interesting, might have been left out so as to include a paragraph of Joseph Barcroft, or Gilbert Lewis, or Ramon Cajal, or I. P. Pavlov. Certainly Sir William Osler would have been happy to yield a page to Robert Boyle!

The editors say there are six ways to read the book: as a storybook, a history book, a textbook, a reference book, a source book, or as a chronicle. It seems to me to have best use as a reference book. But from this standpoint it is appallingly incomplete. It is also incomplete as a source book. It is really not a chronicle. As a textbook it is reminiscent of the literature anthologies which are used in some of the more progressive preparatory schools. It can hardly be called a history book. As a storybook, however, it is delightful. It is simply a popularization of some of the better known Western contributions to scientific literature.

Nothing can better illustrate the success of the editors than to quote their own appreciation: "Many of the passages in this anthology partake of the highest qualities of

literary style. Present-day 'third person,' 'passive voice' fashion in scientific communication notwithstanding, the science masters of the ages, speaking in the first person, unequivocally illustrate that 'the literature of power' and 'the literature of knowledge'—De Quincey's phrases—can be one. Good science makes good reading."

The hope of the editors will be thoroughly fulfilled: "that the book will introduce many readers to the history of science and orient them in it; that it will give them a first-hand, original account of the substance of great scientific thinking; that it will suggest the sweep, scope, and international brilliance of scientific development; that it will give them a basis for discriminating between true and pseudo-science, and finally, best of all, that it will provide an accurate, intimate, living knowledge of the great men and women of science, some insight into their characters, some sense of the true nature of their work." The volume is an excellent effort to offer the best in scientific literature and it is certainly to be hoped that the book will arouse wide interest and appreciation.

CHAUNCEY D. LEAKE
UNIVERSITY OF TEXAS SCHOOL OF MEDICINE
GALVESTON, TEXAS

INDIVIDUALITY IN THE MAKING

The Biological Basis of Individuality. Leo Loeb. 711 pp. 1944. $10.50. C. C. Thomas, Springfield, Illinois.

IN this masterly volume, the fruit of a lifetime of biological experimentation and reflection, Professor Loeb considers the nature of individuality from the standpoint of the biologist. For much of the basic exposition of his findings Professor Loeb relies upon the experimental work on transplantation of tissues carried out in his own laboratory by himself and his collaborators. Admirers of Professor Loeb will be particularly glad of this since here for the first time is performed something of a synthesis of a lifetime's labors. This is not to say that tissue transplantation is the sole experimental work relied upon or that the work of others is neglected: far from it. The range of subjects and material is very broad indeed, and there are few biologists who would be as competent to deal with such a vast range of material as is Professor Loeb. Transplantation experiments, serology, immunology, oncology, embryology, morphology, genetics, chemistry, theoretical biology, and finally psychology and sociology, are all so much very relevant grist to Professor Loeb's mill.

In a short review it is quite impossible to do justice to this remarkable work, a work which is bound to increase in usefulness and stature with the passage of time. For the scientist of today, no matter what field he may be interested in, and for the philosopher, the present volume should become indispensable reading. It is full of the most novel insights and generative of others. Its greatest usefulness will, doubtless, be in the rewarding field of experimental biology, but workers in such seemingly distant fields as the psychological and social sciences will benefit considerably from a study of this work, and I would here especially draw their attention to Part VIII which deals with "The Psychical-Social Individuality." Such workers will, at the very least, find this part stimulating, for Professor Loeb's views are strictly his own, and this is all to the good. Not altogether justly we may paraphrase Hobbes and say, had Professor Loeb read as widely in the social and mental sciences as other men have done he would have been as ignorant as they are. As it is he brings a fresh and original viewpoint to bear upon the analysis of the somewhat tergiversating problem of human individuality. As Professor Loeb very properly points out in the second chapter of this part "the problems of philosophy have been largely concerned with the meaning of individuality" (p. 648).

If there is one criticism I would make of this last part of the work it is that Professor Loeb does not sufficiently jettison the old mind-body dichotomy. Though he obviously does not subscribe to that way of thinking he still writes of the body *and* the mind. It would do us all a great deal of good if we could give these two confusing terms a long vacation.

I should like to conclude this very inadequate notice of a most important book with the paragraph which concludes Professor Loeb's final chapter.

"Thus, in matters which relate to man as a

psychical-social organism, it is the environment which has become a preponderating influence and which largely determines his fate. To adapt the psychical-social environment to the needs of man, so that he can function in the most adequate manner, is, therefore, the most important task which humanity has now to face."

M. F. ASHLEY MONTAGU
THE HAHNEMANN MEDICAL COLLEGE
PHILADELPHIA, PA.

HERALDING AN AGE OF AGE

New Goals for Old Age. Edited by George Lawton. 210 pp. 1943. $2.75. Columbia University Press.

You Are Younger Than You Think. Martin Gumpert. 244 pp. $2.75. 1944. Duell, Sloan and Pearce, Inc., New York.

Ageing and Degenerative Diseases, Vol. XI, Biological Symposia. Various Authors. 250 pp. Illus. 1945. $3.00. The Jaques Cattell Press, Lancaster, Pa.

TIME waits for no man. We have but two alternatives: To grow old or to die young. More and more of us are being given the opportunity to choose the former course. Science has enhanced human longevity to the point where even those not yet old themselves are becoming aware of the significance of senescence and of the immensely complex and urgent problems of aging. Ultimate solution of these problems is still far off. It will involve application of almost every known scientific discipline, for aging is a part of living and life is a complicated enigma. There are so many facets to aging that the thoughtful efforts of all those concerned with the advance of knowledge will be required for the full development of the latent potentialities of later maturity.

The fact that three recent books dealing with different aspects of senescence are being reviewed together emphasizes both the growing interest in this field and its broad scope, for each attacks the subject from a different approach. Two of the volumes are symposia; the third a popular type of presentation of what aging means to the individual.

In *New Goals For Old Age,* Lawton has collected and organized a splendid group of deeply sensitive discussions of the major psychological problems of senescence. The papers were originally delivered in a course, "Mental Health in Old Age," conducted under the auspices of the Section on the Care of the Aged of the Welfare Council of New York City. The lectures were planned to be as practical as possible. Lawton's careful and inspired editing has brought the divergent viewpoints together to create a delightful and decidedly thought-provoking book. Worthy of special emphasis, because of their exceptional merit, are the discussions of Lewellys F. Barker on "Physical Changes in Old Age and Their Effects upon Mental Attitudes," Lawrence K. Frank's on "The Older Person in the Changing Social Scene," and George Lawton's fundamental consideration of "Aging Mental Abilities and their Preservation." Throughout, the book is scientifically accurate and fully documented but it is also so palatable and digestible that there is sheer pleasure as well as profit in study thereof.

The second volume of the three is a monograph intended for "popular" consumption, and as such is an excellent résumé of most of the major problems likely to beset us as we pass from later maturity into final senility. Doctor Gumpert divides his material into four major divisions: (1) The Biology of Aging; (2) Normal Old Age; (3) Social Problems of Aging; and (4) Diseases in Old Age. The first of these is very ably presented. The last section, dealing with the frequent and disabling diseases of later years, suffers from unfortunate oversimplification. Certainly a description of the mechanisms, significance, outlook, and logic of therapy of hypertensive disease in two pages or a discussion of prostatic disorders in three small pages requires omission of much that is pertinent and significant. Such excessive condensation may confuse rather than enlighten the previously wholly uninformed layman. A little knowledge can be dangerous even though that be accurate. Doctor Gumpert is to be congratulated upon the splendid accuracy of his presentation, though throughout the text there runs the same overly optimistic vein which is so apparent in his title. It is all very well to try to make senescence attractive, but people will still dislike and fear the thought of growing old. Sharp realism, with equal emphasis of

the liabilities of aging and the assets of continued maturation is what we need even if it isn't what we want.

The Series of Biological Symposia are surely well known to the great majority of readers of THE SCIENTIFIC MONTHLY. The eleventh volume of the series, *Ageing and Degenerative Diseases,* fresh off the presses of Jaques Cattell, contains the papers presented at a symposium in St. Louis, March 24 and 25, 1944, held under the joint sponsorship of the Research Unit of the St. Louis City Infirmary and the Washington University School of Medicine. The ramifications of gerontology and geriatrics are so extensive that only a few subjects could be considered in the two-day program; thirteen papers and a round-table discussion on aging and the degenerative disorders so commonly associated with senescence from the divergent viewpoints of experimental morphology, biochemistry, nutrition, endocrinology, and clinical medicine. We shall not single out any one of these contributions for particular comment; all are noteworthy for original, sound, scientific thinking. The volume contributes significantly to the advancement of the science of gerontology. It deserves close study by everyone interested in aging.

EDWARD J. STIEGLITZ, M.D.
WASHINGTON, D. C.

A SCIENTIST LOOKS AT FREEDOM

Freedom Is More Than a Word. Marshall Field. 190 pp. 1945. $2.50. University of Chicago Press.

To the man of science, freedom is something more than a desirable and gratifying attainment; it is an absolute necessity. Only in an atmosphere of freedom can science flourish. Without its invigorating presence, the scientific method of experiment, exploration, and adventure inevitably fails. No wonder that many scientists are at this moment displaying such deep concern in the sociopolitical trends of the day. Beyond question, all scientists have a tremendous stake in the plans for freedom that must be put into effect if science is to continue to flourish when the political state necessarily becomes the social service state. This concise, closely printed, slender volume may well be recommended as helpful reading to any man of science who recognizes the fact that for him as scientist, even as for him as citizen, freedom must be more than a word.

In spite of its dimensions, this is really two books in one. First, there is a dissertation on "the proper relationship of the individual to the state and of the state to the individual." The reader who is familiar with the extensive literature in this field, or even those who have read only two or three of the score of books published in the last three or four years that touch upon it, will find little that is new. Many of the phrases will be recognized as old friends. It is, however, an excellent presentation of a social philosophy that appeals strongly to every mind habituated to scientific ways of thought. Never trite, seldom superficial, its ideas move swiftly, logically forward from a description of a society of free men to an analysis of freedom of expression and a critique of the obstacles to its universal development.

This part of the book could have been written by any one of a thousand intelligent, thoughtful surveyors of the American social scene. Its significance stems not so much from what is said as from who it is that says it. Marshall Field is the grandson of the "merchant prince" of the same name who made his fortune in Chicago and London. Perhaps my appraisal of the book's significance is colored by the fact that when I was an impecunious college student I worked for a time as a shoe salesman in the basement of his Chicago store. In any event, it is something of a phenomenon when the scion of a wealthy family, educated at Eton and Cambridge, reveals a social philosophy so completely antithetical to that of the "Old School Tie" and the "playboy." Evidently the "American dream" has vitality and power at the top as well as at the bottom of the social ladder.

The second part of the book is more specific. Only Marshall Field himself could have written it, but here the reader's interest is riveted to the printed pages; he does not care who the author is, nor what his background may have been. It is an account of the objectives and vicissitudes of Mr. Field's two well-known adventures in the newspaper

world, *PM* in New York and the *Sun* in Chicago. The inside story of these efforts to develop freedom of expression in a field restricted by intrenched monopoly is in very truth an epic of our times. It includes, of course, an account of the long and complicated legal battle with the Associated Press, but it was written too soon to end with the final note of triumph that was sounded on June 18, 1945, when the U. S. Supreme Court sustained the injunction, issued by a federal court in New York City last year, ordering the news agency to abandon a by-law allowing competitors to influence the election of new members. At last it is established that the government has an obligation under the First Amendment not merely to refrain from interfering with the freedom of the press but also to protect it actively from private restraints.

Realizing as every scientist should, that the freedom to undertake research is inextricably interwoven with all other aspects of the life of freedom, that freedom of expression is just as priceless a treasure for the investigator in the laboratory as for the reporter in the newspaper office, we may well be grateful to Marshall Field for his valiant battle and for this clear-cut exposition of his philosophy and experience.

KIRTLEY F. MATHER
DEPARTMENT OF GEOLOGY
HARVARD UNIVERSITY

AMERICA THROUGH FRENCH EYES

Democracy in America. Alexis de Tocqueville (Phillips Bradley, Ed.). 2 Vols. 973 pp. 1945. $6.00. Alfred A. Knopf, New York.

IN May 1831 a twenty-six-year-old Frenchman and a congenial companion, Gustave de Beaumont, landed in New York City. Ostensibly they had come to make a study of American prisons for the French Minister of the Interior. In nine months they traveled 7000 miles observing, inquiring, and recording a vast quantity of notes. Out of these data was to emerge *Democracy in America,* "perhaps the greatest work ever written on one country by a citizen of another," as Harold Laski put it in an introduction to the edition under review. Incidentally, contemporary Americans were gratified over a European visitor who come seeking to understand rather than to ridicule.

The tumult of the July Revolution in France had only recently subsided when Tocqueville arrived here in the midst of what American historians have since termed the Jacksonian Revolution. The prolonged depression of the 1830's had served to accelerate a developing social ferment in the American masses. Convinced that the old ruling class had betrayed their interests, the people seized the reins of government and converted the theory of popular sovereignty into an established fact. Tocqueville had dropped into the midst of the bustling generation that was planting in young Abraham Lincoln's mind the enduring ideology eventually to be epitomized in the classic phrases of the Gettysburg address. "The American people reign in the American political world as the Deity in the universe," observed Tocqueville. "They are the cause and aim of all things; everything comes from them and everything is absorbed in them."

American Democracy falls in the tradition of Montesquieu, the philosopher of causation, who persistently sought among social conditions and forces the mainsprings of political action. Fearful of the glorious cloud land of the a priori, Tocqueville endeavored to keep his feet on the solid earth by reveling in the copious data he had gathered. Whatever its shortcomings may be, he produced in the 1830's what at the end of the century was pronounced by a competent authority "the best philosophical discussion of democracy using the United States as an example that can be found in any language."

The key to this commentary centers in Tocqueville's belief, as expressed so well in the preface to the twelfth edition, "that the advent of democracy as a governing power in world affairs, universal and irresistible, is at hand." He had come to study the world's greatest functioning experiment in democracy, hoping to discover a universal guide by which every people could achieve the advantages of such a way of life and yet avoid its defects. So what would happen to Europe and particularly to his own France when this phenomenon crossed the Atlantic was never out of his mind. He succeeded in producing

what Phillips Bradley, the editor of the present edition, considers "a litmus paper by which to test the process of change from aristocratic to democratic institutions."

The reader can get some idea of the scope of Tocqueville's curiosity when informed that he dealt with American geography, social conditions, local and state government, judicial power, the Federal Constitution, parties, civil liberties, unlimited powers of majorities and the checks on their tyranny, the future of democracy, and its influence on the intellect, feelings, manners and on political society.

This French savant was struck with the fact that the United States provided the world's first opportunity to know intimately the infancy of a nation. Americans had somehow hit upon the corrective for centralization of administration by the participation of the people in the process of government, voting for public officials, and creating and implementing public policies, all of which, incidentally, constituted a potent educative force. Though not especially religious himself, he pleaded for the maintenance of faith as man's most powerful dynamic. "I should not hesitate to decide" he declared, "that the community would run less risk of being brutalized by believing that the soul of man will pass into the carcass of a hog than by believing that man is nothing at all."

Who would have expected in the infancy of the industrial revolution and a quarter of a century before the publication of *Das Kapital* such a clairvoyant flash as this? "The manufacturing aristocracy of our age first impoverishes and debases the men who serve it and then abandons them to be supported by the charity of the public." Here is no propaganda based on a petrified ideology but the mature judgment of one whose parents perished by the guillotine in the Reign of Terror. How refreshing to find a century-old faith in democracy as the ultimate way of life for all peoples and the one sure answer to totalitarian materialism.

Whoever would decide to cancel out of this political classic all that the course of a century has outmoded might be astonished at how little he had discarded. American readers owe thanks to Mr. Knopf for making again available a work so long out of print. Typographically it has been executed in the manner book lovers have come to expect in a Borzoi book. Phillips Bradley is to be congratulated on the diligence and care with which this new edition has been edited.

WILFRED E. BINKLEY
DEPARTMENT OF POLITICAL SCIENCE
OHIO NORTHERN UNIVERSITY

WHAT IS LIFE?

What Is Life. Erwin Schrödinger. 91 pp. Illus. 1945. $1.75. Cambridge University Press, London.

"WHAT IS LIFE?" is an old and a perennial question. In this little volume an able physicist tries to advance the factual answer to the question by illuminating living matter by the light of the known physics and the known chemistry of dead matter. The results are both instructive and thought-provoking. But the volume brings out little that is fundamentally new to the present generation of biologists. The workers in functional biology have in the last fifty years added significant support to the working hypotheses that the laws of physics and chemistry apply to living matter. However, Professor Schrödinger's little book is intended for laymen. It is, indeed, an aid to adult education. But, as with all real education, even educated laymen will get little from Schrödinger's essay by reading it as a "pastime," for it stands at the opposite pole from the current "spawn of the press and the gossip of the hour." On the first 86 pages (chapters 1–7) Dr. Schrödinger is clearly a physicist and a philosopher disciplined in science, with a good grasp and understanding of both structural and dynamic biology, even though some of his statements or presentations of known biological facts and relations assume knowledge and understanding not yet at hand or concepts less complex than the known facts warrant. Nevertheless, the informed reader is not a little startled to find in the *Epilogue* (pp. 87–91): "On Determinism and Free Will," that Dr. Schrödinger, like some other fellow physicists, becomes an out-and-out religious mystic. I quote: "Let us see whether we cannot draw the correct, non-contradictory conclusions from the following two premises:

(1) "My body functions as a pure mechanism according to the laws of nature.

(2) "Yet I know, by incontrovertible direct experience, that I am directing its (the body's) motions, of which I foresee the effects.

"The only possible inference from these two facts is, I think, that I am the person, if any, who controls the motions of the atoms according to the laws of nature. . . . Please consider whether the above inference is not the closest a biologist can get to proving God and immortality at one stroke. . . . I have become God."

To this reviewer, and I think to most informed biologists, the second premise is obscure, naive, incomplete, and hence essentially untrue. Irrespective of logic, the conclusion drawn is a *non sequitur* in our known biology of man.

A. J. Carlson
Department of Physiology
University of Chicago

MAMMALS OF THE PACIFIC ISLANDS

Mammals of the Pacific World. T. D. Carter, J. E. Hill, and G. H. H. Tate. 227 pp. Illus. 1945. $3.00. The Macmillan Co., New York.

When the war focussed American attention on the Pacific Islands, professional biologists and scientists in other fields as well had reason to be chagrined at the dearth of informative books on the plants and animals, races of mankind, and general geography of the region. A flood of letters and telephone calls came to museums and libraries and university departments inquiring for references on the natural history of the Solomon Islands, or of New Guinea, or of the South Seas in general. It was disappointing to have to reply that the literature in question is scattered through hundreds of journals, is mostly too technical for anyone but a specialist, and that no syntheses of such information, suitable for the general reader, had been made.

Regional handbooks of the sort desired are a by-product of descriptive botany and zoology, and these departments of biology have suffered from two generations of neglect in American colleges and universities. It is unfortunate that the general public, and a considerable section of the scientific public, does not appreciate the fact that except for such conspicuous creatures as birds and butterflies the exploration of the world for plants and animals is far from completed. The project for an adequate description of the life of the world is a noble one, and one of far-reaching importance to education on one hand and to the progress of research in the related fields of biology on the other.

The Pacific World Series took shape in answer to the specific need for information for the men in the armed forces in their suddenly novel surroundings, and to the equally legitimate questions of their relatives and friends at home. The reviewer justifies a somewhat parental interest in this series by his modest share in the preparation of the introductory volume, *The Pacific World.* It is gratifying to have the successive volumes available, though the course of the war has left many of the islands in a backwash. Perhaps the bored GI stranded on such a forgotten island may need them more than ever.

The present volume, the third to appear out of the projected seven, deals with the mammals in systematic order, beginning with the egg-laying duck-bill and spiny anteater of Australia and ending with the tapirs (the monkeys and apes, according to modern arrangement, appear between the bats and the flesheaters). An introductory chapter explains the system of scientific naming and gives a general idea of how mammals are classified. The chapters on conservation and on the distribution of mammals in the Pacific are especially useful since the Pacific area includes the extremely distinct mammalian faunas of Malaysia, Australia and New Guinea, New Zealand, and the more eastern multitude of smaller islands in Melanesia, Micronesia, and Polynesia. A completely nonzoological GI might bog down in a complicated group like the bats, but will find identification enormously simplified by reference to the concluding section, which gives lists of the mammals known from each island or island group.

Karl P. Schmidt
Chicago Natural History Museum
Chicago, Illinois

COMMENTS AND CRITICISMS

ON THE WELTFISHIAN OATH

The Scientific Spirit and Human Prejudice

In the September SCIENTIFIC MONTHLY under the title "Science and Prejudice" an anthropologist called the scientist to task for lack of social responsibility. Miss Weltfish's article is commendable for its recognition of personal responsibility for world order, and it has the further merit of making a simple, specific suggestion. But is her evaluation of the scientist and his position in society just, and is her suggestion practicable?

Miss Weltfish illustrates her point by a lengthy description of the activities and personal animosities of anthropologists of seventy-five years ago. This is an odd example to choose because the threat of destruction which we now contemplate does not stem from the researches of the anthropologist; moreover, his methods and principles are not necessarily representative of those of the physical sciences because his is a very young discipline. It would have been more pertinent to consider the social consciousness of a physicist or a chemist. Miss Weltfish might have asked, for example, whether Nobel, when he discovered how to make nitroglycerine safe for the user by mixing it with a porous, inert material, was motivated by purely academic interests or whether he weighed the possibilities of destruction which his discovery would place in man's hands against the undreamed of feats of constructive engineering that it held out. The five prizes for which he left his fortune are some measure of his concern, and it is not a reflection upon him or his profession that on more than one occasion there has been no candidate considered worthy of the peace prize. Again one's thoughts turn naturally to the Curies whose researches form a link in the chain that led to the production of the atomic bomb. We can never forget that when the significance of their recovery of radium from pitchblende was recognized they unhesitatingly dismissed the thought of patenting the process, considering it contrary to the scientific spirit to withhold for personal profit a discovery which would be of untold benefit to mankind. In recalling the character of great scientists, I do not mean, however, to employ the fallacious method of supporting a thesis by individual cases. We know that, as among all men so among scientists, there are those who are driven by selfish ambition as well as those whose devotion to high purpose is worthy of a saint. But if we are to think clearly on this question we must not confuse the spirit of inquiry, which is inseparable from the sciences, with the foibles displayed by individual scientists, not because they are scientists but because they are human. The common misconception of the man of research as characteristically preoccupied with the selfish satisfaction of his curiosity can be corrected by a review of the history of science in the light of its interplay with industrial development, systems of philosophy, and social, economic, and political theory. Perhaps, however, the most immediate and effective answer to those who regard the scientist as indifferent to the consequences of his discoveries is afforded by the stand which scientists are today taking on the use of the atomic bomb.

The oath which Miss Weltfish proposes is an appeal to idealism, to the desire of the individual to live as a useful, integral part of common humanity. But an ideal which cannot find expression in action is only the good intention that paves the way to hell. The question is, how would the oath affect the activities of scientists, and what security, if any, would it give us? Suppose the men who worked on the atomic bomb had taken it; how could they, knowing the threat of German development of such a weapon of destruction, have refused their government's call? While hazards exist, is it wrong to provide for self defense? If not, then the possibility of the offensive use of the same instruments confronts us. An even more serious objection is that the oath is based upon the naive assumption that research can be separated into two classes, beneficial and harmful. Nobel's dynamite affords a simple illustration of the fact that a discovery may be used either constructively or destructively depending not upon the intent of the scientist but upon the moral status of mankind. It is indeed curious that the scientist should be singled out as a scapegoat upon whose head the sins of society are placed.

Discoveries of fundamental theoretical importance often have the most revolutionary practical applications; consequently, every scientist well knows that if we were to subtract from our knowledge everything which can be put to harmful use, the tattered remnant would hardly be recognizable. Moreover, it is seldom if ever possible to foresee the full results of a discovery. James Watt was an instrument maker at Glasgow College when the task of repairing a defective model of a Newcomen engine used for class demonstrations caused him to note its large consumption of steam. Even though he was thinking of industrial applications when he bent his ingenuity to its improvement, it would hardly be reasonable to demand that he foresee the multitude of problems arising from the social revolution to which it contributed so much. Faraday's interest in

the relationship of electricity and magnetism was more theoretical. After the effect of an electric current on a magnet was observed, the question of the reverse action was on his mind and the subject of experiment for seven years before his study of effect previously neglected enabled him to formulate the laws of electromagnetic induction in ten days. Would Miss Weltfish becloud our appreciation of Faraday's genius by suggesting that, in as much as he did not anticipate all the consequences of the use of the electric motor, he was not giving due consideration to "the purposes for which his investigation was being carried out. . . ."?

Whether the scientist is concentrating his energies upon the solution of an industrial problem or trying to gain a clearer understanding of natural phenomena, his discoveries may sooner or later call for social adjustment. It is not within his power to decide whether that adjustment will be wise and progressive or unthinking and disastrous. Consequently, discoveries in the natural sciences demand compensating advances in the social sciences, in politics, economics, and morals. If we fail to make those advances, it is a sorry expedient to tell the scientist that he must be a good boy and not discover anything we can misuse.

Aside from being inpracticable and misdirected, a movement for a scientist's oath might well have the effect of hindering the accomplishment of its ultimate purpose in so far as it absorbs effort and diverts attention from the problem of understanding the causes of social and international maladjustments. One is reminded of Bishop Ripon. When in 1927 he pointed out the fearful consequences of the lack of balance in our culture, he put his finger on a weakness that has worried many, but his proffered solution, a moratorium on science, suggests a teacher demanding that his capable pupils stop studying in order to let the backward ones catch up. Our pressing need is to accelerate progress in the social sciences. We plead complexity of subject matter as the cause for their retardation relative to the physical sciences; but it is not a foregone conclusion that this is the sole cause. Nor would anyone argue that the rapid advance of the physical sciences is due to simplicity of subject matter. The significant circumstance is that our material environment affords a realm where it has been easier for man to detach himself from his likes and dislikes, his hopes and fears. It is not an accident that in great scientists we find a recurring pattern of traits with thirst for understanding, the highest degree of mental integrity, humility, and ingenuity conspicuous. These traits are not peculiar to the scientist, far from it, but he is distinguished by the degree to which they are developed. He gives us a new realization of what mental integrity means. Both experiment and mathematical computation make him keenly aware of the possibility of error in observation and conclusion, of the insufficiency of the usual accumulation of data, of the risk of neglecting some of the many factors influencing his results, and of the danger of being blinded by preconceived ideas. Hence his humility. But he also has confidence and determination because he knows from experience how far ingenuity—imagination and originality, if you will,—can take him. Miss Weltfish is perhaps correct when she says that it is little understood that science is an instrument for seeking the truth, and we might add, the quality and effectiveness of that instrument is not appreciated outside the sciences.

It is not easy to study man with the same detachment with which the physicist studies matter because as soon as we consider human relations we enter the "field of force" of ideas about what is desirable. The recognition of this fact should steel our determination to maintain the scientific spirit in all research. Throughout time men have fought savage and disastrous battles over ideas; ideas advanced more often than not with talk of betterment. But the earnest desire to understand, eagerness to extend knowledge, to put it to test, the honesty to admit error, the courage to recognize insufficiency of facts and experience, these are peacemakers and they are of the essence of science.

At this particular moment in history we are faced with the question of how to develop mutual understanding in a world made physically one before we were psychologically prepared for it. The anthropologist made a significant contribution when he demonstrated the closeness of biological relationship of all races of man, although blood relationship alone does not assure harmony of life. The similarities of man's cultural aptitudes are even more encouraging, and when the mind runs over man's activities, the sciences stand out as particularly adapted to foster the spirit of internationalism. Our body of scientific knowledge is a monument to mankind which is entirely free from national or racial flavor. If we are not keenly aware of this and tend to think with particular pride of the contribution of our fellow countrymen, it is the fault of our education. By contrast, linguistic differences set men apart, increase the difficulty of understanding. The arts leap the bounds of language and constitute a heritage which enriches all mankind, but they also often draw inspiration from and express nationalistic feelings; we can identify the artistic styles of different peoples, and independent creative genius looms large. Certain ethical principles are shared by great religious systems, but we must contend with intolerance which has often made it impossible for people of different religions to work together for the attainment of their ideals. The sciences on the other hand open man's

mind to an intellectual comradeship of the ages that knows no bounds. They have grown as a single interrelated and interdependent whole. Everyone must thrill to the realization of how ideas and obsrvations of men of far countries have repeatedly interlocked and stimulated, leading to ever broader and truer understanding. We can pick up the thread anywhere and follow its intricate weave. Take current electricity, for example. There was the Italian Galvani noting that a frog's leg contracted under the effect of a discharge from an electrostatic machine and that the same reaction occurred when a nerve and muscle were connected by different metals. His countryman Volta demonstrated that the essential phenomenon did not depend on animal substance and built a voltaic pile, producing electric current, which was quickly used in studies of the chemical effects of current by Englishmen, including Humphry Davy and, finally, Faraday. Then Oersted in Copenhagen noted the effects of current on a magnet, the importance of which was immediately appreciated by a Frenchman, Ampère, who studied the laws of the forces acting between currents. The Germans, Ohm, Gauss, Weber, speeded progress by exact definitions of quantities and invention of a system of units for electrical measurement. Faraday was aided in his investigation of induction by the improved electromagnet designed by the American Joseph Henry; and so on in an unending stream. Those were days of limited communication. As we approach our own times, reactions quicken, and the circle widens to include the world. One name, Noguchi, should suffice to remind us that we owe our debt of gratitude and respect to scientists of all races and all nations.

Contemplating this aspect of science, we seem to be faced by a paradox because in this age of outstanding scientific achievement the sense of nationalism is quickened and racial prejudice flares. Ours, however, can be called a scientific age only if we measure it by the achievement of the scientist himself and by our dependence upon him for our material comforts and pleasures. One need only reflect upon our approach to pressing domestic and foreign problems to realize how far we are from having attained what might properly be called a scientific state. One of the tragedies of this time of cataclysmic change is the extreme difficulty of learning the truth about what is going on in the world. We are condemned to struggle almost hopelessly with a deluge of opinion and partial information highly colored by prejudice, fear, ambition, and even good intention. Suppose foreign diplomats or leaders of labor and industry could meet with the same mental integrity that the scientist must exercise in his research, how different our world would be! Madame Curie once said, "Nothing in life is to be feared. It is only to be understood." It would do no good to extract an oath of mental integrity from those entrusted with the direction of our affairs. We must begin farther back and learn to understand what Faraday meant by the simple maxim "Keep thyself teachable." In other words, we face a tremendous educational problem, and I believe the brunt of responsibility should be borne by the social scientist. Therefore, as Miss Weltfish's fellow professional, I would plead with her that it is not for us to accuse the scientist; rather we should learn to understand the spirit of his research in order that we may fulfill better our duty to society.—ANNA O. SHEPARD.

Ethical Dualism

Dr. Weltfish is to be commended for her paper entitled "Science and Prejudice," and for her good intentions expressed in it. However, it appears to me that in her efforts to bar the "ivory tower escape" for the scientist, Dr. Weltfish is entirely unaware of what I shall term the "ethical dualism" of matter.

To explain my statement by way of a specific example let us consider two chemists. One works on explosives such as TNT, and the other on better aviation fuels. If the products of their efforts are used in war, to fill shells or to power bombers, the chemists have violated their pledge. If on the other hand the explosive was used in quarries and mines, and the fuel for air transport, travel, and exploration, they have worked toward the good of humanity. Many similar cases could be cited.

In my opinion, Miss Weltfish does not take into account that the scientist is not at the dispensing end, as, e.g., the physician, but that he is a creator of matter, of processes, of machines which per se have no ethical values. Those who control their application and use bring in the ethical problem and can turn them into benefit or destruction of humanity.—FREDERICK C. NACHOD.

Prejudice against Prejudice

A Weltfishian oath for scientists! Why would it be that I could mistrust the integrity of the scientist who took this oath, provided he took it with conviction and vehemence and not merely in the impetuosity of an emotional moment? Even the most ivory-towered of scientists must want "the good of humanity," must rue "the destructive forces of the world." Perhaps the difficulty comes in the oath's condemnation of "the ruthless intent of men." Doesn't Dr. Weltfish want us to be ruthless against what is sometimes called "intolerance"? Isn't she fomenting ruthlessness against ruthlessness, hoping to get us to swear to this paradox?

It is not as if I had not puzzled and written about this problem of scientific prejudice for the last

twenty years. I have. But there is an essential contradiction involved, a contradiction inherent in human nature. Even Dr. Weltfish's article illustrates it. She complains about how Quatrefrages was bombed out of his ivory tower and started up some social harm while he was out, and then about how modern scientists too often withdraw into the tower, leaving outside enough particular responsibilities to do a lot of social harm. It is not a question of being in the tower or out of it. You can do both good and harm in both places. Dr. Weltfish has her eye on the harms.

The basic dilemma is here, I am sure, the dilemma between drive and prejudice. It is thus the dilemma of ego-involvement. Science has its equivalent of the Hippocratic oath, although the formula has never been set down. The scientist should follow through to the truth, should read the book of nature accurately no matter how unpleasant the reading may be. Such an oath he might take, paradoxical as it is, for it is an oath to be prejudiced against prejudice. He might take it, but, because of the paradox in it, he could never wholly fulfill it. To fight prejudice takes a prejudice against prejudice. To do research you need an hypothesis to test, and the man who holds his hypothesis so lightly that its disproof is just as pleasant to him as its verification is not the man who works long hours for little pay and limited social recognition. Enthusiasm is prejudice. That's the way human motivation is constituted.

The successful scientist has really got to be a schizophrene. He has got to get all excited about an hypothesis that he thinks might be true (and that means he hopes it is true) and then be ready to celebrate because he finds it is false. No wonder he goes wrong. No wonder we keep meeting silly scientific controversies whose chief purpose seems to be to prove one or two protagonists right. No wonder the scientific truth has so often to transcend the individual, waiting upon his professional posterity, because the individual cannot himself transcend his own pride and ego-involvement.

The conventional way of meeting this problem is to provide the ethic of judicious disinterestedness. The scientist constrains his public behavior in accordance with the ethic, keeping his private hopes and prejudices to himself and to his friends—except, of course, when his reasons turn out to be rationalizations and his cat is out of his bag. That's what happened to Quatrefrages.

On top of this perpetual dilemma Dr. Weltfish wishes to impose another prejudice—another prejudgment. She wants the scientist to decide, not only whether his own egoism is filtered out of his publication, but also whether his finding is good for society. How is he to know what is good without knowing what the results of discovery will be? I do not speak of the obvious. Of course, the psychologist who has finished testing a schoolchild in the interests of impersonal truth does not murder the child and throw him away. He gives him a cracker, pats his head, and sends him home. The cracker is not science but what decent people do. But what about splitting atoms? Will that do more harm than good? Suppose those physicists had taken a vow against "the destructive forces of the world"? But perhaps it is not fair to put that difficult question up to Dr. Weltfish.

Here, however, are some simpler questions. Should psychologists and anthropologists continue to investigate the nature of psychological defense and escape mechanisms that the psychoanalysts have given us? Should they publish their results? Should they disseminate them widely, trying to get ordinary people to think scientifically about human nature? It is argued that such activities do social harm because they tend to induce in men the habit of thinking about their behavior as the consequence of external causes and past events. That, it is held, makes them irresponsible. Read R. B. Perry's *Puritanism and Democracy* and then decide whether anthropology and psychology with their support of the un-Puritanic irresponsibility of psychic determinism are or are not adding to "the destructive forces of the world."

In short, I feel that the aggressively socially minded psychologists are just one kind of psychologist, a bold kind, who venture to prejudge what in science will be good and what bad, who probably seldom suppress the truth for humanity's sake, but who probably win the battle against their own ego less often than their colleagues who feel less intensely the problem of making science come out the right way. And what goes for the psychologists goes for the anthropologists and the other scientists too.—EDWIN G. BORING.

The Last Word

In connection with Professor Boring's criticism of the scientific oath I propose in the September MONTHLY I should like to make some general remarks and then be specific to his point:

In the course of many discussions, criticisms of the oath have been of two main sorts: *one,* that it should treat of the integrity of the scientist in seeking the truth, and *second,* that it will not accomplish the purpose that it is apparently designed to achieve.

With reference to the first objection, I want to point out that in the body of the article "Science and Prejudice," I have said that for the most part the scientist has done an excellent job along these lines. I do not feel, therefore, that an oath for this purpose is now needed.

The whole object of my proposal is that there is another aspect of the scientist's experience that re-

ceives from him either very meager consideration, or none at all; viz., the purposes for which his work is carried on, and his place in society and his obligations to it. It is to the latter emphasis that the oath is addressed.

On the second question as to whether the oath will accomplish its purpose:

It is true that the fact that a scientist has taken this oath is no guarantee that he will automatically acquire a social consciousness, and that, further, this social consciousness will take a "good" direction. It is, in fact, quite possible that a scientist might go on doing precisely what he has done before, even after taking the oath.

I think there is a rather general human desire to get the world's work done by magical formulas. We share this desire with the New Guinea native and with many other human beings who, by reciting a spell, would call down the aid of the supernatural powers to alter the whole course of their lives. We like to think that by finding a four-leaf clover we can change the whole atmosphere of the universe so that it is especially shaped to the desires and needs of one person; viz., ourselves.

On a reality level, one could not possibly devise an oath that would completely change the "moral atmosphere" of a scientist.

My particular purpose in proposing this oath is twofold: to stimulate the young science graduate to consider his place in society and to get the people themselves constantly to consider and reconsider the place of the scientist in society. In other words, I do not expect the scientist to gain a social conscience by a single flash of insight nor by lonely introspection, but with the collaboration of the people of whom he is a part. To me this sort of procedure is the essence of democracy. The fear of the scientist that has arisen in the public as a result of the atomic bomb is a rather discouraging symptom of the aloofness of the scientist.

Professor Boring says that he would be inclined to distrust the scientist who sincerely took such an oath. I think he is going too far in the opposite direction, in the same sense as those who expect the recitation of an oath to have immediate and universal positive effects.

Professor Boring's reasoning on a psychological level is extremely interesting. He feels that it is a common human tendency to "put your money" emotionally on one or another side of a question, and that for your side to win is an ego-satisfaction while the opposite is a threat or source of displeasure. He pictures the quest for objectivity on the part of the scientist as an inhibition of this satisfaction because he must act as if he didn't care whether his results went with or against his hypothesis. My teacher, Franz Boas, was never through warning us that a negative result was as valuable scientifically as a positive one. He repeated it many times in the hope that we would follow it unfailingly. There is no doubt that it is a hard thing to do.

Now Professor Boring maintains that this is enough of an emotional strain on the scientist without demanding of him that he also examine the social implications of his work. On the other hand, I believe that the process of understanding the social milieu in which he lives and of considering the place of his work within it will mitigate his anxiety at negative results rather than augmenting it. The value of his results, positive or negative, will be much clearer to him than if he considers them in introspective isolation.

Also, I would not demand of the scientist that he try to predict all the results of his scientific discoveries before he made them known. This is something he certainly could not do. What I would ask of the scientist is that he know and understand his society well enough to have some gauge of what his work means in it. In other words, it would be of great value to his work for the scientist to be a citizen in the full sense of the word. (See my article "The Scientist is a Citizen," *Christian Register,* September, 1944, vol. 123, no. 9; pp. 325–7.)

At the risk of being anticlimactic, I want to add that there are those who would defend the social isolation of the scientist on the grounds that he doesn't have time to gain information about the social scene—that he is necessarily too preoccupied with his work. This concept of the scientist as incessantly preoccupied does not come entirely from the exigencies of his work. Projective thinking does seem to demand irregular hours, times of long and intensive preoccupation, and times of inactivity. But this does not mean that in the periods of inactivity other matters cannot be considered.

Because of this irregularity in the working time of the scientist, science has been conceived as a kind of random procedure awaiting only inspiration—which it is not. The procedures of science are certainly orderly and predictable. The scientist is creative precisely because he follows these time-honored procedures. And even the problems that appeal and suggest themselves to the scientist are not random, but have a continuity in the history of science and are a response on the part of the scientist to contemporary social stimuli.

In suggesting that the scientist give some of his attention to this social scene, I am suggesting that in common with many fields of activity in modern civilization, specialization has proceeded too far and that it is desirable to seek a greater degree of integration. Professor Black, also in the September issue of the MONTHLY, writes to this point in his

article "A Lend-Lease Program for Philosophy and Science" in which he points out that historically science was not as limited to technical procedure as it is today and indicates that philosophy be reintegrated with science. I am, of course, fully in accord with this concept—and I would advocate this course of action as well as the one I am suggesting.

In these times of transition, the scientist must outgrow his role of alchemist, astrologer, and esoteric medicine man and become a worker among the people—albeit a highly skilled worker with his brain.—GENE WELTFISH.

Beachcombing

The article "Scientific Beachcombing" in the October SM represents the opinions of the author but does not give the cause, why the Santa Barbara shoreline is scalloped. Enclosed are four photographs of water being poured from a widemouthed graduate. Surface tension causes the drawing together of the stream, as shown by the reduced cross section. In the same manner the backwash from the wave front draws together and breaks up into scallops. The increased erosion along the center of the scallops leaves the fringe of points. This fringe is an effect of which surface tension is the cause.—HIRAM W. HIXON.

Dr. Evans' short article "Scientific Beachcombing" in the October issue is interesting in view of the fact that so little scientific study of beach forms is available. My late brother, William F. Jones, at one time Associate Professor of Geology at M.I.T., was keenly interested in beach formation as a practical matter in connection with the control of erosion. He did an enormous amount of work in the detail survey of beaches, both stable and unstable. At one time he sent me a set of contours of stable beaches. These proved to be parabolas of one family, the beach crest being under the focus. So far as I could make out from the available data, the particular parabola at any locality depended on the average size of the sand (or gravel) particle, the distance between mean high and mean low water, and on the average size of the breakers.

This conclusion is largely speculation. The shape of the contours is not speculation. Possibly Dr. Evans may be interested enough to either verify or disprove the above. If correct, it has an important bearing on the use of artificially stabilized beaches in coast protection, a result that has not been attained by the use of groins, jetties, bulkheads, etc.—methods that futilely resist rather than dissipate the energy of the waves, and seek to prevent natural long-shore sand movement.—BASSETT JONES.

Melly Clismas

So Association want big building? Well, easy if big shot give big money. Big atom bomb make big noise and stop big war and big shot got big money to make building. Government also got big money, 3 million carloads gold in big cave in Kentucky. Let government give big building, so cost nobody anything. Let government do everything so everybody have no trouble and pocketful of money.

What for big building anyway? Diogenes great scientist. He live in tub. Confucius big shot thinker have no big building. Mohammed sit on big stone all alone in big desert. Robinson Crusoe sit on island. Jack Horner sit in corner. Big men have no big building, but have big brains. Let somebody else do work and give money make big building. Scientists have big brains, save money, smoke big pipe, take it easy, don't hurry and don't worry. Allee same in million years. Keep kimono on and take it easy.—OLD HI HO.

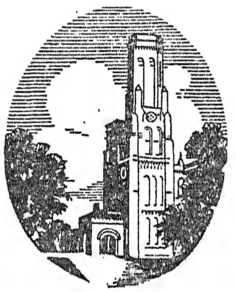
Our honorable and venerable Hi Ho is great apostle of optimism. He preach gospel of wishful thinking. Wish hard and wish come true. Wish very, very hard for beautiful stone monument building home of A.A.A.S., and, lo and behold, money come flying. Yes? No!! Honorable members A.A.A.S. trained science thinkers; Ho Hum hope have outgrown belief in fairies, Santa Claus, and miracles. Science make miracles like split atom, but Science not wish for them. Science hard work. Hard work means sweat. Sweat great fertilizer all growth. Sweat very, very honorable. Great man Churchill dignify sweat even for British faces: "Blood, sweat and tears."

No argue that new building not needed. But no like wishing. Building belong members, serve members. Members appreciate if all pay alike. No pay, no sweat, no appreciate. All pay same if money come from bigger dues. Science now big. Science now have plenty face. People afraid and no trust science men. Afraid make tools to destroy. Peoples forget tools no destroy, but fools who misuse tools do. Science can do plenty good. To lead need keep face. Science lose face, which very sad, if go begging. Science must know people. That my business.—HO HUM.

Belated Acknowledgment

I would like to express gratitude to Clarence and Elizabeth Ferry of Westwood Village, Los Angeles, for their excellent photographs of artifacts which appeared in my article, "An Anthropological Campaign on Amchitka," in the July 1945 issue, acknowledgment of which was omitted through an error.—PAUL GUGGENHEIM.

THE BROWNSTONE TOWER



ALONE in the Brownstone Tower, I am thinking tonight of my father-in-entomology, Dr. William Moore, who not only introduced me to the Japanese beetle in 1921 but taught me something about natural history in the fields and woods near Riverton, N. J. He was, and is, a robust lover of life. Having a lively imagination and an unrestrained sense of humor, he not only saw the fun in every odd incident but treasured every personal anecdote and, like Mark Twain, embroidered each one with riotous exaggeration till, after a few tellings, the truth became totally eclipsed by fancy.

The story I remember best, of course, concerns myself. The simple truth was this: Needing a supply of tent caterpillar egg masses for experimental work in early spring, Dr. Moore took me, his chemist, out in the field and taught me to recognize on dormant wild cherry trees the conspicuous ringlike bands of eggs laid on twigs by moths the preceding summer. Then he made it clear that he wanted me to climb such trees and procure twigs bearing these varnished egg masses. I viewed this assignment with misgiving because my center of gravity was abnormally low and I had previously flunked my gymnasium examination on chinning. But duty called and, with Dr. Moore looking on, I somehow managed to ascend to the first crotch of the tree. But to reach the twigs I had to get out on a limb. I tried and soon found myself out on a limb in the usual sense of that well-worn phrase. To put it plainly, I was stuck and had to give up my attempt to secure the twigs.

Dr. Moore has told that story off and on ever since, until today it goes something like this: "One warm day in early spring at the Japanese Beetle Laboratory Campbell concluded his lunch with a generous slice of lemon pie. Then we went out to get some tent caterpillar egg masses. I found a good tree and asked Campbell to climb it to get the eggs. He huffed and puffed his way up into the tree and crawled out on a limb. All of a sudden the lemon pie lurched and threw him off balance. Down he swung until he was hanging from the limb like a sloth. He couldn't get up and he couldn't get down. Finally, in order to let him go home, I had to cut the tree down!"

When, fresh from Philadelphia and the University of Pennsylvania, I went to work with Dr. Moore I thought there were only two species of birds in the vicinity: English sparrows and robins. He opened my eyes to the birds of the Riverton golf course and a little later at Rutgers my friend and contributor to THE SCIENTIFIC MONTHLY, Dr. L. A. Hausman, increased my ornithological knowledge, which, however, is still elementary and fragmentary.

Although my birding days are over, my most memorable encounter with a bird occurred only a few weeks ago here in the Brownstone Tower. It was a hot, dark night, and my circular window was wide open and hence horizontal. Perhaps I needed to relax from my work for, without having heard a sound, I raised my head and turned to look at the open window. There, perching on its outer periphery and looking solemnly at me, was an owl—a large white owl, it seemed to me. I did not invite him to come in. I felt that my cubicle was too small for an owl and an editor; that one might get in the other's hair or feathers, as the case might be. So I outstared him, and he turned and flew silently away. Later, with the help of A. A. Potter and Herbert Friedmann, I learned that my feathered friend was a barn owl and that he was a family man. I saw his family flying at dusk and heard them chattering. They undoubtedly lived in one of the smaller vacant towers of the Smithsonian. For further information I turned to Dr. Hausman's *The Owls of New Jersey* and read: "[Its] eerie sort of flight and the habit of nesting in deserted buildings, old towers and steeples, and ghostly ruins have resulted in the bird's being taken more than once for the 'haunt' or spirit of such localities, and more especially since one of the common flight notes is a weird, subdued hissing scream." Then I knew that I had seen the ghost of the Smithsonian and that, if I did not take care, my body would be found one night in *rigor mortis*.

The foregoing owl story is the truth and nothing but the truth. I should like to know what Dr. Moore would make of it. I do know how it has been exaggerated and distorted by that unprincipled poetical prevaricator Paul H. Oehser, whom I introduced to our readers last month. Paul's version of the story follows:

The Owls of the Brownstone Tower

The owl, from ornithologists I've heard,
Is commonly a solitary bird;
It flits around at night *to-whit-to-wooing*
And never cares what other birds are doing.
That's usual. But every now and then
The owl seeks out the company of men,
Consorts with scientists in lofty towers,
As if to emulate their human powers,
Follows them up into their musty attics
Absorbing wisdom, wit, or mathematics.

A story known by every Washingtonian
Concerns the owls that live in the Smithsonian:
One night a Dr. C. at a late hour
Climbed to his office in the Brownstone Tower,
And soon, within his cozy foursquare crypt,
Was lost in scientific manuscript;
When lo! exactly fifteen minutes later
A pair of owls got off the elevator.
In somber style they marched into the room
As serious as a little bride and groom.
"Excuse," spoke one of them, "our innate shyness,
But we would ask a favor of your Highness."
"Shoot!" said the Doctor to the spokesman fowl,
"I'm not a man who would turn down an owl."

Seeing the Doctor's heart so promptly thawing,
The birds proceeded without hem or hawing:
"It's this," they said, "we've tried by every media,
But we can't lift your darned encyclopedia;
Last night our brother nearly killed himself
Trying to get that atlas off the shelf;
And *you* should know how very necessary
Is access to a Webster's Dictionary.
We won't drop pellets in your chair no more
If you will leave them books out on the floor."
The Doctor could but cry, "Well, I'll be blessed!"
And with a smile he quickly acquiesced.

And that is why those owls have grown so wise
Up in their towering Brownstone Paradise;
They live and die and raise their fluffy broods
Immune from usual vicissitudes;
Envied by all the other birds of prey,
They read all night and sleep throughout the day,
And quite emboldened by their self-reliance
Nest in the very periwig of science.

—Paul H. Oehser

In January of this year, when The Brownstone Tower first appeared in the SM, I mentioned those members of the A.A.A.S. staff who have something to do with The Scientific Monthly. Recently some changes have occurred. My assistant, Mrs. Scovill, recovered from an obscure infection or allergy shortly after Captain Scovill returned from Germany. She left the hospital to join him at Fort Leavenworth, Kansas. She was a loyal, able, and cheerful worker who did much for the SM during her year in the Tower. Her place has been taken by Gladys M. Keener, of Chicago, a woman of experience in editorial work on a national periodical.

We shall also greatly miss Sam Woodley, who performed many services for the SM, including proofreading during my illness. For more than 25 years Sam worked like a superman for the A.A.A.S. I wish that every member of the Association could thank him personally for his devoted services and wish him success and happiness in the less exacting task he has undertaken in private business. Sam's place will be taken in part by Dr. John M. Hutzel, a graduate of the University of Michigan and Ohio State University. Dr. Hutzel, a Lt.j.g., now wears the uniform of the Fourth Marines. Responsible for insect control during the invasion of Iwo Jima, he directed the airplane spraying of the island with DDT. At present he is the Navy's entomologist in Washington and is writing a manuscript for the SM on control of malarial mosquitoes. Upon his release from the Navy, which is pending, he will begin to "get things done" for the Association.

Our advertising manager and book review editor, T. J. Christensen, and his assistant have left the dungeon and moved to temporary quarters on the campus of American University, Massachusetts and Nebraska Avenues, Washington 16, D. C. There Mr. Christensen, who is also advertising manager for *Science,* will work with the new editor of *Science,* Dr. W. L. Valentine, and his staff.

The end of the year approaches with much undone. Many readers have been good enough to write to us about articles that have appeared in the SM, or have expressed their opinions on other matters. Owing to Mrs. Scovill's long illness it was not possible to acknowledge all these letters. Perhaps it would be best to try to discharge these obligations here by thanking those who have written to us and with the New Year make a fresh start on current correspondence. To those who have received Christmas cards from me in the easier days before the war and to my former students I take advantage of The Brownstone Tower to say, "I have not forgotten."—F. L. Campbell.

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